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Living in Complexity: Institutional Arrangements, Disciplinary Identities, and Epistemic Practices at the Complexity Science Hub (CSH)

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Abstract in German

Obwohl es Complexity Science seit rund vier Jahrzehnten als wissenschaftlichen Begriff gibt, hat sie sich nie wirklich zu einer eigenständigen Disziplin entwickelt und wird von Wissenschaftlern stattdessen entweder als ‘Quasi-Disziplin’ oder als ‘wissenschaftliche Plattform’ bezeichnet. Einerseits verbindet Complexity Science eine ‘antireductionist’ Haltung mit der formalen Anerkennung von Unsicherheit – wobei sie die Bedeutung der ‘entstehenden’ Eigenschaften von Systemen gegenüber den einzelnen Elementen betont, aus denen sie bestehen. Andererseits setzt sie auf ein datenintensives und rein quantitatives methodisches Instrumentarium, das sich hauptsächlich auf Mathematik, Physik und Informatik stützt. Angesichts dieser methodischen Enge wird Complexity Science oft als inter- und transdisziplinäres Forschungsgebiet beschrieben, und tatsächlich gelingt es ihr, Forscher mit sehr unterschiedlichen Hintergründen unter ihrem Banner zu versammeln. Dies wird zum großen Teil auch durch die semantische Offenheit des Begriffs ‘Komplexität’ ermöglicht, wodurch Complexity Science ein breites Spektrum an Forschungsschwerpunkten abdecken kann: von der Wirtschaftswissenschaft bis zur Cybersicherheit, von der Stadtforschung bis zur Verhaltenspsychologie. Seit seinen Anfängen stellte dieses Fachgebiet eine klare Herausforderung für die akademische Tradition dar und wird zunehmend mit der Erarbeitung gesellschaftlich relevanter Erkenntnisse in Verbindung gebracht und als möglicher Lösungsansatz für die vielen Komplexitäten der heutigen Gesellschaften gefeiert. Als eigenwilliges Beispiel für zeitgenössische disziplinäre Umstrukturierungen zeigt Complexity Science, dass Bemühungen zur Verbesserung der Wissenschaft das chaotische und instabile Ergebnis gegensätzlicher und oft widersprüchlicher Kräfte sind. Aus Sicht der Wissenschafts- und Technikforschung ist es daher von entscheidender Bedeutung, darauf zu achten, wie Wissenschaftler sowohl kollektiv als auch individuell mit diesen Kräften umgehen.

Zu diesem Zweck habe ich empirische Forschung am Complexity Science Hub (CSH) durchgeführt, einem Forschungszentrum für Complexity Science mit Sitz in Wien. Ich stützte mich dabei auf Dokumentenanalysen, Interviews und teilnehmende Beobachtung und konzentrierte meine Forschungsinteressen auf die Wahrnehmungen, narrativen Rekonstruktionen und Bewertungen der dort tätigen Wissenschaftler. Meine Forschungsfragen befassten sich mit drei unterschiedlichen, aber miteinander verbundenen Aspekten: 1) der institutionellen Konfiguration des CSH, 2) seiner interdisziplinären Praxis und 3) der Artikulation der gesellschaftlichen Relevanz der Complexity Science. Was den ersten Punkt betrifft, so ergab sich aus meiner Forschung, dass die institutionelle Flexibilität der Complexity Science sowohl eine Herausforderung als auch eine Chance darstellt. Zwar haben Forscher Schwierigkeiten, sich in der Complexity Science zugehörig oder sicher zu fühlen, doch schätzen sie die CSH auch als

alternativen Raum zu traditionellen Disziplinen und der akademischen Welt im Allgemeinen. Zweitens zeigt meine Arbeit, dass die Integration von Wissen in der Complexity Science mit Asymmetrien behaftet ist. Dies beruht hauptsächlich auf der epistemischen Autorität quantitativer Ansätze und auf den übergreifenden akademischen Bewertungssystemen, die einen ständigen Publikationsdruck auf Wissenschaftler ausüben. Während Ersteres Complexity Science zugunsten einiger weniger dominanter Disziplinen verzerrt, wird Letzteres selbst von den wohlmeinendsten Wissenschaftlern als unüberwindbares Hindernis für die sorgfältige Praxis der Interdisziplinarität empfunden. Schließlich stellte ich fest, dass die Wissenschaftler am CSH die gesellschaftliche Relevanz ihrer Arbeit durch die Mobilisierung einer Vielzahl von Idealtypen und Vorstellungen von gesellschaftlichen Interventionen zum Ausdruck brachten, aber auch auf Herausforderungen und Diskrepanzen zwischen den Erwartungen an die Interaktion zwischen Wissenschaft und Gesellschaft und deren Realität stießen. Insgesamt leistet diese Arbeit einen Beitrag zur STS-Forschung der Uneinigkeit und Situiertheit der Wissenschaft und bietet eine strukturierte Fallstudie, anhand derer verfolgt werden kann, wie Veränderungen in der Beziehung zwischen Wissenschaft und Gesellschaft die wissenschaftliche Praxis vor Ort verändern. Darüber hinaus liefert sie mehrere Beispiele für die Herausforderungen und Spannungen, die interdisziplinäre und transdisziplinäre Kooperationen beeinflussen können, und analysiert die Rolle quantitativer und datenintensiver Methoden sowie institutioneller und struktureller Elemente bei der Neuordnung der Disziplinen in der zeitgenössischen Wissenschaft.

Abstract in English

Despite having existed as a scientific label for roughly four decades, complexity science never quite institutionalized into a discipline and has instead been described by scholars alternatively as a 'quasi-discipline' or as a 'scientific platform'. On one hand, complexity science combines an antireductionist stance with the formal acknowledgement of uncertainty – stressing the importance of 'emerging' properties of systems over the individual elements that compose them. On the other hand, it deploys a data-intensive and purely quantitative methodological toolbox – mostly informed by mathematics, physics and computer science. In the face of its methodological narrowness, complexity science is often described as an inter- and transdisciplinary field of study, and it indeed manages to gather under its banner researchers with very disparate backgrounds. In large part, this is also enabled by the semantic openness of the term 'complexity', which allows complexity science to encompass a broad array of research foci: from economics to cybersecurity, from urban studies to behavioural psychology. Since its early days, the field explicitly embodied a challenge to academic tradition and, increasingly, it is associated with the production of societally relevant knowledge and celebrated as a possible resolutive approach to the many complexities of contemporary societies. As an idiosyncratic example of contemporary disciplinary rearrangements, complexity science shows that efforts to improve science are the messy and unstable result of contrasting, and often contradicting, forces. From an STS perspective, it is therefore crucial to attend to how scientists navigate these forces, both collectively and individually.

To this end, I carried out empirical research at the Complexity Science Hub (CSH), a complexity science research center based in Vienna. I relied on document analysis, interviews and participant observation, and specified my research interests to the perceptions, narrative reconstructions and valuations of scientists working there. My research questions addressed three distinct but interrelated aspects: 1) the institutional configuration of the CSH, 2) its interdisciplinary practice, and 3) the articulation of the societal relevance of complexity science. Concerning the first point, the institutional flexibility of complexity science emerged as both a challenge and an opportunity from my research. Indeed, while researchers struggle to feel a sense of belonging or security in complexity science, they also value the CSH as an alternative to traditional disciplines and academia writ large. Second, my work shows that the integration of knowledge in complexity science is fraught with asymmetries. This is mainly predicated on the epistemic authority of quantitative approaches and on the overarching academic regimes of valuation that exert constant publication pressure on scientists. While the first biases the complexity science landscape towards a few dominant disciplines, the latter is experienced even

by the most well-intentioned scientists as an insurmountable bottleneck for the careful practice of interdisciplinarity. Finally, I found the scientists at the CSH to articulate the societal relevance of their work through the mobilization of a variety of ideal types and imaginations of societal interventions, but to also encounter challenges and mismatches between expectations of science and society interactions and their reality. Overall, this thesis contributes to the STS investigation of the 'disunity' and situatedness of science and offers a textured case study to follow how shifts in the relation between science and society transform scientific practice on the ground. Moreover, it also provides several examples of the challenges and tensions that can affect interdisciplinary and transdisciplinary engagements, analyzing the role of quantitative and data-intensive methods and of institutional and structural elements in the disciplinary rearrangements of contemporary science.

Acknowledgments

Writing this thesis was a long and rewarding process. While it was at times challenging – and overall requiring of a good dose of stamina – I was lucky enough to always be able to count on solid support. In fact, many of the following pages would be different or non-existent was it not for all the pushes and influences that informed, encouraged, assisted and inspired my research. This is my attempt to do some justice to that.

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Contents

List of Acronyms and Abbreviations

AI	Artificial Intelligence
AIT	Austrian Institute of Technology
AMIF	Asylum, Migration and Integration Fund
ANT	Actor Network Theory
ASCII	Austrian Supply Chain Intelligence Institute
CEU	Central European University
CSH	Complexity Science Hub
ERC	European Research Council
ESF	European Social Fund
EU	European Union
FFG	Austrian Research Promotion Agency - Österreichische Forschungsförderungsgesellschaft
FWF	Austrian Science Fund - Fonds zur Förderung der wissenschaftlichen Forschung
GDP	Gross Domestic Product
IPCC	Intergovernmental Panel on Climate Change
LANL	Los Alamos National Laboratory
LLM	Large Language Models
ML	Machine Learning
MQ	Main Question
NPM	New Public Management
NTU	Nanyang Technology University
NUSAP	Numerical, Unit, Spread, Assessment, Pedigree
ÖAW	Austrian Academy of Science - Österreichische Akademie der Wissenschaften
ÖNB	Austrian National Bank - Oesterreichische Nationalbank
PI	Principal Investigator
SFI	Santa Fe Institute
SOTA	State of the Art

SQ	Sub Question
STS	Science and Technology Studies
TOC	Theory of Complexity
TU	Technical University
UBS	Union Bank of Switzerland
WU	Economic University - Wirtschaftsuniversität
WWTF	Vienna Science and Technology Fund - Wiener Wissenschafts-, Forschungs- und Technologiefonds

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1 Introduction

1.1 Societal Crises and the Role of Science

"Against a backdrop of rapidly accelerating technological change and economic uncertainty, [...] the world is plagued by a duo of dangerous crises: climate and conflict. Underlying geopolitical tensions combined with the eruption of active hostilities in multiple regions is contributing to an unstable global order characterized by polarizing narratives, eroding trust and insecurity. At the same time, countries are grappling with the impacts of record-breaking extreme weather, as climate-change adaptation efforts and resources fall short of the type, scale and intensity of climate related events already taking place."

(World Economic Forum, 2024; p.4)

"It is increasingly recognized that addressing the polycrisis in ways that are not just effective but also equitable and just will require deep transformative change of political, economic and cultural structures, paradigms and practices. As I will argue in this essay, this has to include the transformation of [...] science as well."

(Turnhout, 2024; p.2)

The last couple of decades have seen the rise of what has by now become custom to define 'polycrisis' (Morin & Kerne, 1993): a series of distinct sociopolitical issues that interact with each other through feedback loops and interdependencies. If there was any need to make a list, one could mention climate change and environmental destruction, the Covid pandemic and its aftermath, conflicts, wars and genocides, the erosion of democratic values and the related rise of the far-right, growing economic inequalities and increasing economic volatility. While these crises unfold, drastic innovations are in parallel reshaping the way in which we think and work, latest among them the ascent of artificial intelligence – which builds on the internet and the digital revolution and is contextual to a series of sea changes in the role of data and computational tools both in the industry and in academia.

In the meanwhile, science struggles. Even though it has for centuries been the pride and glory of modernity, feeding the hubris-filled hope of technocratic prediction and control, science seems today to be barely holding on to the saddle of progress. Are we still able to understand the world we live in and to address the crises it is facing? How did the position of science with respect to society change, or must change, vis-à-vis the growing uncertainties and complexity of the contemporary world? How can science still be credible and produce knowledge that is 'relevant' for society over the noise of fake news and AI slop and given the reproducibility crisis and issues of legitimacy that affect scientific results themselves? Because of these multiple struggles, the last few decades have witnessed deep transformations in academia and many attempts to 'reform' science: from the introduction of metrics for the evaluation of scientists and their work, to the growing importance given to inter- and transdisciplinary approaches in the attempt of improving the quality of research. Moreover, science has progressively come more into the spotlight, either in positive or in negative terms, and new forms of communication and medialization have developed. Yet, all things considered, the critiques to science are all but rampant – both from without (consider the multitude of Covid and climate deniers) and from within (from a vocal crowd of critical scholars who more or less constructively denounce its shortcomings).

In its own small way, this thesis tries to keep in mind these two strains of background sensibilities. My approach, though, it is not to engage with them head on, but rather to take a detour, to trace them in a specific case study with the hope of learning something maybe less definitive but also much more textured about how these transformations are changing the world in which we live in and the knowledge we produce. Specifically, the choice of my case study is motivated by a fascinating concept – complexity – which is often positioned by commentators at the core of contemporary societal and scientific crises. Indeed, many argue that societal crises are all about issues that are 'too complex' to be addressed with traditional governance. In fact, the term polycrisis itself owes much to the notion of complexity (cfr. 1.3) and was introduced by scholars, Edgar Morin and Anne Brigitte Kern (1993), who openly associate with 'complexity thought'. By the same token, there is a sense in which most of the issues of contemporary science stem from the inability to properly deal with complexities – some of which science itself has brought about – and a sense in which science 'reforms' are all but an attempt to remedy this predicament. Before getting into the specifics of my empirical research, I begin by giving some hints towards a more direct, but still incredibly hard question. What is complexity?

1.2 Enter Complexity

"All human societies have invented means of coping with uncertainty and ways of reducing complexity. These coping mechanisms have ranged from elaborate and highly sophisticated cosmologies, inhabited by deities and demons, to institutionalized religions that have upheld their own belief systems supported by adequate social structures of authority. With the onset of

modern science, forms of coping based upon knowledge [...] have greatly enhanced the efficiency of reducing complexity as a way of coping with it."

(Nowotny, 2005; p. 20)

"The more we know about the workings of individual genes, banks or neurons, the less we understand the system as a whole. Consequently, an increasing number of the big questions of contemporary science are rooted in the same problem: we hit the limits of reductionism. No need to mount a defence of it. Instead, we need to tackle the real question in front of us: complexity."

(Barabási, 2012: p.14)

Many scholars agree that the last decades have scientifically, culturally, and politically been characterized by a renewed interest in and acknowledgement of complexity. Urry (2005) talks about a 'complexity turn' – originating in the natural sciences in the 80s and involving the social sciences starting from the late 90s – and affirms that by the early 2000s "the term complexity [was] 'present' and doing metaphorical, theoretical and empirical work within many social and intellectual discourses and practices besides 'science'" (Urry, 2005; p. 2). This resonates with Thrift (1999)'s suggestion that complexity theory might be the herald of a 'new structure of feeling', a new 'cultural hypothesis' characterized by a greater sense of openness and possibility. In a nutshell, the pillars of complexity thinking are the rejection of reductionist approaches, a greater emphasis on interactions and relations, the breaking down of the epistemological link between determinism and predictability, and the complication of linear causal relations[1] (Mitchell, 2009; Nowotny, 2005; Parisi, 1999; Urry, 2005). The notion of 'emergence', which is often used to characterize complex systems, is paradigmatic of complexity thinking: the whole is assumed to be superior to the sum of its parts, as properties 'emerge' at the collective level that cannot be reduced to, nor understood as, the simple summation of individual behaviour.

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[1] It goes without saying that the erosion of these epistemological tenets is part of a longer historical trajectory. In physics, for example – where this trajectory connects relativity, quantum mechanics, chaos theory, and complex systems – the notion of predictability has changed drastically throughout the last centuries (see Parisi, 1999). Some authors (see Wynne, 2005) are in fact critical of accounts that describe complexity as a 'new category' emerging since the 80s only within the sciences, arguing that such narratives tend to delete the fact that in ordinary culture a certain 'vernacular awareness' of complexity was already present from before "as an epistemic and indeed moral stance, [which could] be seen in many forms of popular resistance to institutional scientific hubris." (Wynne, 2005; p. 82).

Clearly, these perspective shifts are very radical and can act at multiple levels: from epistemology to culture, from cosmology and ontology to politics. For example, according to Najmanovic (2002), the scientific method itself should be brought into question when moving towards complexity thinking and the notion of paradigm be rejected in favour of more fluid and dynamic 'figures of thought'. Schweber and Wächter (2000) go as far as positioning complexity right at the heart of a new Hacking-type scientific revolution (Hacking, 1981; 1992a)[2] – which they term 'complex systems computer modelling and simulation' revolution, since "complexity has become one of our buzzwords and mathematical modelling and simulation on computers constitute [...] its style of reasoning" (Schweber and Wächter, 2000; p. 585). Clearly, a serious acknowledgement of complexity also affects politics and governance, for example by questioning the viability of social planning and control and redefining expectations and goals of policy interventions (Nowotny, 2005; Strand, 2002; Kovacic & Di Felice, 2019; Schrickel, 2017). Some scholars have even argued that complexity theory provides the central metaphor that underpins 'digital modernity', a new onto-epistemology that sees "society as organic and alive, to be herded and nudged through digital infrastructures, AI, and algorithms" (Törnberg & Uitermark, 2025; p.i).

Despite its importance, though, complexity remains extremely hard to define and pin down. While rhetorically it is a notion that can be deployed rather straightforwardly, what complexity refers to in practice is highly contested and situated. In fact, one could even say that the problem of defining complexity is in some sense ill-posed, since, with its vagueness, complexity connects to a myriad of concepts, domains, and levels of analysis. In the scientific context, just to give an idea, there are definitions of complexity and of complex systems in virtually any discipline (e.g., physics, see Parisi, 1999; biology, see Rosen, 1991; anthropology, see Tainter, 1998; engineering, see Perrow, 1984). Even restricting the interest to quantitative measures of complexity, one immediately incurs in a list of dozens of different options (see Lloyd, 2001). Moreover, complexity is also a matter of degrees – not an attribute dichotomously opposed to simplicity, but rather something located undefinedly along a spectrum. In this regard, scholars have distinguished between 'thin' and 'thick' complexity (Strand,2002), between 'ordinary' and 'emergent' complexity (Funtowicz & Ravetz, 1994a), between 'restricted' and 'general' complexity (Morin, 2007), and, of course, between 'simplicity', 'complicatedness' and 'complexity' (Allen et al., 2018; Strand, 2002). In addition, complexity is confusing because it is both an ontological and an epistemological

[2] A scientific revolution which entails the transformation of a wide range of scientific practices in multiple disciplines, changes research at an institutional level, and is linked with a substantial social and cultural change.

matter. Indeed, talking about complexity carries questions about what it is that we can know, and how, but also of what the world is, and what we are in its relation. Is complexity a property of phenomena out-there or is it only in 'eye of the beholder' (Horgan, 1995) – a relational property connecting to reality merely in terms of our descriptions of it? (see Allen et al., 2019). Finally, since even the tiniest rock can both be understood as something extremely simple or something extremely complex (for example by thinking about its atomic structure, or its geological and geographical history unfolding for millennia into the past and into the future), complexity is often a matter of framing, boundaries and scales – and it can therefore also become implicated with action and power through simplification and erasure.

1.3 Enter Complexity Science

"The nations and people who master the new sciences of complexity will become the economic, cultural and political super-powers of the next century."

(Pagels, 1988)

"The Santa Fe Institute gathered a motley crew of physicists, economists, computer scientists, and ant scholars, seeking to articulate from the fragments of digital experimentation a new arch-metaphor – a new way of seeing. This digital way of seeing – complexity science – revealed a hidden order in what had thus far appeared chaotic. A growing number of systems were said to follow similar decentralized principles. Flocks of birds. Schools of fish. Brains. Darwinian evolution. Immune systems. These complex structures are emergent, they rise up out of distributed local interactions."

(Törnberg & Uitermark, 2025; p.11)

Because of the ambiguities discussed in the previous section, it is hardly surprising that openly engaging with complexity in knowledge production confronts scientists with a series of particularly thorny issues. Indeed, even if complexity came into the scientific spotlight already a few decades ago, this has only shown, as George Cowan[3] put it, that "it's very hard to do science on complex systems." (cited in Horgan, 1995; p. 104). From a broad-brush perspective, there are two tensions that cannot be escaped when dealing with complexity in scientific research: that between the increase and the reduction of complexity and that between complexity and simplification. The first is described by Nowotny (2005) as an 'ongoing co-evolutionary process

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[3] Cowan is one of the founders of the Santa Fe Institute (SFI), the flagship institution of complexity science (see 2.4.1).

between science and society', i.e. a dynamic of mutual influence which oscillates between the need to reduce and tame complexity and the tendency to build and produce higher levels of complexity. Caught in an onto-epistemological paradox, it is often our very attempts at explaining or solving complexity through the production of new knowledge or technologies that inevitably also increase it. Simplification, for its part, is already a central aspect of scientific work in general (see Star, 1983) and becomes even more relevant when dealing with complexity. Indeed, no matter how much one nominally acknowledges complexity, simplification will still be a necessary step in research. How much can complexity be simplified? What can be simplified and for what reasons?

It is against this backdrop that a field of study named 'complexity science' began to emerge in the 70s and 80s. Sort of riding the wave of public and scientific excitement that followed the development of 'chaos theory', this scientific 'movement' first coalesced around an extremely influential research institute based in New Mexico: the Santa Fe Institute (SFI) (Williams, 2012; Li Vigni, 2020; for a longer history of the field, see 2.4.1). While society was being faced with increasingly global and 'wicked' problems (Rittel & Webber, 1973), complexity science was proposed as a possible approach to understand and model complex natural and social systems. These are defined, within the community, as large systems composed of several coupled sub-systems that interact in non-trivial ways and give rise to emergent complex behaviour that defy microscopic description. Often, they are characterized as chaotic (i.e. affected by discontinuous and non-linear dependency on initial conditions), adaptive (i.e. evolving according to dynamics that depend on on-going feedback loops and cannot be precisely determined at time zero), or nested (i.e. composed of sub-systems that are themselves complex systems). In specific regimes or conditions, they can also display phase transitions or the emergence of spontaneous order, two features that resonate with statistical mechanics models and quantum theory and that, it could be argued, hint at the possibility of engineering their evolution trajectories for the benefit of society. According to this loose definition, examples of complex systems abound – the environment, the web and the stock market being just the first three that come to mind.

As much as complexity science is often framed as providing a "post-Laplacian, anti-reductionist, holistic and interdisciplinary approach", it is an open question of whether this is actually achieved (Li Vigni, 2023). In particular, it can be questioned whether it is true that complexity scientists manage to tame complexity without reducing it or over-simplifying it, and whether the inter- and transdisciplinary relations inherent to complexity science are symmetrical or not. Indeed, mathematical, computational and physicalist assumptions and epistemic forms often seem to dominate in practice, opening up the question of whether the asymmetry is so marked to even qualify as 'imperialist' and colonizing towards the social sciences and humanities (Li Vigni, 2023; for a longer discussion of the epistemology of complexity science, see 2.4.3). Since complexity has become a powerful rhetorical and scientific construct – with growing relevance in many political and societal issues – it is interesting and worthwhile to attend to the ways in which complexity science and complexity more generally are performed and enacted, and what power relations are embedded into their practices and ideology. Complexity science is in fact not the only way to engage with complexity[4] and different approaches to the science of complexity can have drastically different consequences. As Gare (2000) has observed, whether or not complexity science is interpreted reductionistically, can have ramified consequences beyond epistemology, namely into the political realm[5]. Moreover, as noted above, scholars have also critically pointed to complexity science as a pillar of digital modernity (Törnberg & Uitermark, 2025), while others have identified in the SFI "an important node in a network of elite actors in business, politics, and media working to construct a new leadership class – the stewards of the New Economy and today's philanthrocapitalism; the creative class, the social entrepreneurs, the Santa Fe Institute libertarians" (Baker, 2022; p. 50).

In light of this discussion, complexity science is a great entry point to investigate the intertwinement of societal issues, the role of science and the political consequence of how knowledge production is organized. Moreover, complexity science also represents an idiosyncratic and worth-studying example of the recent transformations of academia, since it is purported to embrace a novel paradigm that in one fell swoop acknowledges and makes sense of complexity while reconfiguring scientific practices and institutional arrangements. Together with Law and Mol (2002), I agree that critical social sciences should try to find engagements with complexity that are more than mere critiques of simplification and it is exactly in this spirit that I decided to make complexity science the topic of my thesis. Concretely, I opted for an approach that aims to make sense of the nuances and minutiae of the practice of complexity science, rather than trying to develop a macro analysis of its ideology or epistemology. This reasoning brought me to my empirical site: The Complexity Science Hub (CSH) in Vienna.

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[4] To begin with, there exists an important tradition of 'critical complexity science' (see "Special Editors' Introduction: What Is Complexity Science?", 2001; Richardson et al., 2001; Woermann & Cilliers, 2012; Cilliers, 1998; 2005; 2016; Juarrero, 2002).

[5] Just to make an example, in Gare's opinion, the reductionists members of complexity science, the majority at the SFI, "have generally been happy to treat complexity theory as supportive with only slight modifications of a market economy and a social Darwinist view of history" (Gare, 2000; p. 21).

1.4 Empirical Site: Building Complexity Science at the CSH

"Since 2016, the Complexity Science Hub has conducted independent science for a better understanding of our complex world. We advance progress by extracting meaning from the enormous amounts of data representing our planet [...]. Using this knowledge, we can make evidence-based statements about how complex systems will respond to change and propose realistic interventions to move them in a positive direction for society. In other words, we want to find solutions for a better world."

(Complexity Science Hub, n.d.a)

Founded in 2015, the Complexity Science Hub falls into a broader tradition of large complexity science research centers, among which the SFI is the most well-known. From a sociological point of view, it is interesting to note that while complexity science managed to exert a strong influence over other scientific fields and the general culture, it has failed to institutionalize and become a scientific field in the Bourdieusian sense (Williams, 2012; Li Vigni, 2021c). To this day, in fact, complex systems specialists form a loose, international community that shares epistemic forms and practices but does not qualify as a "well-established and autonomous research and educational field capable of reproducing itself through professional institutions" (Li Vigni, 2021c; p.1; for a longer discussion of the institutional characteristics of complexity science, see 2.4.2). Nevertheless, this scientific enterprise is not short of success. The CSH, for example, has grown in the last ten years to employ more than seventy independent researchers (Complexity Science Hub, n.d.a), establish connections and collaborations with several other Austrian and international academic institutions, and collect funding from a wide range of both public and private organizations (for a longer history of the CSH, see 6.1). All these connections are in line with the general relevance of complexity science and testify for the academic authority of the hub and its embeddedness in the Viennese and Austrian context, as well as in the international network of researchers that to some extent identify with this field.

During my empirical work, which extended from June 2024 to October 2025 (see 5.3), the institute changed headquarters, moving from the 8th district, where it was based in the beautiful location of Palais Strozzi, to the 3rd district, where it set up tents in Palais Springer-Rotschild – an equally charming building surrounded by embassies and touristic attractions. This allowed me, quite literally, to witness the construction works of a new scientific institution. In fact, throughout the most intense part of my empirical work, the CSH was not only crowded with scientists, computers and ideas, but also with dust, workers and plastic sheets. Following this metaphor, which I also used to organize the empirical findings of this thesis (see 6), I would like to frame my empirical work as aimed at tracing and elucidating the scientific practices that construct complexity science at the CSH. Yet, I do not mean to convey an investigative view according to which I would be 'opening the black box' of complexity science, but rather highlight how my research process has tried to keep track of all the elements that relationally compose complexity science in the specific conjunction given by the CSH – regardless of whether they are public or intimate, designed or contingent, epistemological or social. Specifically, I have focused my discussion on two aspects. The first is the institutional configuration and institutional positioning of the hub, a hybrid between a private and a public research institute that manages to be very close to the Austrian universities and at the same time be markedly independent, and different, from them. The second is its scientific focus and its methodology, which are mainly characterized by grand claims of societal relevance and a focus on inter- and transdisciplinary methodology – with a generous sprinkle of complexity science approaches and sensibilities. The research questions I have addressed (see 4) focus on the lived experiences and perceptions of scientists working at the CSH and are thereby only of secondary relevance to the background interests laid out in Section 1.1 and Section 1.2 – to which I will only come back in Section 8. In fact, through research that started as an attempt to make sense of complexity, societal crises and the role of science, I found many other facets that multiplied like a fractal my research interest in the CSH – and decided it was worthwhile to give them space and attention.

2 State of the Art

2.1 Knowledge is Situated: Tracing the CSH as a Specific Location of Knowledge Production

This thesis contributes to the long-standing, and arguably defining, strand of Science Technology and Society (STS) research that investigates the locations and modalities of knowledge production, bringing attention to its situatedness and contingency. I say defining because, at least in some sense, it is through the anthropological study of scientific processes (see among many others, Latour & Woolgar, 1979; Knorr-Cetina, 1981; Collins, 1992; Traweek, 1988) that STS came to be organized as a specific area of study. This line of inquiry has been extremely prolific and successful and, while it is beyond the scope of my work to discuss it in any more depth than in passing, I do want to take as a starting point of my discussion one of its main contributions: showing that science is, in many ways, a deeply cultural activity – not one detached and neatly separated from culture (Pickering, 1992). As opposed to positivist imaginations, science is indeed not the result of purely rational and disembodied processes, but rather the result of contingencies, tinkering, and decisions that unfold within specific cultural and social settings and are therefore shaped by them. From this perspective, scientific 'facts' do not exist 'out-there', independently of whether or how we know them, but are rather the impressive outcome of an incremental and often 'black boxed' (Latour, 1987) work of 'construction' that entails purification, ordering and representation. Note, importantly, that this is not tantamount to a 'relativist' stance towards knowledge and its validity but only, put simply, to a change of perspective that understands truth as the result of scientific work instead as its starting point (Latour & Woolgar, 1979). If knowledge is then constructed, and not simply discovered, the practices and locations that underpin this achievement become all the more interesting to study. Born out of contingency, and not out of logical necessity, scientific practices and locations appear as multiple and situated, varying drastically across time, space and objects and methodologies of study. This was confirmed by the many works that focus on how science is done differently in different contexts, e.g. in different historical moments (e.g., Fleck, 1935/1979; Kuhn, 1962), in different nation-states (e.g., Traweek, 1988), in different institutional and organizational arrangements (Fochler, 2016a), and in different disciplines (Knorr-Cetina, 1999; Galison & Stump, 1996; Becher & Trowler, 2008). The present work contributes to and talks to this literature, focusing on a specific research center situated in Vienna.

As I have already argued above and will further argue below, there are many aspects that make the CSH unique and worthwhile studying. Nevertheless, the CSH is not exceptional and does not exist in a vacuum but moves within and connects to a broader scientific landscape. In order to pinpoint the aspects of this landscape that I believe are more salient for my analysis, I begin this State of the Art (SOTA) section by reviewing strands of literature that I organize into two broad blocks. The first block, Section 2.2, is concerned with the shifting relations between contemporary academia and society, while the second, Section 2.3, focuses specifically on the role, itself evolving, of data and quantitative methods in scientific research. This choice of topics is motivated by the facts that 1) it directly speaks to the institutional and epistemological configuration of the CSH; and 2) as I already begun to argue above, complexity is a red thread shining through both these two blocks of literature, at least at a rhetorical level. Finally, I will conclude this SOTA with an additional third section, Section 2.4, devoted specifically to STS and history of science literature on complexity science. Together, the following three sections will provide the necessary background and language for an analysis of the CSH and serve to trace all the main strands of literature that my analysis wishes to contribute to.

2.2 Shifting Landscapes: The Evolving Relations Between Science and Society

"Science has spoken, with growing urgency and conviction, to society for more than half a millennium. Not only has it determined technical processes, economic systems and social structures, it has also shaped our everyday experience of the world, our conscious thoughts and even our unconscious feelings. Science and modernity have become inseparable. In the past half-century society has begun to speak back to science, with equal urgency and conviction. Science has become so pervasive, seemingly so central to the generation of wealth and wellbeing, that the production of knowledge has become, even more than in the past, a social activity, both highly distributed and radically reflexive. Science has had to come to terms with the consequences of its own success, both potentialities and limitations."

(Nowotny et al., 2001; p.1)

"The Complexity Science Hub uses science to benefit society. We identify and study the problems that (will) affect citizens every day – directly or indirectly – and discover their causes and possible solutions."

(Complexity Science Hub, n.d.a)

"Combining qualitative expertise from diverse disciplines with a profound understanding of quantitative methods and modeling techniques, we map complex systems – ranging from ecosystems to supply networks. We convey this approach through hands-on experience. Central to our philosophy is to teach a research culture that transcends traditional disciplines or structures."

(Complexity Science Hub, n.d.a)

In this section, I discuss how knowledge production and academia have transformed in the last decades as a consequence of the progressive erosion of the boundary between science and society. While this boundary has always been blurred and contested, the phenomenon has been compounded in the last few decades because of a variety of factors. After a broad-brush introductory discussion (summarized in **Table 1** below), in Section 2.2.1 and Section 2.2.2 I pick up and expound the elements that I find most relevant for an analysis of the CSH – that is the cells in the rightmost column of the orange area in **Table 1** below.

<table>
<tr><td rowspan="7">Shifting science-society relations</td><td rowspan="4">Addressing societal challenges by modifying epistemic approaches and the organization of science (Mode 2 Science, Post-Normal Science)</td><td rowspan="2">Interdisciplinarity</td><td>Tensions in interdisciplinary collaborations (essential tensions, trading zones)</td></tr>
<tr><td>Transformations of disciplinary maps (emerging disciplines, proto-disciplines)</td></tr>
<tr><td rowspan="2">Transdisciplinarity</td><td>Different models of science-society interactions</td></tr>
<tr><td>Different modes of articulating societal relevance</td></tr>
<tr><td rowspan="3">Commercialization of science</td><td>Relation between economy proper and academia</td><td>Academic capitalism</td></tr>
<tr><td>Economic logics entering academia</td><td>NPM, epistemic capitalism</td></tr>
<tr><td>Medialisation of science and science communication</td><td>Public role of science, public role of scientists</td></tr>
</table>

Table 1 *Schematic representation of the aspects of the shifting relations between science and society that are most relevant for my analysis.*

The first order of reasons that brought science and society closer can be identified in the growing centrality of the former in understanding and governing the contemporary world – a task which, to make matter worse, appears to be riddled with unprecedented challenges. This predicament has been characterized in the literature in terms of complexity (see 1.2), uncertainty (Callon et al., 2009), risk (Beck, 1992), and wickedness (Rittel & Webber, 1973) and has often been diagnosed to be causally connected with the pervasiveness of science and technology themselves (see e.g., Perrow, 1984). Regardless of the perspective, almost all academic work seems to agree that standard modes of knowledge production need to be updated and expanded. Indeed, contemporary scientific research is characterized by a spurt of inter- and transdisciplinary collaborations[6] and a marked focus on societally relevant problems. Some scholars (e.g., Barry & Born, 2013) have argued that these transformations are part of the general societal drive towards 'innovation', while others (e.g., Felt et al., 2016) have pointed out that the transformation of scientific production can also be associated with at least three more goals, which connect to each other but are often pursued by different actors in an asynchronous, if not antagonistic, fashion: 1) making science more democratic, by allowing a broader set of actors to participate in defining scientific problems and developing solutions; 2) making science more epistemologically sound, by embracing a holistic approach that provides the right blend of knowledge and experience to identify and address the complex and interrelated challenges society is confronted with; 3) making science more legitimate and thereby strengthening the credibility of science-driven solutions in the public domain. There are mainly two overarching analyses have attempted to make sense of these developments in their totality: Post-Normal Science (Funtowicz & Ravetz, 1993; Funtowicz & Ravetz, 1994b) and Mode 2 science (Nowotny et al., 2001). Threading a fine line between the descriptive and the prescriptive, these two frameworks delineate and at the same time call for the integration of societal needs and demands in scientific research, a deep modification of scientific validation and evaluation criteria, and the acknowledgement of a multiplicity of previously neglected perspectives, commitments and locations of knowledge production.

At the same time that the epistemic gears of science were being changed in response to growing societal demands, a second move has equally been implicated in the erosion of the science-society boundary. I will label it 'commercialization of science' and conceptualize it as characterized by three related but distinct tendencies. First, the transformation of the relation between economy proper and science. On one hand, this has brought universities and research institutions to increasingly act as economic and for-profit actors (e.g., patenting scientific results, researchers acting as entrepreneurs, or study programs commercialized as products on an increasingly global market) and, on the other, has allowed commercial actors to enter and thrive in academia and decentralize the role of public universities (Mirowski, 2011; see also Fochler, 2016a, and Fochler & Sigl, 2018, for an interesting comparative study of knowledge cultures

[6] Interdisciplinarity refers to research that entails the collaboration of researchers belonging to different disciplines. Transdisciplinarity is an extension of interdisciplinarity and also extends the collaboration to societal and non-academic actors. Thereby, the latter also has stronger implications in terms of what counts as 'science', recognizing a much broader set of locations and modalities as legitimate in producing knowledge.

across the public-private divide). This new 'knowledge regime' (Bleiklie & Byrkjeflot, 2002), termed by some authors 'academic capitalism' (Slaughter & Rhoades, 2004), is characterized by a set of economic and social relations that progressively superpose knowledge to economic capital and thereby enact knowledge production as a chiefly economic process. Second, the progressive encroachment of economic and neoliberal logics, even beyond economy proper, into the functioning of science and academia. This dynamic is exemplified by the introduction in the 90s and 2000s of a new audit culture in research and teaching (New Public Management – NPM), motivated by the general distrust in public institutions and articulated through 'quasi-markets' underpinned by quantitative rankings and indicators (see Shore, 2008; Burrows, 2012). Within this system, capitalist accumulation of non-monetary capital (e.g. number of publications and citations) has become a central aspect of contemporary academic life. Going beyond the strictly economic connotation of 'academic capitalism', this phenomenon has been analyzed using the more flexible notion of 'epistemic capitalism' (Fochler, 2016b). Finally, the third element of what I understand as 'commercialization of science' are the new and ever more prominent formats of mediatization (Weingart, 1998; Rödder, 2009) and communication (Felt & Davies, 2020) of science. Increasingly resembling the activities of corporate Public Relations departments, such outreach activities have transformed academia both from within and from without, modifying the public perception of science but also reconfiguring scientific practices and the role and work of scientists (Felt & Fochler, 2012).

Overall, these transformations have yielded uneven results on a macro-scale. Indeed, many authors have argued that the quality and form of scientific outputs have not been improved much in spite of the great efforts to open up and restructure scientific practice (Hackett & Rhoten, 2009; Jahn et al., 2012; Felt et al., 2016; Schikowitz, 2020). The same seems to be true with respect to the organization of knowledge production according to market logics – which, if anything, has been called out for deteriorating the quality of scientific research (Alberts et al., 2014; Fochler et al., 2016). In part, this is also the result of these two pushes largely being in tension with each other and thereby forcing scientific institutions and individual scientists to engage in a perpetual juggling of different priorities and evaluation criteria (Felt et al., 2013; Felt et al., 2016; Fochler et al., 2016). In this scenario, falling back to a sort of business as usual becomes attractive as a lower risk approach to research. As a matter of fact, maybe the most remarkable effects of these transformations have been felt at a micro-scale, in the restructuring of everyday scientific practice and the modification of the lived experiences of scientists – often for the worse (Gill, 2016; Felt & Fochler, 2012; Felt et al., 2013; Turner et al., 2015; Fochler, 2016b; Fochler et al., 2016; Fochler & Sigl, 2018). In order to understand how these processes are unfolding, address their shortcomings, and realize their positive potential, closer scholarly and political attention are needed. This becomes even more pressing in the face of the urgency of the crises these transformations are supposed to help address.

2.2.1 Disciplinarity Updated: Interdisciplinarity

Disciplines are often considered to be the main organizational form in science. Scholars have conceptualized them as consisting of a community, a network of communication, a tradition, particular sets of values and beliefs, a domain, and particular concepts and approaches (Becher & Trowler, 2008). In practice, disciplines also 'discipline disciples' (Barry & Born, 2013, p.1), exerting a major and often tacit ordering force in scientific institutions. In light of this, it is hardly surprising that the modification and decentralization of disciplinary structures have turned academia upside down, engendering a host of issues both at an individual at a collective level.

2.2.1.1 'Essential Tensions' and 'Trading Zones' in Interdisciplinary Collaborations

Turner et al. (2015) identify three 'essential tensions' (Kuhn, 1977) in interdisciplinary research, which Kuhn defines as inherent and not resolvable tensions that, broadly speaking, stem from the friction between convergence and divergence – both fundamental aspects of scientific activity. In their empirical study involving program directors of interdisciplinary research centers, Turner et al. (2015) find interdisciplinary collaborations to add an additional layer to Kuhn's notion. Specifically, they organize interdisciplinary 'essential tensions' into three categories: an 'epistemic tension' between pluralistic and unified approaches, a 'structural tension' between institutional flexibility and institutional stability, and an 'affective tension' unfolding at the individual level between the need for comfort and the drive to exploration. Exactly because these tensions are essential, they cannot be resolved in any definitive way and are in fact negotiated by scientists, institutions and communities on an ongoing and always somewhat provisional basis. Turner at al. (2015)'s analysis brings attention to the fact that the modification of disciplinary structures has affected academia at multiple levels, extending way beyond the purely epistemic.

With respect to the first tension, the authors show that research teams or centers focusing on a specific problem or domain (e.g. sustainability research) might tend to lean towards a pluralistic epistemic stance, retaining much of the characteristics or the original disciplines. Yet, the drive to the integration of different paradigms into a unified framework is always at play and, when predominant, can establish a sufficiently stable set of norms and practices to turn, rather paradoxically, an interdisciplinary knowledge domain in what the authors term 'proto-discipline' (e.g., biochemistry, astrophysics, and, as I will argue below, complexity science). The second tension, which plays out at an institutional level, entails devising structures that provide researchers with independence and flexibility to engage in innovative research but that, at the same time, also provide them with enough continuity and stability to conform with academic expectations. In fact, one of the major challenges in this respect stems from the bureaucratic and organizational rigidity of universities, causing much interdisciplinary research to operate outside the university system proper or at least to seek independence from well-established disciplinary departments. Yet, this can only go so far, as researchers still need, in most cases, to go through the bottleneck of scientific evaluation. This is a clear instance of how the competition and market logics so prominent in science can indeed be in opposition with the drive towards new forms of knowledge production. Finally, Turner et al. (2015) argue that the affective tension between the sense of belonging and security provided by a disciplinary affiliation vis-à-vis the risks and curiosity of engaging in interdisciplinary research is often uneasily navigated by individual scientists, who resort to juggling 'multiple identities' and iteratively moving between disciplinary and interdisciplinary work.

Another interesting analysis that can help make sense of interdisciplinary collaborations is provided by Galison (1997). Inspired by anthropological studies of the interactions between social groups that do not share language or culture, the author introduces the metaphor of 'trading zone' to explain how scientists manage to collaborate and communicate across incommensurable scientific frameworks. The concept of 'trading zone' is particularly useful to understand the semiotic and linguistic aspects of interdisciplinary collaborations, in which exchange of knowledge and ideas can take place even if no common background has generally been established. In these situations, Galison highlights, different kinds of 'agents' (e.g. people, objects, tools) can function as mediators and afford a smoother communication. As I will discuss at length in the next section, computer simulations and mathematical models provide a good example of such mediating agents in interdisciplinary collaborations (see also, Galison, 1996).

While extremely helpful, these overarching analyses could be criticized for being somewhat insensible to power differentials between different disciplines and approaches. Clearly, these play an important role in shaping interdisciplinary collaborations and it is therefore important to complement the notions of 'essential tensions' and 'trading zones' with other works that focus on this aspect. For what concerns the notion of 'trading zone', Collins et al. (2007) are a good reference. In this work, the authors extend Galison's framework by considering two dimensions along which 'trading zones' can be different: the first referring to the extent to which power is used to enforce communication ('collaboration-coercion axis'), the second referring to the degree of homogeneity of the resulting language ('homogeneity-heterogeneity axis'). As a result, they categorize 'trading zones' according to four possible ideal types: 'inter-language' (collaborative homogeneity), ‘subversive’ (coerced homogeneity), ‘fractioned’ (collaborative heterogeneity), and ‘enforced’ (coerced heterogeneity). From an STS perspective, it is in general very important to stress that the integration of different forms of knowledge is not only an epistemic and organizational matter, but also a deeply political one. This of course applies to inter- and to transdisciplinarity alike (see Klenk & Meehan, 2015, for a discussion of this point but in reference to transdisciplinarity).

2.2.1.2 Unification and Stabilization: Emerging Disciplines

As discussed by Turner et al. (2015), the epistemic and structural essential tensions in interdisciplinary collaborations can be resolved through the creation of relatively stable and well defined ‘proto-disciplines’. In general, the modification of the disciplinary structure of academia has recently brought about a flourishing of new fields and areas of research, often characterized by a marked inter- or transdisciplinary focus. Molyneux-Hodgson and Meyer (2009)’s investigation of the emergence of synthetic biology is a great reference for understanding how such processes can play out in practice. Note that this analysis is particularly relevant for my research since complexity science, as I will argue in much more detail below and other authors have argued before me, can also be understood as an emerging ‘proto-discipline’ – somewhere between the discipline and the research domain. Molyneux-Hodgson and Meyer (2009) identify ‘movements’, defined as “the (re)orientation and enrolment of people, stories, disciplines and policies” (Molyneux-Hodgson & Meyer, 2009; p. 129), and ‘stickiness’, defined as “the objects and glues that begin to bind together the various constitutive elements of community” (Molyneux-Hodgson & Meyer, 2009; p. 129) as the two essential elements of a discipline in the making. These very much resonate with convergence and divergence, the two contradicting drives that engender Kuhn’s (1977) essential tensions. As per the authors’ definition, ‘movements’ can be of many different kinds: the movement and enrolment of people, the alignment of disciplines and approaches, the mobilization of material and symbolic resources, and the manipulation of past events and future promises in order to create a coherent story and trajectory. Yet, they are all geared towards the construction of a more or less homogeneous space in which it is possible and fruitful to work together. ‘Stickiness’ counterbalances this process by locking things in place, establishing a ‘within’ and creating a sense of collective and community. This is achieved, according to the authors, by tapping into a repertoire of ‘community making devices’ – such as associations, events, conferences, and journals. Molyneux-Hodgson and Meyer (2009) also stress the importance of ‘identity work’, interestingly noting that “the people who do synthetic biology do not describe themselves as synthetic biologists” (Molyneux-Hodgson & Meyer, 2009; p. 139). Rather, the scientists working in synthetic biology seem to articulate their identity in opposition to already existing ones and by resorting to 'imagination' and 'alignment': "the envisioning of possible futures and the doing what it takes to get there" (Molyneux-Hodgson & Meyer, 2009; p. 139).

2.2.2 Letting Society In? Transdisciplinarity and 'Societally Relevant' Science

The process of opening up science to the contributions of societal actors is by no means straightforward, neither is the attempt to make science outputs more focused on issues that are relevant for society. Achieving these goals, indeed, is predicated on a dramatic restructuring of scientific practice and a deep modification of what is understood as 'good science'. This is particularly challenging in contemporary academia, which is shaped by requirements and priorities that have been shown to often be at odds with the production of societally relevant knowledge and the development of careful collaborations with societal actors. In particular, scholars have drawn attention to the competition and production imperatives embedded in the NPM ideologies and to the narrow quantitative assessment of scientific worth (Felt et al. 2013; Felt et al., 2016; Fochler et al., 2016) as compromising factors of the quality of transdisciplinary research. Moreover, the notions of societal and scientific relevance are also in tension with each other, engendering complicated dynamics in transdisciplinary collaborations (Schikowitz, 2020). Within disciplines, finally, which lately have also tended to reconfigure themselves in order to accommodate societal relevance within their scope (see Granjou & Arpin, 2015; Sigl et al., 2023; Bieszczad et al., 2025), articulating societal relevance engenders a friction between entrenched ideals that tie relevance to the production of 'fundamental knowledge' and the drive towards producing actionable and directed knowledge through applied research (Bieszczad et al., 2025).

Because of the dominant rationales of contemporary academia, little time and space can be devoted to acknowledging plurality and carefully dealing with the tensions that riddle transdisciplinary and societally relevant science. This bottleneck brings scientists and research institutions involved in transdisciplinary research to too often force the integration of different points of view, complying with what Klenk and Meehan (2015) define as 'integration imperative' in transdisciplinary research. The two authors criticize this paradigm as exclusionary and concealing of "the friction, antagonism, and power inherent in knowledge co-production" (p.2) and advocate for more careful engagements with plurality that would recognize and value difference instead of glossing over it or erasing it. This also resonates with the work of Schikowitz (2020), which has shown that scientists mostly resort to the 'integration machine' (Igelsböck,

2016) of modelling in order to smoothen out and resolve the tension between policy relevance and scientific relevance in transdisciplinary research.

In practice, overall, both transdisciplinarity and ‘societal relevance’ are highly contested and situated, with no one-size-fits-all articulation or description. The works that I discuss next are attempts to make sense of this multiplicity and can provide a useful chart to orient oneself in the messy geography of transdisciplinary practice and societally relevant science.

2.2.2.1 Different Models of Science-Society Interaction in Transdisciplinary Collaborations

In (Felt et al., 2016) the authors identify three ideal typical models of science-society relations in transdisciplinary collaborations, distinguished according to the extent and modalities of interaction between scientific and extra-scientific actors: the ‘linear translation model’, the ‘delimited neutral arena model’, and the ‘temporary shared epistemic arena model’. As ideals, these models are not necessarily separated in practice, rather they should be considered as an analytical tool that can help making sense of the different registers and frames of reference used by scientists and practitioners alternatively depending on the situation and context. Overall, it is important to stress that in all these models the collaboration between scientific and extra-scientific actors only takes place in a rather delimited and temporary fashion, without involving durable and extended knowledge relations across the science-society divide. As already anticipated above, according to the authors this predicament can be ascribed to the “tensions between broader societal valuing of research – that is, considering research in terms of its contribution to the public good [...] – and evaluating research – that is, following disciplined thinking and complying with the values that are important in research systems and funding schemes, which predominantly focus on narrow indicators when assessing people and knowledge” (Felt et al., 2016; p. 753).

In the ‘linear translation model’, the scientific and societal arenas are clearly separated, with knowledge being produced by science and then being translated to society. In this model, societal issues are assumed to be well-defined and preexisting, and the role of transdisciplinary collaborations to be that of steering science towards societally relevant problems and of communicating relevant scientific knowledge to society. A prime example of this model is commissioned work, in which, “although the societal problem is initially formulated by societal actors, it is then appropriated by scientists and transformed into a scientific problem to be processed without further external intervention” (Felt et al., 2016; p.746) – an exchange that relegates extra-scientific participants to the role of users of scientific solutions and leaves science’s epistemic authority unquestioned. In the ‘delimited neutral arena model’, while the scientific and societal spheres are still very well distinguished, societal actors are conceptualized to possess key knowledge or information about the problem to be solved (e.g., datasets, experiential knowledge). Therefore, specific moments of the research process are designated to the mutual exchange between scientists and societal actors. These exchanges, though, are 'neutral', in the sense that they do not affect the core of knowledge production (carried out by scientific actors in a separate setting) and allow societal actors to only act as advisors. Finally, in the 'temporary shared epistemic arena model' scientific and extra-scientific actors are guaranteed an almost equal level of epistemic authority, but only in a clearly delimited part of the research process (the shared epistemic arena). After a short period of mutual exchange of knowledge (e.g. a workshop), scientists "engage in purification work by translating their experiences, knowledge, and reflections into an adequate scientific form" (Felt et al., 2016; p. 751).

2.2.2.2 Different 'Modes of Doing Societal Relevance'

As Hessels et al. (2009) have argued, it would be a mistake to consider the stress on societal relevance in science as a completely new phenomenon (see also, Rip, 1997; Savransky & Stengers, 2016). Nevertheless, the last couple of decades have seen both an intensification in the use and a semantic shift of the term 'societal relevance'. While relevance has traditionally referred mostly to linear and trickle-down models of how science benefits society (Godin, 2006) – often with a direct correspondence between relevance and economic growth, for example in terms of technological innovation -, lately this expression has largely come to signify the ability of scientific knowledge to address and possibly tame the multiple global challenges that affect contemporary societies (Brunet et al., 2025). The term remains extremely vague and ambiguous[7], though, with its definitions and articulations often being context and field-specific (Hessels et al., 2009; Brunet, 2024). Building on previous works that have tried to make sense of this multiplicity (Hessels et al., 2009; Nicolai & Seidl, 2010; Granjou & Arpin, 2015; Klenk & Meehan, 2017; Schikowitz, 2020), Brunet et al. (2025) define four different 'modes of doing relevance'. Their conceptualization is based on a 'practice-based' approach, which "highlight[s] how researchers, research funding institutions, political administrations and various stakeholders jointly shape what counts as relevant knowledge" (Brunet et al., 2025; p. 188). From this perspective, the

[7] As Brunet et al. have noted (2025), this ambiguity is also reflected in the flourishing of a vast vocabulary of similar, and similarly ambiguous, notions – e.g., 'research usefulness' and 'societal impact'.

different modes are to be understood as four different dimensions with respect to which the construction of relevance can be articulated in practice.

The first mode, 'relevance as selecting and reorienting research topics and disciplines', refers to the epistemic content, the disciplinary organization, and the methodology of scientific research – following the idea that more societally relevant knowledge can only be produced thanks to novel scientific approaches. This mode is reflected both in transformations taking place within disciplines (see e.g., Granjou & Arpin, 2015; Sigl et al., 2023; Bieszczad et al., 2025) and in transformations that change the geography of disciplinary organization (see 2.2.1). The second mode, 'relevance as engaging societal actors and conducting user-driven research', focuses on the engagement of societal actors in research, i.e. transdisciplinarity itself. The third mode, 'relevance as rearranging the interaction between science and policy' focuses on the reorganization of the relation between science and policy. In this case, relevance is often defined with respect to the demands and needs of political and funding institutions, which are able to define what counts as relevant in terms of existing norms and policy frameworks. The fourth and final mode, 'relevance as transforming academic institutions', accounts for the institutional transformations that are aimed at making science more relevant. Central to this mode are attempts to modify the academic evaluation and competition criteria that, as already mentioned, are often directly counterproductive to the production of societally relevant knowledge (Felt et al., 2013; Felt et al., 2016; Fochler et al., 2016). Moreover, this mode also entails a partial reorganization of university structures, with a flourishing of inter- and transdisciplinary initiatives and research centers (for example the CSH).

2.3 Weathering (and Forecasting?) the Storm: Quantification and Datafication

"Computer models are, and will remain, the historical, social, and epistemic core of the climate science/policy community. Without them, we would know little if anything about the causes and possible future consequences of climate change. At least as importantly, from a science-studies viewpoint, the global epistemic community that now surrounds the climate change issue would have lacked a fundamental organizing principle. [...] Thus global modeling does not merely represent, but in a social and semiotic sense constructs, the global atmosphere."

(Edwards, 2001; p.64)

"We are Europe's Research Center Translating Data into Solutions for a Better World."

(Complexity Science Hub, n.d.a)

"We share a determination to understand the world through rigorous quantitative approaches."

(Complexity Science Hub, n.d.a)

Espeland and Stevens (2008) broadly define quantification as the "production and communication of numbers" (p.407). A central tenet of modern ontology (Espeland & Stevens, 2008), quantification so defined affects and shapes virtually every aspect of contemporary societies, from politics to economy, from health to sociality. In this section, I am particularly interested in one facet of quantification, and specifically in the relevance of quantitative methods for the production of knowledge. Increasingly, though, talking about quantitative research methods has become impossible without explicit reference and attention to data (and, relatedly, artificial intelligence) – which I have therefore chosen as the second focus of this section. Indeed, as Edwards et al. (2013) have argued, "enormous transformations have occurred over the last 20 years in our systems for generating, sharing, and disputing human knowledge" (p.1) – transformations mostly rooted in the advent of the internet and big data and in the increase of computational power. These changes have been so consequential to bring some commentators and scholars to diagnose the 'end of theory', the obsolescence of the scientific method (Anderson, 2008), and the onset of the 'fourth scientific paradigm of data-intensive science' (Hey & Microsoft Research, 2009; Mayer-Schönberger & Cukier, 2013). More than fifteen years later, these analyses assume even higher relevance vis-à-vis the rise of AI, Machine Learning (ML) and Large Language Models (LLMs) and the appearance of 'digital twins' as a proposed tool for understanding and managing social and environmental phenomena (see "The increasing potential and challenges of digital twins", 2024; and, for a critical take, Saltelli et al., 2025). Incidentally, the variety of these examples also showcase another aspect I find very important for my discussion: that is that the two categories of 'data' and 'quantitative methods' are anything but stable and unitary. Quantitative methods, for one, encompass an extremely broad set of practices and methodologies – from modelling and simulations to evolutionary equations and statistical analysis, just to name a few – and have been the subject of very deep and ramified transformations throughout the last century. Just think – even if both can be understood as a form of 'quantification' – what other commonalities there are between the work of a theoretical physicist trying to work out the principles of quantum mechanics in the 1920s and the work of a modeler who tries to identify meaningful correlations between anthropic action and climate change for the next IPCC report. Similar examples could be drawn for the category of 'data', which also means wildly different things depending on the specific epistemic contexts, for example in different disciplines (see 2.3.3) or at different points in time (e.g., before and after the introduction of web data-scraping techniques). Therefore, another important goal of this section is to pin down the meaning and frame of reference of these two broad categories for the purposes of my analysis, fleshing out exactly what aspects, conceptualizations, objects and practices that relate to them I am most interested in.

Finally, I believe the following analysis to also be a very fitting complement to the previous section, in which I reviewed literature on the shifting relations between science and society (see **Table 1**, p. 21). In fact, I would go as far as to say that data and quantitative methods are the glue through which most of the science-society interactions are held together, even though they play different roles in relation to different aspects of those interactions (see **Table 2** below). Just to give a few examples, data and quantitative methods play a crucial role in inter- and transdisciplinary collaborations – often being used as a way of smoothening tensions and frictions (see Schikowitz, 2020) or becoming a constitutive element of interdisciplinary 'trading zones' (Galison, 1996; 1997; see below); they are the pillars of scientific evaluation and competition, thanks to indicators, indices and rankings; they also sit comfortably at the nexus of science and policy – informing 'evidence-based' political decisions, representing 'boundary objects' (Star & Griesemer, 1989) for different political and scientific groups (Edwards, 2001), and functioning as promissory devices that shape our vision of the future and at the same time act in practice to reconfigure the present (Hoeyer, 2019; Lippert, 2022); and, maybe most importantly, many of the societal issues and crises that science is currently tasked to address can only be understood through, and thereby constructed by, massive data collection and the deployment of quantitative methods – as Edwards (2001) argues in the opening quote of this section. Therefore, data and quantification not only shape the form and format of what we know, but also the scope of our knowledge, the way science is structured from an organizational and institutional point of view, and its relation to society at large. The explicit stance of the CSH – which openly embraces data and quantitative methods as core components of its scientific practice – makes it a great location to follow how the centrality of quantitative methods, the differences they encompass, and the challenges they engender are made sense of and navigated by scientists. This is especially true in light of the epistemic focus on 'complexity', which also consists of elements that are irremediably resistant to quantification.

Different aspects of the science – society relation		Role of quantification
Epistemic and organizational shifts	Interdisciplinarity	Quantitative methods and data as mediators
	Transdisciplinarity	Quantitative methods and data as mediators; authority of quantification to justify societal interventions
Commercialization of science	Relation between economy proper and academia	Quantification as an ordering principle and as a tool of evaluation (metrics, indicators)
	Economic logics entering academia	

Table 2 *The role of quantification, data and quantitative methods in relation to different aspects of the science – society relations discussed in Section 2.2 (see **Table 1**, p. 21).*

In Section 2.3.1, I will trace literature that can help unpack the notion of quantification, in general, and understand the ethical baggage that comes with its relevance in knowledge production, in particular. Section 2.3.2 explores STS and philosophy of science works on modelling and simulations – only two of the many practices that fall under the umbrella term of 'quantitative methods', but which are both particularly relevant for an analysis of the CSH. Finally, Section 2.3.3 is devoted to the discussion of literature that engages with the concepts of 'data' and 'big data' from a critical perspective.

2.3.1 Sociology and Ethics of Quantification

Back in 2008, Espeland and Stevens claimed that sociologists, contrary to anthropologists and scholars from other disciplines, had so far paid little attention to the "spread of quantification or the significance of new regimes of measurement" (p. 401). In the same work, the authors also pointed out that recognizing the social character of quantification is the first step in the longer project of also developing an 'ethics of quantification'. In this section I mostly rely on a specific corpus of works that have answered to this double 'call to arms' (Berman & Hirschman, 2018; Espeland & Yung, 2019; Islam, 2022) and I begin with laying out and explaining the characteristics of quantification that, brought into light by these analyses, I find most relevant for my discussion. In doing this, I roughly follow three of the five quantification themes introduced in (Espeland & Stevens, 2008), additionally highlighting the main ethical concerns implicated in each of them. Importantly, together with Islam (2022), I distinguish between epistemic and political concerns related to quantification[8] – focusing on the former. I then close by briefly introducing three approaches that, also inspired by the work of Espeland and Stevens (2008) but more neatly fitting into the Post-Normal Science tradition, have been proposed to operationalize ethics in quantification processes.

2.3.1.1 The Composition of Quantification

First of all, it is important to deconstruct 'quantification' as a polished and black-boxed category. There is a tendency to see numbers as independent and self-reliant entities, abstracted from the context in which they are produced and abstracting of whatever they aim to represent. This position cannot be held from a sociological point of view, as numbers are socially produced through a great deal of effort and coordination. Espeland and Stevens (2008) use the term 'work' to indicate this dimension of quantification. In their words, "Rigorous, defensible and enduring systems of quantification require expertise, discipline, coordination and many kinds of resources, including time, money, and political muscle." (Espeland & Stevens, 2008; p. 411). Making these processes visible, together with the choices and trade-offs they entail, has been a central aim of sociology of quantification also according to Berman and Hirschman (2008), who rephrase it as the question of "What shapes the construction of numbers?" (Berman & Hirschman, 2018; p.3).

By the same token, quantification is an articulate category, which is performed through a series of interlocking steps. Islam (2022) breaks it down into three distinct subprocesses: 'capture', 'specification', and 'appropriation'. The first subprocess refers to "how lived experiences and everyday interactions in social life are cast into quantified or quantifiable forms" (Islam, 2022; p.199). It entails the conflation of the many ambiguities of reality into a unique quantitative expression and a form of alienation, i.e. the separation, through abstraction, of experience from its subjects. The second subprocess refers to the procedures "by which choices are made as to *how* something should be quantified" (Islam, 2022; p.200). These choices are in most cases black-boxed *ex-post* but have deep ethical implications, especially concerning who or what is excluded or included, simplified or accounted for. Espeland and Yung (2019) term this ethical dimension of quantification 'attention' – referring to the capacity of numbers to focus attention

[8] The first are about "numbers and their relation to social reality, representation, and the consequences of articulating complex qualitative experience as quantitative data" (Islam, 2022; p. 196). The second relate "quantification to social control, both by state and corporate actors who use numbers as technologies of governing [...], or by market actors who can capitalize on numbers by extracting economic value from quantitative data" (Islam, 2022; p.196).

and produce relations of (in)visibility). The third process, finally, refers to the step in which "numbers become the property or capital of specific actors" (Islam, 2022; p.200).

2.3.1.2 The Authority of Quantification

As Espeland and Stevens (2008; p. 416) put it, "one thing numbers do is persuade". Indeed, it is the authority of numbers and quantitative methods that grants them their privileged role in knowledge production, society, and politics. Suspending value judgements, it is important and interesting from a sociological point of view to first untangle how this authority is constituted. Here, Espeland and Stevens's work (2008) is again very helpful, as it identifies several distinct but related elements that work in this direction. A first component is the idea that numbers provide faithful and accurate representations of reality (Desrosières 1998) – both an epistemological and an ontological statement. Relatedly, the second element identified by Espeland and Stevens (2008) is the association of numbers with objectivity and rationality (see Daston, 1992; Daston & Galison, 2010; Weber, 1978). Third, the authority of quantification is also vested in the idea that numbers are particularly useful in solving problems (see Porter, 2005), an opinion that can be hold even if their adherence to reality is bracketed away or openly questioned. Finally, according to Espeland and Stevens (2008), numbers also increase their authority by becoming embedded in networks, institutions and infrastructures. Once produced, indeed, measures and numbers travel lightly, spreading widely and affording easy practical usage. Through use, they then become routinized and progressively more stable, engendering a lock-in effect that makes them hardly dispensable. Numbers support organizations and infrastructures and, thereby, become institutionalized (see Kalthoff, 2005; March & Simon, 1958).

Clearly, a good part of the authority of numbers is well deserved. Embodying maybe better than anything else the very spirit of modernity, quantification has indeed being extremely productive, both historically and in the present. Even without subscribing to any 'progress' or 'innovation' narrative, it is undeniable that without measurements and numbers many of the things that make our lives (better) would not exist. Yet, as Espeland and Stevens argue (2008; p. 432), "an ethics of quantification should view this ontology as productive but partial". The risks of an unchecked faith in numbers are comparable to those of an absence of quantification, especially if it comes at the expense of other ways of knowing. To its extreme, the consequence can be that of conflating what is real with what is measurable and thereby irremediably narrow our ability to know and ascribe value. In sum, "an ethics of quantification should investigate how the world is made by measures, but should strongly reject any conceit, scientific or otherwise, that measurement provides privileged or exclusive access to the real" (Espeland & Stevens, 2008; p. 432).

2.3.1.3 The 'Reactivity' and 'Performativity' of Quantification

Another important characteristic of quantification is that once numbers are produced, they act and intervene in the world – not necessarily in expected ways. Espeland and Stevens (2008) call this 'reactivity', bringing attention to how measures "cause people to think and act differently [and] alter relations of power by affecting how resources, status, knowledge and opportunities are distributed" (Espeland & Stevens, 2008; p. 412). This is a well-known feature of measures and is in fact somewhat resonating with Goodhart's law in economics, which states that "When a measure becomes a target, it ceases to be a good measure" (Goodhart, 1984; see also Campbell's law, Campbell, 1979; Saltelli, 2020). A specific form of 'reactivity' is performativity, i.e. the ability of quantification to "produce the realities it purports to describe" (Islam, 2022; p.203; see also, Gond et al., 2016). With respect to performativity, Islam (2022) proposes a very useful distinction between 'retro-performativity' (Appadurai, 2016) and 'telic performativity'. The first "describes how the process of attributing a numerical value [...] to something based on a particular scalar dimension [...] produces the impression that that thing always existed, waiting to be measured." (Islam, 2022; p. 203-204). The second "involves positioning a metric as a telos or goal" (Islam, 2022; p. 204) and basically coincides with Goodhart's law. All these concepts bring attention to the fact that measurements and numbers are never neutral and without consequences: much more than simple 'representations' of reality, they are deeply implicated in 'making reality'. From an 'ethics of quantification' point of view, this aspect is particularly relevant as science is pushed to produce 'societally relevant' knowledge and more directly than ever intervene in society (see 2.2; 2.2.2.2).

2.3.1.4 Applied Ethics of Quantification

So far, I have discussed quantification, and the ethical concerns it raises, mostly from a descriptive and rather conceptual point of view. But there is also another way in which scholars have engaged with the matter – that is, by developing frameworks that normatively aim to embed ethical considerations within specific quantification processes. Using Islam's (2022) language, these scholarly contributions attempt to intervene at the 'specification' level, engaging headfirst with the question of *how* quantification is performed. All these works share a Post-Normal Science sensibility, according to which knowledge should not be valued in terms of 'validity' but rather in terms of 'quality' – the latter being a broader category premised on the acknowledgement of the uncertainty and ethical implications entailed by any scientific process (see Funtowicz & Ravetz, 1993; Funtowicz & Ravetz, 1994b). Even if not directly relevant to my analysis, I still find it important to mention these frameworks. In particular, I believe these references could come in handy for any practitioner who might happen to read this thesis.

The broadest in scope and oldest of these frameworks is NUSAP (Funtowicz & Ravetz, 1990; van der Sluijs et al., 2005), a notational system for the management and communication of uncertainty in science for policy. NUSAP is based on the idea that any quantitative statement that is relevant in policy processes should be complemented with additional information about how it was produced and what is its intended use. Sensitivity auditing (Saltelli et al., 2013; Saltelli & Funtowicz, 2014), the second framework I want to mention, is inspired by NUSAP but is more specifically tailored to modelling. Proposing seven rules that are intended to serve as a reminder of important and often overlooked aspects of quantification, it can be used as a checklist both during the modelling process per se and in the evaluation of existing models. Finally, during the Covid pandemic Saltelli et al. (2020) wrote a manifesto to propose five rules to ensure that modelling serves society (see also, Saltelli & Di Fiore, 2023).

2.3.2 Modelling and Simulations

The term model is rather promiscuous (Sismondo, 1999) and can refer to a great array of different things. The most basic scientific models are material and conceptual analogous of real-world phenomena: “manageable systems, or systems thought to be comprehensible, that stand in for unruly or opaque ones.” (Sismondo, 1999; p. 248) – e.g., Wilson’s cloud chamber was a model system to study cloud formation (see Galison, 1996). Mathematical and computer models share a similar relation to reality and a similar scope but, more elaborate and in some sense more abstract, in their case the correspondence with the phenomena they are purported to be analogues of is articulated through semantic rather than material components (e.g. equations, data sets, interaction rules and so on)[9]. Simulations, instead, are the results of the implementation of models over time and are aimed at obtaining information about how the original systems might behave. Historically, while physical and mathematical models have, to

[9] Another type of modelling that is particularly relevant for my analysis of the CSH is Agent Based Modelling (ABM). ABM is mostly used in social sciences and biology (e.g. to model a crowd or a flock of birds) and consists in creating an analogous of a system by initializing in a computer program the elements of the system (agents) and a set of rules for the interactions between the elements. By simulating the behaviour of the individual agents, given the interaction rules, the model will then be used to reconstruct the collective behaviour. With the appearance of LLMs, a new type of ABM is starting to be deployed: Generative Agent Based Modelling (GABM), in which the agents are represented by LLMs – supposedly reproducing human conversation and behaviour. Showcasing the importance of ‘minding the assumptions’ and ‘minding the framing’ (Saltelli et al. 2020; Saltelli & Di Fiore, 2023) in modelling practice, ABM has been criticized for being embedded of an ‘individualist view’ of the social world (O'Sullivan & Haklay, 2000).

some extent, always been part of the repertoire of modern science, the advent of computer models, beginning in the period right after World War II, can be understood as a complete paradigm shift in knowledge production (Edwards, 2001). At this point in time, not only was a new method developed, but this was also one that relied on a new metaphysical understanding of nature (in the case of Monte Carlo models, "a nature composed of discrete entities interacting through irreducibly stochastic processes" – Galison, 1996; p. 120) and that required a revolution in terms of how science was practiced and by whom (with the "traditional professional categories of experimenter and theorist challenged by an increasingly large and vocal cadre of electrical engineers, and later by computer programmers." – Galison, 1996; p.121). In this process, also the role of the computer was transformed, going from "computer-as-tool to computer-as-nature" (Galison, 1996; p. 121) – a location where experiments could be performed.

Today, models are "an increasingly important set of tools, methods and practices that complement and, in part, substitute for the traditional theoretical and experimental modes of doing science" (Knuuttila et al., 2006; p. 3). As such, they are of relevance in basically any scientific field as well as in "social, economic and environmental prediction and decision-making" (Knuuttila et al., 2006; p. 3). Indeed, models have become an 'obligatory passage point' (Callon, 1984; Edwards, 2001) to understand many of the greatest scientific challenges of our time. Edwards (2001) gives the example of climate modelling, which is crucial to make predictions about climate change and assess its causes – in some sense, as mentioned above, "constructing the global atmosphere" (Edwards, 2001; p. 64). Because of their pervasiveness and socio-cultural and political relevance, then, models cannot be analyzed simply as scientific tools – but need to be understood also as "distinctive cultural forms that deserve their own scrutiny, a scrutiny that extends beyond the technical expertise of those who make and use them" (Espeland, 2023; p. xii). Expanding on this point, an analysis of models should not only consider their consequences on society (their 'reactivity', using the language introduced in the previous section), but also be attentive to the many cultural elements that participate in the 'work' of constructing them. This resonates with Ravetz's (2003) proposal to consider models as 'metaphors' – which, besides serving a scientific function, also embody "societal and cultural values which doubtless shape the[ir] practice" (Ravetz, 2003; p. 68). As metaphors, models not only provide knowledge by 'assertion' (e.g. by providing predictions about the future), but also by 'suggestion', telling us "about our social and intellectual world of the present" (Ravetz, 2003; p. 69-70). An empirical confirmation of these ideas can be found in (MacKenzie & Spears, 2014), in which the authors investigate the use in finance of the 'Gaussian Copula Models'. This is an important family of models that were deeply implicated in the 2009 economic crisis. Building on previous work by

MacKenzie (2003), the authors understand modelling construction in economics as 'bricolage' and show that the development of the 'Gaussian Copula Models' was the result of a historically situated process – deeply influenced by a specific 'evaluation culture' (that of the 'no-arbitrage modelling'; see also 3.2.2) and a multitude of socio-cultural and political goals and constraints lying outside economy proper.

Keeping in mind the dual status of modelling as both an epistemic practice and a 'cultural form' (Espeland, 2023), Section 2.3.2.1 discusses what is maybe the single most defining socio-epistemic characteristic of models: their ability of inhabiting a space in-between and from there to act as 'mediators' (Morgan & Morrison, 1999). Section 2.3.2.2, instead, is devoted to a more general epistemological discussion of models – touching upon issues of validity, prediction, and accuracy.

2.3.2.1 Models in the In-Between

In the literature on modelling from history and philosophy of science and STS, one of the main recurring themes is the realization that models are able to "cut across boundaries of pure categories we accept in science, and sometimes politics" (Sismondo, 1999; p. 258; Sismondo, 2000; Galison, 1996; Morgan & Morrison, 1999; Dowling, 1999; Winsberg, 1999; Knuuttila et al., 2006; Mattila, 2005). Gaining momentum since the 90s, a practice-oriented approach to the study of models has allowed scholars from these disciplines to see "models and simulations [...] as objects that draw together the entire web of activities in which they are enlaced" (Knuuttila et al., 2006; p.6). Sismondo (1999; p.258) beautifully describes them as "monsters necessary to mediate between worlds that cannot stand on their own, or are unmanageable [...] a form of glue, simultaneously epistemic and social, that allows inquiry to go forward, by connecting the ideal and the material". In order to better understand the connecting and mediating quality of models, I first focus on their relationship with theory and experiments and then on their role in interdisciplinary collaborations.

While earlier accounts only relate models to theory (Redhead, 1980) and attempt to make sense of them as either 'impoverished' (e.g., relying on simplified assumptions or equations) or 'enriched' theories (e.g., specifying boundary conditions for otherwise too general frameworks), by the late 90s scholars started to adopt a broader and more nuanced point of view – one that at the same time complicated the relation between models and theory and acknowledged their similarity with experiments and their relation to data. Sismondo (1999), for example, understands models as bridges between theories and data, which take theories as inputs and produce predictions about the real world. Edwards (2001), on the other hand, describes models, climate ones specifically, as ‘data-laden’. This term refers to the fact that models heavily depend on data to fill in the gaps left open by physical theories and laws. Reconstructing a historical account of the development of Monte Carlo simulations, Galison (1996) shows that this novel methodology bounded its practitioners to the work of the experimenter and to that of the theorist at the same time, thereby being "at once nowhere and everywhere on the usual methodological map” (Galison 1996; p. 120). Winsberg (1999; p. 290) argues that models possess their own epistemology, which is “very much an empirical epistemology, and not merely a mathematico-logical one”. Morgan and Morrison (1999) describe models as ‘autonomous agents’, whose independence from both theory and the world allow them to be used as tools of exploration in both realms. Dowling (1999), finally, maybe expresses the experimental-theoretical duality of models in the clearest fashion, defining modelling work as a form of ‘experimentation on theories’. Investigating how practitioners themselves reconstruct the usefulness and authority of models in the accounts of their own practices, Dowling (1999) shows that modelling is like theory in terms of ‘what scientists work with’ (i.e. ‘representations’ rather than ‘things’ or ‘reality’) but that it is like experiments in terms of the type of activity it involves (i.e., ‘fiddling’, ‘tinkering’, ‘trying things out’, and ‘waiting to see what happens’, rather than ‘manipulating equations’ or ‘developing ideas’).

The ability of models to connect both with theory and with experiments, their heterogeneity and their “articulation with other goals, resources, and constraints” (Sismondo, 1999; p.254) also makes them particularly well versed in applying to different domains. Actually, Sismondo (2000) argues that solid and successful models should be complex enough to “afford complex representational relations” (Sismondo, 2000; p. 254) and thereby travel among domains. This is a remarkable feature, which brought several scholars to understand models as ‘boundary objects’ (Star & Griesemer, 1989; Sismondo, 2000; Edwards, 2001; MacKenzie, 2014). One manifestation of this characteristic is the important role of models in interdisciplinary and transdisciplinary collaborations. Galison (1996) discusses how Monte Carlo simulations were extremely successful in coordinating the paradigmatic interdisciplinary ‘trading zone’ that coalesced during and immediately after World War II first around the development of the atomic bomb and then around the creation of modern meteorology. This was even more remarkable as each “pure mathematician, applied mathematician, physicist, bomb builder, statistician, numerical analyst, industrial chemist, numerical meteorologist, and fluid dynamicist” (Galison, 1996; p. 151) had a different view of Monte Carlo simulations and yet managed to communicate with their colleagues through a sort of pidgin language – which then, by the 1960s, had developed into a ‘fully fledged creole’. Mattila (2005) even describes modelling as an ‘object-oriented interdisciplinary activity’, the study of which is particularly suited to trace interdisciplinarity ‘in-the-making’. In an empirical study of an interdisciplinary collaboration aimed at developing a disease spread model, Mattila (2005) shows that the tensions and disputes stemming from the friction between different expertise ended up being incorporated into the final results as elements of the model – which are then described as 'carriers of interdisciplinarity'. It is interesting to put these accounts in conversation with the work of Schikowitz (2020), who instead found modelling to be an 'equalizing but exclusive integration machine' – meaning that tensions were obscured through modelling rather than carried over and valued. Clearly, if models are 'productive' in the interaction between different forms and styles of knowledge, *how* they are utilized and *how* productivity is articulated in different contexts matter (see 2.2.1.1).

2.3.2.2 Evaluating Models

"All models are wrong, but some are useful."

(Box, 1976)

Since models seem to possess a rather specific epistemology, a series of questions can be raised about them. When are models correct? When are they accurate (enough)? How far can they predict? A first attempt to make sense of the notion of 'validity' of models is that of adopting the philosophy of science framework of 'confirmation'. According to it, the object to be confirmed (in this case the model) remains in contention for truth as long as its predictions are not falsified by experimental results – but can never be established as 'true' (see Oreskes et al., 1994). This is because there could always be more than one model able to explain the available observations and it is always possible that future observations might call the original model into question. Yet, in the case of many models even this framework can hardly be applied, as their predictions cannot be properly tested against empirical observation – either because they are too far in time or because the available data is too sparse or incomplete. Here it is important to distinguish, with Oreskes (2003), between short-term and long-term predictions of models, or, in other words, between 'model-testing' and 'forecasting'. While the first, which can be used as a means to confirm a model, are extremely important as they can "provide a test of our understanding", the second "cannot be tested, and therefore do nothing to improve scientific understanding" (Oreskes, 2003; p. 24). Concerning the accuracy of models, the situation is equally riddled with challenges and trade-offs, again limiting our ability to confirm or validate models. In fact, models are affected by a 'complexity paradox', according to which "the more we strive for realism by incorporating as many as possible of the different processes and parameters that we believe to be operating in the system, the more difficult it is for us to know if our tests of the model are meaningful." (Oreskes, 2003; p. 19). This situation is compounded by another difficulty in producing ‘accurate’ models: as Edwards (2001) highlights, because of computational bottlenecks models cannot at the same time have a high resolution and a high complexity, forcing modelers to choose between accounting for many elements of the system or accounting for each of them in depth. Finally, all models embed uncertainties, regardless of their accuracy or their temporal scope. Oreskes (1998) categorizes uncertainty in modelling as ‘theoretical’ (aspects of the modelled system that are not fully understood), ‘empirical’ (aspects of the modelled system that are difficult or impossible to measure), parametrical’ (uncertainties arising from the simplifications needed to manage model input), and ‘temporal’ (uncertainties that arise from assuming the stability in time of the modelled system). For all these reasons, it does not make so much sense to try and establish the validity or correctness of a model with a ‘true’ or ‘false’ outcome, and it would be preferable to assess models qualitatively in terms of their usefulness (as Box, 1976, suggests in the opening quote of this section), or other desirable qualities they might share (Oreskes, 1998). Crucially, this shows the political dimension of modelling, as any form of qualitative judgement entails deliberation and debate and cannot be formulated in a purely ‘objective’ or numerical way.

2.3.3 Data

“On Exactitude in Science

…In that Empire, the Art of Cartography attained such Perfection that the map of a single Province occupied the entirety of a City, and the map of the Empire, the entirety of a Province. In time, those Unconscionable Maps no longer satisfied, and the Cartographers Guilds struck a Map of the Empire whose size was that of the Empire, and which coincided point for point with it. The following Generations, who were not so fond of the Study of Cartography as their Forebears had been, saw that that vast Map was Useless, and not without some Pitilessness was it, that they delivered it up to the Inclemencies of Sun and Winters. In the Deserts of the West, still today, there are Tattered Ruins of that Map, inhabited by Animals and Beggars; in all the Land there is no other Relic of the Disciplines of Geography.”

(Borges, 1960/1998)

While data is and has always been a central component in the scientific process, it is important to reflect on a few aspects that otherwise risk being taken for granted. For example: What bits and pieces of information or experience can qualify as ‘data’, and which as ‘good data’? How should one think about the relation between data and phenomena? And what role can and should data play in the scientific process? Besides trying to provide at least provisional answers to these questions, moreover, it is also extremely important to pause and appreciate the specific relations between ‘quantification’ and how data is collected, used, and understood. Indeed, the very first step of any quantification process, ‘capture’ in the language of Islam (2022), coincides with data collection, and the question of *how* quantitative data Is collected pertains to the second step, ‘specification’ (Islam, 2022). Moreover, if quantitative data is often considered more suited for scientific research than qualitative data, this is no doubt also in virtue of the ‘authority’ of quantification that I already discussed at length above. Importantly, the production of data itself heavily relies on quantitative methods, complicating the whole picture. For example, statistical methods, AI, and ‘intermediary models’ (Edwards, 2001) are often deployed to correct systemic errors in sampling and data collection (e.g. due to noise or a faulty calibration of measuring instrument) and models are also used directly to simulate ‘artificial’/’synthetics’ data sets based on historical data.

As I already hinted at above, all these dynamics have assumed even more relevance in the last decades, especially in relation to increased computational power and the availability of ever larger data sets – ‘big data’. These transformations have sent shockwaves through virtually every scientific field and have often been surrounded by glorifying and heavily promissory narratives (for a discussion of the promissory qualities of datafication, see Hoeyer, 2019; Lippert, 2022). In their wildest forms, such narratives have gone as far as heralding the death of the scientific method and the advent of a ‘fourth scientific paradigm’ based on data-centred or data-driven research (e.g, Anderson, 2008; Hey & Microsoft Research, 2009; Mayer-Schönberger & Cukier, 2013). While this process is for sure remarkable and has definitely engendered interesting developments in scientific practice, from an STS point of view it is important to carefully assess how it is unfolding and to eventually complicate the picture of its success. In this vein, Saltelli et al. (2025 have argued that the idea that more data will necessarily bring better knowledge and an improved ability of controlling natural and social systems is grounded in a ‘Cartesian dream’ that sees ‘nature as a machine’. The authors described this cosmological view as “a historical contingency arising from a mechanistic and reductionist view of science and human affairs, compressing the complexity and uncertainty of open systems” (Saltelli et al., 2025; p. 3). Ultimately, this idea can end up favouring technocratic forms of control (Lippert, 2022) by hiding the political dimensions of knowledge production (Lippert, 2022) and disrupting accountability in decision-making (Hoeyer, 2019). In order to contribute to these discussions, in Section 2.3.3.1, I introduce a conceptual framework to help make sense of the evasive notion of ‘data’ (Leonelli, 2015) and, in Section 2.3.3.2, I follow Leonelli (2014; 2025) for a critical analysis of the epistemology of ‘big data’.

2.3.3.1 Towards a Relational Understanding of Data

Leonelli (2025) distinguishes between two philosophical views on data, the 'representational view' and the 'relational view' (Leonelli, 2015; Leonelli, 2025). The first view "construes data as reliable representations of reality which are produced via the interaction between humans and the world" (Leonelli, 2025; section 4). In the 'representational view', data form a legitimate foundation to empirical knowledge in light of their faithfulness to reality and are objects with fixed and unchangeable content, revealing whose meaning is the ultimate goal of the scientific process. In the 'relational view', instead, any object can be considered data as long as "(1) it is treated as potential evidence for one or more claims about phenomena, and (2) it is possible to circulate it among individuals" (Leonelli, 2015; p. 6). Therefore, the 'relational view', instead of taking the fact that data is used as a form of scientific evidence to be a consequence of their ontological properties, flips the point of view to conceptualize data as precisely what can be used as scientific evidence – bypassing ontological questions all together. According to this definition, which is in line with the 'practice turn' in philosophy of science, data are first and foremost a tool for communication. For the purposes of my analysis, I find the 'relational view' more productive, especially in light of the theoretical sensibilities I will explain below (see 3).

Adopting a relational understanding of data encompasses a series of implications. First of all, if data are not necessarily representations of phenomena, they cannot have truth-value in and of themselves (Leonelli, 2015). Secondly, understanding data as what is gathered, stored and disseminated in order to be used as scientific evidence, also disqualifies any notion of 'raw data' and the very etymology of the term, which literally means 'that which is given'. On the contrary, according to a relation understanding, data are made. And since they are made for the very specific purpose of being received and understood by other scientists (or other actors), this also heavily informs *how* they are made. This explains the very well-known idea that data are 'theory-laden' (Boyd & Bogen, 2026), i.e. inevitably influenced by theoretical presuppositions (an idea that Edwards, 2001, turns around when talking about 'data-laden' models). Finally, the relational definition of data crucially brings attention to their 'portability' (see also, Daston, 1992), i.e. their ability to travel and to be communicated, which is "a crucial precondition for using data as evidence" (Leonelli, 2015; p. 7). Data, therefore, cannot be considered as abstract and fixed entities: they travel long and fascinating 'journeys' (Leonelli & Tempini, 2020), along which they are contextually and relationally reinterpreted. Moreover, they are made fungible in concrete and material realizations – characterized by properties like format and medium. Along these journeys, data also encounter what Edwards has termed 'data frictions' (Edwards, 2013), especially at the "interfaces between data 'surfaces': the points where data move between people, substrates, organizations, or machines" (Edwards et al., 2011; p.669). 'Data frictions' also compound to create 'science friction' (Edwards et al., 2013; p.669), i.e. the "difficulties encountered when two scientific disciplines working on related problems try to interoperate." (Edwards et al., 2013; p. 669). Incidentally, this notion extends the previous discussions about inter- and transdisciplinary collaborations, allowing to appreciate how data can engender specific challenges at the junction of different knowledge domains.

2.3.3.2 The Epistemology of Big Data

As Leonelli (2025) argues, we are currently witnessing a process of 'datafication'. This unfolds as ever more effective monitoring and recording of social life and nature generate an enormous digital footprint in the form of 'big data' – which Leonelli defines as "large datasets that are produced in a digital form and can be analyzed through computational tools" (Leonelli, 2025; section 1). Yet, quantity is not the only novelty presented by 'big data'. Even more importantly, Leonelli brings attention to "the prominence and status acquired by data as a scientific commodity [...], both within and beyond the sciences" and on the "methods, infrastructures, technologies and skills developed to handle data" (Leonelli, 2014; p. 1). What is the justification for such a strong push towards 'datafication'? Analyzing the work of Schönberger and Cukier (2013), Leonelli (2014) identifies two main features that the authors mobilize to justify the supposedly radical transformative potential of 'big data'. The first is their 'comprehensiveness', meaning that 'big data' are supposed to be so vast as to cover all the relevant information on any phenomenon of interest. An important corollary of 'comprehensiveness' is that issues of sampling and bias should suddenly become irrelevant (Leonelli, 2014). The second feature is that, while messy, 'big data' is comprehensive enough to allow for what Schönberger and Cukier (2013) name 'the triumph of correlation'. This is the idea that, in virtue of size and computational advancements, meaningful patterns can be extracted from 'big data' without necessarily a causal explanation for them. In this view, which Leonelli (2025) conceptualizes as 'Baconian' rather than 'Popperian', "procedures, techniques, methods, software and hardware are the prime motors of inquiry" (Leonelli, 2025; section 2) and theory leaves center-stage to data and empirics. Crucially, the focus is shifted from causal knowledge to predictive knowledge – or, in other words, from 'knowing-that' to 'knowing-how' (Cartwright, 2019). This move decentres the role of human agency and cognitive abilities in favour of an 'agnostic science' (Napoletani et al., 2011) underpinned by the opacity of 'algorithmic rationality' (Lowrie, 2017).

Through a study of big data in biology, though, Leonelli (2014) proceeds to criticize rather drastically the hyperbolic success story painted by Schönberger and Cukier (2013) – talking instead of a 'non-revolutionary' role of 'big data' in biology. First of all, adopting a relational understanding of data, Leonelli (2014) highlights the huge amount of labour that goes into the collection and assemblage of 'big data' – an aspect which is often bracketed away and overlooked in the move of constructing 'big data' as self-evident and unmediated. Moreover, the author also argues that there is an enormous gap between the totality of data produced and the portion of it that actually gets collected and disseminated, a distinction which entails many choices and trade-offs and engenders the risk of amplifying rather than reducing biases and inequalities. Moreover, Leonelli (2014) shows that biologists still care deeply about 'causation' and mostly use correlations as heuristics[10]. Overall, Leonelli (2025) finds 'big data' to engender a series of thorny ethical and epistemological issues, ranging from a 'data-divide' in terms of who has access to the production and fruition of data, to issues of bias, opacity and accountability. Indeed, the author concludes that in practice "access to data is fraught with conceptual, technical, legal and ethical implications; and even when access can be granted, it does not guarantee that the data can be fruitfully used to spur further research. Furthermore, the mathematical and computational tools developed to analyse big data are often opaque in their functioning and assumptions, leading to results whose scientific meaning and credibility may be difficult to assess." (Leonelli, 2025; section 10).

2.4 Dealing in Complexity: History, Organization and Epistemology of Complexity Science

"There are many thinkers, researchers, and even journalists who want the 'copyright' on complexity (and, of course, to take advantage of it). If we intend to develop a fruitful and powerful complex way of thinking, we have to face the people who pretend to be the 'owners' of the concept (some even look like priests of a new cult). They claim, a brilliant but insubstantial claim, that a 'complexity paradigm' or 'complexity theory' is possible."

(Najmanovic, 2002; p.85)

"We embrace a pioneering research approach: not only do we advance complexity science, but we apply it to problems that matter now."

(Complexity Science Hub, n.d.a)

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[10] The caveat is that the situation might be different in "other areas of social life [opposed to biology], particularly economics and politics, where the widespread use of patterns extracted from large datasets as evidence for decision-making is a relatively recent phenomenon" (Leonelli, 2014; p. 9).

While the previous two sections provide the necessary context and frame of reference for situating my empirical site, it is in this last section of the state of the art that I will finally come to the heart of my research interests: that loosely defined scientific community that is most commonly referred to as ‘complexity science’, or ‘complex systems science’ – to which the CSH openly ties its epistemological and institutional identity.

Just as much as complexity entails a series of multiplicities, tensions, and ambiguities (see1.2), it should also be no surprise that trying to pinpoint what it means to produce knowledge about complexity – trying to define a science of complexity – is a highly non-trivial and contested endeavour. In some basic sense, all scientific research is about understanding complexity and, maybe a bit simplistically, one could argue that modern science, after cosmologies and institutionalized religions, is nothing more than the latest approach we developed to collectively cope with uncertainty and reduce complexity (Nowotny, 2005). Luckily, though, my object of study is a bit narrower than that. ‘Complexity science’, the community to which the CSH ties its identity to, is indeed in no way synonym to all the sciences that deal with complexity, and the term possesses a much higher rhetorical concreteness. As a label, indeed, complexity science has by now been around for about four decades, coalescing around a rather vague but also somewhat coherent ensemble of objects of study and methodological approaches. In this time, ‘complexity science’ and ‘complexity thinking’ spread both within science and in the broader culture, and, in the process, began to put down rather solid institutional roots. Not only is the term more concrete, but there is also a certain sense in which complexity science represents something ‘unique’ with respect to the broader scientific landscape, instantiating “a divergence in form from, or lack of conformity with, certain modes of practice, institutionalization, and communication” (Williams, 2012; p. 4)[11]. In the face of this, if one tries to scratch the surface of complexity science’s superficial consistency, pinning it down precisely becomes immediately quite a challenging task: both from an epistemological and a sociological point of view. As Li Vigni (2021a; p. 31) put it, “we

[11] Importantly, Williams’ (2012) analysis is very careful in reconstructing this ‘uniqueness’ without investing it of ‘revolutionary’ qualities, as the boastful narratives of some complexity science scholars would suggest. In order to achieve this, the author introduces a specific notion of ‘difference’ (a combination of the commonsense notion, Derrida’s ‘différance’ and Deleuze’s ‘différence’ – Williams, 2012; p.4) to describe the relation between complexity science and its earlier filiations as well as its contemporary counterparts. Williams (2012) identifies three dimensions with respect to which complexity science needs to be understood as ‘different’ and dedicates a chapter to each: modelling (epistemology), building (organization and institutional configuration), and writing (communication).

face a paradox: if the boundaries of complexity [science] seem soft, undefined and open, its label has nevertheless a consolidated, acknowledged and clear identity".

In order to attempt to clarify this paradox and flesh out at least some of the historical (see 2.4.1), sociological (see 2.4.2), and epistemological (see 2.4.3) 'unique' characteristics of complexity science, I will base most of the following discussion on the works of Williams (2012) and Li Vigni (2020, 2021a, 2021b, 2021c, 2023). While exhaustively mapping complexity science in all its ramifications and flavours is a task well beyond the capacity of this state of the art – probably worth of a thesis of its own – I believe the analyses of these two authors to be sufficiently rich to provide a solid basis for my empirical work[12]. In part, moreover, the question of what complexity science is needs to be addressed empirically, and the CSH, with its specific socio-cultural and historical context, represents a novel location for its investigation.

2.4.1 With or without the Santa Fe Institute: A Brief History of Complexity Science

The history of complexity science, especially in its early years, is inextricably linked to that of its flagship institution: the Santa Fe Institute in New Mexico. Indeed, comparing complexity science to chaos theory, Williams (2012) found that while the latter emerged ubiquitously from the bottom up, complexity science "moved into view rather more from the top down" (Williams, 2012; p. 49), specifically at the SFI. Having the organization of a general science of complexity as its original core mission (Li Vigni, 2021a), the SFI went on to largely contribute to the dissemination and growth of complexity science (Li Vigni, 2021c) and to establish a strong influence, still hegemonic today, over the whole field.

The SFI, named Rio Grande Institute until 1985, was founded in 1984 by a group of senior physicists of the Los Alamos National Laboratory and other American universities (Williams, 2012; Li Vigni 2021a). Before its foundation, it was not clear that the new institute would be devoted to the study of complexity, an option which ended up being chosen because it could subsume all the other ones on the table – e.g. an institute for advanced computing or for information science (Williams, 2012). Between 1984 and 1994, the SFI went through its

[12] The work of Helmreich (1998, 2020) – which focuses on an area of research called artificial life – deserves at least a brief mention. Artificial life was particularly prominent in the first decades of complexity science and is based on the computer simulation of 'life' through the development self-replicating computer programs. This topic is less relevant for my discussion, especially because no one working at the CSH specializes on artificial life

‘Philosophical/Ambassadorial period’ (Williams, 2012), which was characterized by the attempt to create a coherent discipline – a ‘new universal science’ – and to develop a unified theory of complexity (TOC). During this phase, very ambitious plans were made, for example in terms of the size of the institute (supposed to arrive at a full capacity of up to 500 scholars). Additionally, a series of ‘legitimization strategies’[13] were deployed in order to manifest and give substance to complexity science (Li Vigni, 2021c): 1) the conscious and strategic use of the symbolic, social and economic capitals of the SFI founders’; 2) the mediatization of complexity science and the publication of popular books[14]; 3) the outreach for funding to federal agencies, foundations and big enterprises; 4) the publication of a series of SFI related scientific publications (including, for example, conference and workshop proceedings); 5) the organization of summer schools, secondary level pedagogical projects, and integrative workshops (which, characterized by an informal style and a strong focus on brainstorming and the connection of different areas of research will become a trademark of the institute); 6) the foundation of complexity-science-specific scientific journals.

By the mid-90s, though, the internal discussions about the exact meaning, scope and methodology of complexity science still had not been resolved, and the SFI also begun to be

[13] Li Vigni (2021c) uses this term to denote activities and initiatives that in Molyneux-Hodgson and Meyer (2009)’s language would correspond to both ‘movements’ and ‘community-making devices’. As already discussed in Section 2.2.1.2, these are fundamental ingredients of the stabilization of emerging disciplines.

[14] Especially during the first decade since the foundation of the SFI, mediatization, in general, and the publication of popular books, in particular, had a huge role in the construction and dissemination of complexity science. Thrift (1999), for example, argues that it was “outside science that complexity theory [was] most successfully propagated” (Thrift, 1999; p. 39), and delineates the geography of its circulation through three specific networks: global mediatized science, global business (‘the cultural circuit of capitalism’) and New Age practice. This resonates with the work of Williams (2012), who identifies the popular science book (e.g., Waldrop, 1993; Lewin, 1999; Gell-Mann, 1994) as a specific narrative device deployed strategically towards the popularization and performative construction of complexity science. In opposition to the ‘Sagan effect’, according to which the success of scientists in popularizing science is inversely proportional to their contribution in substantive research, Williams (2012) terms the paradigm in science communication brought forward by complexity science ‘Brockman moment’. This paradigm is named after the literary agent, John Brockman, who dealt with the majority of popular science books published by scientists working on complexity science.

criticized for the delivery of inconsistent results. A symbolic turning point came with the publication of a piece in Scientific American titled 'From Complexity to Perplexity' (Horgan, 1995), which accused both complexity science and the SFI of being excessively hyped. Initially, the SFI struggled to respond to this and other criticism and even faced a short period of severe financial and scientific crisis. In the end, though, a series of internal and external reorganizations brought the institute to evolve from "a place for great (often senior) minds to formulate a unified grand theory of complexity [to] an incubator of young talents" (Williams, 2012; p. 145). The 'legitimization strategies' that were initially thought as a way to make complexity science into a discipline finally concretized, instead, in the SFI's current institutional configuration as an 'institute without walls': a point of passage for scientists working on complex systems all over the world, renowned for its famous summer schools and integrative workshops, and a central node in a dense network of exchanges and collaborations (Williams, 2012). In the meanwhile, complexity science entered its second historical phase (Li Vigni, 2021c), which indeed extends from 1995 until today. During this period complexity science has slowly regained momentum and credibility, and the role of SFI has progressively become less central. In parallel, complexity science institutes and research projects have also begun to spread to Europe, Asia and Australia (in 2021, there were over sixty complexity institutes in the world – Li Vigni, 2021a). This phase has also been characterized by the rise of network theory and data science (Barabási, 2012), which partly took the spotlight from artificial life and the development or a general TOC as the main areas of research within the field.

Through this historical parabola, much of the initial ambitions of the founders of the SFI had to be cut back. Overall, the projects of institutionalizing complexity science and of developing general theory of complexity can both be said to have failed (Li Vigni, 2021c). Yet, complexity science clearly managed to find some other recipe to survive and thrive – "its specific socio-epistemic existence" (Li Vigni, 2021a; p. 32). This opens up the two questions I will address, respectively, in the next two sections. What is complexity science, if not an institutionalized discipline? Without a general theory of complexity, what is the epistemological glue holding the practice of complexity science together?

2.4.2 Stability Through Flexibility: The Porous Boundaries of Complexity Science

From a sociological and an organizational point of view, complexity science defies categorization. Indeed, the discussion of the disciplinary nature of complexity science is a recurring theme in the literature on the topic. Both Williams (2012) and Li Vigni (2021a), for example, came up with new terms to describe it – respectively 'quasi-discipline' and 'scientific platform'. Overall, both authors found complexity science to be characterized by flexibility and porousness. On one hand, the sociological uniqueness of complexity science seems to not be achieved by increasing specificity but rather "by refusing a common form of uniqueness" (Williams, 2012; p.182). On the other, complexity science manages to strike a balance between having defined boundaries and keeping them "more permeable than those of classical disciplines and specialties" (Li Vigni, 2021a; p.42). If complexity science is well-defined, it is largely in opposition to "disciplinary inertia or institutional conservatism" (Li Vigni, 2021a; p. 42)[15], and if complexity scientists do engage in some form of boundary work (Star & Griesemer, 1989) this is "rather more in the spirit of trying to get around boundaries rather than establish or reinforce them" (Williams, 2012; p.111).

2.4.2.1 Complexity Science as a Quasi-Discipline

The notion of 'quasi-discipline' is specifically introduced by Williams (2012) in order to capture the unique institutional and structural dimensions of complexity science and serves thereby a central role in his characterization of complexity science as 'different' (see note 11). Williams (2012) does not build this concept in opposition to other disciplinary categories but rather proposes that one of the main peculiarities of a 'quasi-discipline' is exactly its ability of "steering and slipping back and forth across multiple models of organization" (Williams, 2012; p. 182). Because of this intrinsic multiplicity, Williams (2012) does not provide a precise and succinct definition of 'quasi-discipline'. Instead, the author chooses to reconstruct it along four core elements – by and large, followed through the study of the SFI. The first element of complexity science as a quasi-discipline is its 'quasi-stable funding', i.e. its need to heavily rely on non-public funds in order to carry out scientific research[16]. According to Williams (2012), this predicament – which he describes as "pure science with market dynamics" (Williams, 2012; p. 153) – is a compounded consequence of the ambivalent disciplinary nature of complexity science and of the more general and historically determined withdrawal of public funding from scientific research. Other scholars, on the other hand, have suggested a much more critical reading of the connection between the SFI and the world of finance (see, Baker, 2022), arguing that the SFI "has

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[15] In the case of the emerging discipline of synthetic biology, Molyneux-Hodgson and Meyer (2009) found a similar tendency towards defining the field in opposition to existing disciplines (cfr. 2.2.1.2)

[16] This also resonates with Thrift's (1999) analysis, which recognizes the tight (material) connection between business and finance and complexity science, specifically the SFI. According to this author, this is also one of the reasons behind the wide circulation of complexity ideas in the 'cultural circuit of capitalism'. An extremely detailed history of the connections between the SFI and finance is given by Baker (2022).

acted as an important node in a network of elite actors in business, politics, and media working to construct a new leadership class [:] the Santa Fe Institute libertarians" (Baker, 2022; p. 50)[17]. The second element of complexity science as a quasi-discipline, labelled 'rhetoric and paradox', points to the tension of constructing complexity science as scientific and credible "whilst sustaining difference from the rest of science" (Williams, 2012; p.160)[18]. This tension, source of endless discussions at the SFI and beyond about what complexity science really is, needs to be understood, according to Williams, not as an obstacle to the construction of complexity science, but rather as a constitutive part of its flexible identity. The third element of complexity science as a quasi-discipline is termed by Williams (2012) 'dissemination, organization, and porosity'. Through this element, Williams (2012) refers to the unique composition of complexity science, the surprising width of reach of complexity science publications, and to the flexibility complexity science affords scientists to "publish one week in finance, and the next week in statistical physics" (Williams, 2012; p. 165). In this regard, Williams further argues that scientists working in complexity science have more diverse backgrounds and more complicated trajectories than it is the case within traditional disciplines. Finally, the last element of complexity science as a quasi-discipline is represented by its 'epistemic variations', i.e. the multiple interpretations of complexity within complexity science (see note 20 below). Overall, according to Williams (2012), conceptualizing complexity science as a quasi-discipline means first and foremost to recognize slippage and indefinability as two of the main principles of its institutional configuration. In other words, from a sociological and structural point of view, while complexity science is characterized by a series of specific tensions and ambivalences, it is its meta-attitude to navigate all of these tensions in a flexible fashion to distinguish it as 'different' and unique – "it is *precisely* this slippage between opposites that animates SFI as an institution, and which makes something like complexity science possible" (Williams, 2012; p. 152).

[17] A somewhat troubling aspect of the connection of the SFI with the financial world is provided by its ties with Jeffrey Epstein. The infamous financier was indeed among the funders of the SFI, having donated around 275 thousand of dollars to the institute over the course of his life (Baker, 2022). Moreover, Epstein was very close to John Brockman (see, footnote 14) – who has been defined by commentators as his 'intellectual enabler' (Morozov, 2019) – and several scientists working at the SFI. For example, Murray Gell-Mann was a regular attendee of Epstein's parties (Ward, 2003) and the financier is even thanked in the acknowledgements of the first edition of 'The Quark and the Jaguar' (Gell-Mann, 1994).

[18] Note the similarity with the 'structural essential tension' described by Turner et al. (2015; cfr. 2.2.1.1).

2.4.2.2 Complexity Science as a Scientific Platform

Li Vigni's concept of 'scientific platform' (Li Vigni, 2021a) has many elements in common with Williams' (2012) 'quasi-discipline'. Indeed, flexibility is a key component of the very definition of 'scientific platform': "a meeting point between different specialties, which, on the basis of a flexible common ground, pursue together shared or parallel socio-epistemic objectives" (Li Vigni, 2021a; p.30). Yet, the notion of 'scientific platform' also diverges from that of 'quasi-discipline' in some respects. Mostly, this is because Li Vigni's (2021a) analysis is more concerned about the political implications of the "fuzzy boundaries and institutional weakness" (Li Vigni, 2021a; p. 43) of complexity science. Where Williams (2012) explicitly remarks that "sometime contingencies pose the dominant constraint" (Williams, 2012; p. 153) in the determination of complexity as a 'quasi-discipline', Li Vigni's (2021a) reading of the flexibility of complexity science is a more teleological one – as showcased by the second part of the definition of 'scientific platform'. Since this author's point of view is rooted in a more agonistic understanding of scientific practice and is underpinned by an empirical approach that focuses on complexity scientists' self-perception, Li Vigni emphasizes the strategic affordances of the flexibility of complexity. That is, complexity science as a 'scientific platform' allows scientists to 'ally', 'mutualize resources', and 'increase collective legitimacy', functioning as a sort of "trampoline to carry on different kinds of struggle in the academic field at large" (Li Vigni, 2021a; p. 42). A good exemplification of the strategic deployment of the label of complexity science is given by the fact that it is overwhelmingly used by scientific entrepreneurs "who actively [pursue] the creation of funds and institutions for the development of complexity sciences as such" (Li Vigni, 2021a; p.36). On the ground, instead, scientists keep their disciplinary identity tied to other more established fields[19], and either use the plural to refer the field (complexity sciences) – in which case they still adhere to it only "intermittently or without a full engagement" (Li Vigni, 2021a; p.36) – or even avoid the construct all together. In sum, Li Vigni's (2021a) analysis extends the work of Williams (2012) by showing how scientists make sense of and take advantage of the structural and institutional flexibility of complexity science.

[19] Again, this is also very similar to what Molyneux-Hodgson and Meyer (2009) found in the case of the emerging discipline of synthetic biology (cfr. 2.2.1.2). Also recall that 'juggling multiple identities' is part of the navigation of the 'affective essential tension' in interdisciplinary collaborations according to Turner et al. (2015; cfr. 2.2.1.1)

2.4.3 Epistemology: Defining and Knowing Complex Systems

Largely, the epistemology of complexity science can be parsed in terms of difference (*sensu* Williams, 2012), slippage and ambiguity just as well as its organizational and institutional structure. Nevertheless, there is a crucial caveat: the otherwise extremely fluid nature of complexity science, which also includes some of its epistemological characteristics, hits a wall when it comes to methodology. Indeed, that of complexity science is a purely quantitative epistemological project – at least in the hegemonic current associated with the SFI – and if its approach to knowledge production is multiple and different, this is only within the scope of a certain range of quantitative and data-intensive methods – largely overlapping with the repertoire of physics, computer science, data analysis, and mathematics. This is a crucial point, with ramified and far-reaching consequences. As I shall discuss, this is also the pivotal argument in Li Vigni's (2023) critique of complexity science, which the author develops through the comparison between the 'complexity practice' – "what complex systems scientists actually do" (Li Vigni, 2023; p. 469) – and the 'complexity discourse' – based on 1) the rejection of linear causality and classical determinism, 2) on a "rebellion against the excesses of reductionism" (Gell-Mann, 1994; p.119), 3) on holism as a key to understand complex systems, and 4) on interdisciplinarity as a main *'modus operandi'* (Li Vigni, 2023; 466-468). While the 'complexity discourse', taken at face value, invests complexity science of a rather revolutionary quality with respect to traditional science, the 'complexity practice' brings instead Li Vigni (2023) to conclude that "this field of research rather renovates and even radicalizes the classical science approach by extending it to new study objects" (Li Vigni, 2023; p. 469). Attempting to corroborate this analysis and better expound upon its meaning, I break my discussion into two parts: first, in Section 2.4.3.1, I review how complexity science constructs its objects of study; then, in Section 2.4.3.2, I touch upon the methodological approaches deployed within the field and on their scope.

2.4.3.1 The Power of Definitions: The Objects of Study of Complexity Science

The first step to understanding the epistemology of complexity science is to try to pin down how it defines its object of study: complexity. As discussed in Section 1.2, the term is so charged and ambiguous that only within individual scientific communities is it possible to hope to have some sort of unified definition – an expectation maybe more justified with complexity science. Indeed, if not a close univocal definition or interpretation[20], complexity science does possess a rather

[20]In fact, even within complexity science, even within the SFI, interpretations of the meaning of complexity can vary widely, both at a definitional (see e.g., Lloyd, 2019) and at a philosophical level. Discrepancies at

coherent, unified and articulated vocabulary to talk about complexity and complex systems – a specific and rather powerful jargon[21]. For example, complexity is often framed as a matter of 'emergence' (i.e. collective properties of systems that defy reductionist explanations) and 'non-linearity', and complex systems as systems with the capacity of 'self-organizing' at the 'edge of chaos'. Yet, if this language helps in defining complex systems, it does so only by providing broad coordinates within which various but similar specifications can be constructed. Having a look at such definitions (e.g., see **Box 2**), two rather universal characteristics emerge: on one hand, they always remain extremely broad, sufficiently to include virtually any macro-system from societies to the climate, from the internet to a human body; on the other, they are always expressed in a mathematical and physicalist language. That is, while narrow in terms of perspective and register, all these definitions keep flexibility and multiplicity in terms of referents. This, in line with the characteristics of a 'quasi-discipline' and of a 'scientific platform', allows for an enormous scope in terms of topics that can be approached and an enormous reach in terms of the scientists and disciplines that can be enrolled. Li Vigni (2021a; p. 37) captures this feature by affirming that "the vagueness of the term 'complex systems' is one of the glues that keep [complexity science] together, [as the] concept remains sufficiently general to justify the copresence of very diverse researchers in the same place (be it an institute, a research program, a workshop or other)". Yet, if virtually any discipline can be enrolled under the banner of complexity science, how does this unfold in practice?

2.4.3.2 'Unity without Uniformity and Diversity without Fragmentation'? The Scope and Methodology of Complexity Science

"There is no doubt that the mathematical-formal approach remains the heart of the theoretical elaborations of the science of complexity. And this is precisely where the ambiguity lies: since, in this way, the science of complexity runs the risk of becoming an updated version of

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the philosophical level are the ones that Williams (2012) terms 'epistemic variations' and identifies, together with the slippage between them, as the fourth element of a quasi-discipline (cfr. 2.4.2.1).

[21] Thrift (1999) identifies the power of the language of complexity science in its metaphorical and indefinite quality, which allows "many meanings to be enlivened by [it]" (Thrift, 1999; p. 36) and to travel across different domains – something which very much resonates with Sismondo's (2000) words on models (cfr. 2.3.2.1)

reductionism, that is, of the attempt to subordinate every aspect of reality to the same interpretative key."

(Israel, 2005; p. 505)

If one is looking for a common denominator of the 'complexity practice', this has to be looked for in its methodological approach. For example, the vast scientometric study of complexity science carried out by Grauwin et al. (2012) shows that the epistemology of complexity science is unified by its tools: the computer and the computational techniques it deploys. Yet, while 'complexity practice' has been bound up with modelling and simulations right from its early days (see Helmreich, 1998; 2020; Williams, 2012), this has always been in non-trivial, multiple and evolving terms. On one hand, the deep transformations of the last decades in the availability of data, in knowledge infrastructures, and in computational power (see 2.3.3) have affected the field at its core – opening up many new possibilities in terms of levels and types of modellable complexity. On the other hand, complexity poses unique challenges to modelling, which in turn shape and, as I shall argue, diffract the modelling practices in complexity science. In particular, when working with complex systems, prediction and forecast – at least in their classical sense – become a practical impossibility, regardless of the data and computational power available (see 2.3.2.2). As Williams (2012) put it, in complexity science, and in chaos theory before, "the epistemological 'point' of modelling [...] morphed from a predictive to an exploratory register" (Williams, 2012; p. 55). In light of this, in complexity science classical epistemological categories like those of 'error', 'accuracy', 'theory', and 'experiment' are the object of contextual and plastic re-interpretations – evading universally accepted criteria and procedures. From this perspective, complexity science can be said to display a sort of epistemological 'unity without uniformity': strongly entwined with computer modelling but in a markedly flexible and multiple fashion. Arguably, this feature is functional to and afforded by the high level of institutional flexibility and disciplinary porousness of complexity science. This interpretation of complexity science's epistemology is corroborated by the work of Li Vigni (2020; 2021b), which identifies a wealth of sub-currents and sub-communities within the field. Namely: 1) five distinct 'anticipation communities'[22] (Granjou, 2016; Li Vigni, 2020) – each accounting for a different approach to prediction, embodying different normative views, and engaging in different extra-academic activities; 2) five distinct 'epistemic argumentative regimes' (Li Vigni, 2021b) – distinguishing the different ways in which "complexity

[22]Overall, Li Vigni (2020) finds the attitude of complexity science to be ambivalent towards prediction and forecast, as on one hand complexity and uncertainty pose limit to prediction power, but on the other many practitioners engage with future on an almost everyday basis.

scientists argue the credibility of their research" (Li Vigni, 2021b; p.62); 3) five distinct 'ontological views' (Li Vigni, 2021b) – each corresponding to a different imagined relation between models and reality; and 4) three distinct 'regimes of evidence' (Li Vigni, 2021b) – each resulting from a specific combination of 'epistemic argumentative regime' and 'ontological view' and capturing a different way "complexity scientists interface with policy makers and civil society"(Li Vigni, 2021b; p. 90)[23].

Nevertheless, even with all the multiplicity accounted for, the inquiry tools and the epistemic registers deployed in complexity science all belong to a narrow repertoire – predominantly informed by physics, mathematics, and computer science, and thereby almost exclusively quantitative. This is, ultimately, the mould through which the totality of 'complexity practice' goes through. This caveat is actually a fundamental feature of the epistemology of complexity science, and it has pretty important, and from some perspectives devasting, consequences. Indeed, several scholars (Li Vigni, 2023; Talbott, 2001; 2002; Israel, 2005) agree that the form of interdisciplinarity brought forward by complexity science is flawed by strong asymmetries and power imbalances, with "some disciplines [being] underrepresented in complexity institutional spaces while a few impose their epistemic and ontological frameworks to the others" (Li Vigni, 2023; p. 485). According to Li Vigni (2023), this predicament has its roots in the very inception of complexity science, which, specifically at the SFI, was devised from the outset as a project revolving around "the search for general theories and the mission of formalizing the 'soft' sciences" (Li Vigni, 2023; p. 489). In sum, Li Vigni (2023) expresses his critique of the interdisciplinary practice within complexity science by describing the field as holding an 'imperialistic stance' towards the other disciplines it aims to enroll. Using this as a stepping stone and building on the works of Talbott (2001; 2002) and Israel (2005), Li Vigni (2023) goes on to develop a broader critique of complexity science, arguing that 'complexity practice' also belies all the other elements of 'complexity discourse' beyond interdisciplinarity (anti-reductionism, holism, and rejection of classical determinism). Overall, then, if the epistemology of complexity science can be parsed in terms of 'unity without uniformity', it is at the same time important to acknowledge that it seems to lack diversity all together. Talbott (2002; p.22) expressed this by observing that the "unity being sought [in complexity science] is an emptied unity — the unity that

[23] In this second work, Li Vigni (2021b) attempts to understand how complexity scientists assess their inquiry methods and results as valuable. Overall, beside finding out a rich multiplicity of practices and evaluation registers, Li Vigni (2021b) also finds a remarkable continuity between complexity science and its subdisciplines (cfr. Li Vigni, 2021b; Table 1 and Table 2, pp. 81-89) – "disconfirm[ing] the alleged unity of complexity [science] and minimize[ing] the claim of its radical novelty".

comes from the one-sided urge to strip away differences and refuse to consider them. [...] By contrast, a true unity arises when we recognize differences while at the same time bringing those very differences into meaningful relationship — an essentially qualitative undertaking".

History	Institutional/organizational dimension	Epistemology
Ambassadorial period (1985-1995): strong connection with SFI, attempt to develop a unified theory of complexity and institutionalize complexity science as a discipline **Second phase (1995-today):** decentralization of SFI, complexity science research projects and institutes across the world, data science and network theory as new and central areas of study	**Quasi-discipline** (Williams): quasi-stable funding, tension between being credible and being innovative, disciplinary porosity, slippage and indefinability **Scientific platform** (Li Vigni) "a meeting point between different specialties, which, on the basis of a flexible common ground, pursue together shared or parallel socio-epistemic objectives": flexibility, fuzzy boundaries, institutional weakness, strategically deployed by scientists, ambiguous feelings of disciplinary belonging	**Definition of complexity:** narrow in terms of perspective and register, flexible and multiple in terms of referents **Unifying elements:** computer, computational techniques, modelling as explorative (not predictive), mathematical and physicalist language **Multiplicity:** multiple understandings of accuracy, error, experiment; several anticipation communities, epistemic argumentative regimes, ontological views, and regimes of evidence (Li Vigni) **Asymmetry in interdisciplinary practice, re-entrenchment of reductionism and determinism:** aim to formalize 'soft sciences', imposition of epistemic and ontological frameworks of a few disciplines over the others (imperialistic stance)

Table 3 *Schematic representation of the most salient characteristics of complexity science according to Williams (2012) and Li Vigni (2020, 2021a, 2021b, 2021c, 2023).*

3 Theoretical Framing

So far, I have attempted to map and discuss the academic literature I find most relevant for my research and, in parallel, to put together and refine a sort of theoretical vocabulary – a conceptual toolbox, if you will. Now, a final piece is missing to complete the picture of what I understand as the background and backbone of my work: namely, the overarching theoretical framing that informed and oriented my exploration of the CSH. In this section, therefore, I aim to fill in this gap by introducing ‘epistemic living spaces’ (Felt, 2009; Felt & Fochler, 2012; 3.1) and Valuation Studies (3.2) and by explaining their relevance to my analysis. Before diving into details, though, let me say a few words about the general sensibility in which my other theoretical choices are rooted. In a nutshell, I follow an approach that in sociology and anthropology is sometimes referred to as material semiotics (for an overview from the STS literature, see Law, 2008; for a take from anthropology, see Wolf, 2001).

An attempt to mediate between materialism and symbolism, material semiotics understands social reality as constructed through an ongoing exchange between materiality and practices, on one side, and meaning and ‘culture’, on the other. In fact, one of the main points of such a framework is to destabilize the hard distinction between these two poles[24], arguing that both should be understood in relational rather than ontological terms. Thereby, material semiotics also provides a way to navigate different scales and levels of analysis, suggesting that macro constructs (e.g., nation states, ideology, progress, and power – and, more relevantly to my work, ‘academic capitalism’, transdisciplinarity, the authority of quantification, and, at least to some extent, complexity itself) should be understood as relational “effects rather explanatory foundations” (Law, 2008; p. 147): as the outcomes of the sedimentation and accretion of the micro and the material, rather than as “somewhat stable agents or frameworks” (Law, 2008; p. 148) existing at a different level than social reality and thereby ordering it according to their rationales. At the same time, though, this point of view does not imply that “all forms and aspects of a ‘culture’ are semiotically equal” (Wolf, 2001; p. 378), nor that power or asymmetry do not exist or have no effects in the world. Therefore, as social scientists, we are still urged to “look for the ways in which chains of signification laden with power come to overlay, dominate, ‘enchain’ other signifiers and signifieds; [...] for the effects of such dominance, as well as for any efforts to counter and resist it” (Wolf, 2001; p. 379).

Embedding these discussions into my research project means approaching complexity science and the CSH as the provisional and meta-stable result of an ongoing work of negotiation and meaning making which involves and reconfigures the most disparate set of actors and practices – from scientists to modelling techniques, from computers to funding applications. It means, at the same time, appreciating complexity science and the CSH as participating in a broader socio-

[24] As well as a series of other dichotomies that characterize modern thinking, including nature and culture, social and technical, human and non-human, micro and macro (cfr. Law, 2008).

cultural context and possibly being influenced, contributing to, reinterpreting or resisting its logics – its 'chains of signification laden with power', in Wolf's language. More concretely, and this is what I have tried to operationalize through both my theoretical and my methodological choices (see 5), an overarching material semiotics sensibility fits well with an approach that gives space to the practices and perceptions of individual scientists and thereby tries to trace how meaning and values are enacted and negotiated in the specific configuration given by the complexity science community at the CSH.

3.1 Epistemic Living Spaces

Epistemic living spaces (Felt, 2009; Felt & Fochler, 2012) are defined as researchers' individual or collective perceptions and narrative reconstructions of "the multi-dimensional structures – symbolic, social, intellectual, temporal and material – which mould, guide and delimit in more or less subtle ways researchers' (inter)actions, what they aim to know, the degrees of agency they have and how they can produce knowledge" (Felt, 2009; p. 19). As such, this concept invites the adoption of a person-centred approach to the empirical study of scientific practice, one that foregrounds researchers and investigates how "they inhabit different cognitive and material landscapes and participate in giving shape to them, and how they organise their social, spatial or temporal environments and are organised by them" (Felt, 2009; p. 18). The focus on individual and collective perceptions of life in academia vis-à-vis the structures that both enable it and constrain it makes the concept of epistemic living spaces particularly suited to investigating the tensions that many scientists experience between the personal and the systemic level. Indeed, 'epistemic living spaces' also bring attention to the 'orientation work' and 'boundary work' that scientists engage in in order to navigate such tensions – the first relating "to the activity of making sense of the shape, the boundaries and the central logics governing their own epistemic living space" (Felt & Fochler, 2012; p. 7) and the latter to the efforts finalized to "re-shap[ing] their epistemic living space to fit better with their own expectations, or to resist shifts and changes that are perceived as negative" (Felt & Fochler, 2012; p. 7).

Crucially, the concept of epistemic living spaces does not only focus on epistemic elements of scientific research, but instead "addresses the intertwinedness of the personal, the institutional, the epistemic, the symbolic and the political" (Felt, 2009; p. 19). This feature is articulated through the very definition of epistemic living spaces, which conceptualizes them as composed of five dimensions: 1) the epistemic dimension, accounting for the "assumptions about which kinds of research questions are central, how knowledge should be produced, and which properties and procedures constitute good knowledge" (Felt & Fochler, 2012) the material and spatial dimension, relating to artefacts, material resources, and location (both physical and symbolic) and their relation to scientific practice; 3) the temporal dimension, including how scientists perceive time and rhythm and how they relate to past, present and future; 4) the symbolic dimension, touching "on the values and modes of ordering seen as crucial in governing research work" (Felt & Fochler, 2012; p. 6); and 5) the social dimension, articulated through the "forms of togetherness" (Felt & Fochler, 2012; p.6) that frame and shape research and, more broadly, life in academia. In this regard, the concept of epistemic living spaces somewhat resonates with that of 'epistemic cultures' (Knorr-Cetina, 1999) – but also extends it insofar as it allows to better keep track of aspects like "the broader framing of research through policy discourse and practice, the societal imaginaries that penetrate the research world, the changes in the monitoring and assessment practices and many more institutional aspects and normative imaginaries, such as mobility, speed, excellence" (Felt, 2009; p. 20). For the purpose of my analysis, moreover, the choice of adopting epistemic living spaces over 'epistemic cultures' is further motivated by the fact that the latter, which are very well suited for the study of stable cultural assemblages, would not capture well the fluidity and flexibility of complexity science (see 2.4), nor the ongoing work of contestation and negotiation between the personal and the systemic – which instead I find particularly relevant and interesting.

By adopting the lenses of epistemic living spaces, I also build on the work of other scholars (e.g., Felt & Fochler, 2012; Fochler, 2016a; Fochler & Sigl, 2018; Davies, 2020; Bieszczad et al., 2025), who have already used this concept to investigate how the broader contemporary shifts in the relation between science and society (see 2.2) "affect the actual cultures and practices of research, and how researchers perceive their work and their own identity" (Felt & Fochler, 2012; p. 4). In particular, I find the CSH to be a particularly good case study to further the analyses of Fochler (2016a) and Fochler and Sigl (2018) – who have used epistemic living spaces to trace how differences in institutional spaces of knowledge production are perceived and navigated by scientists – and of Bieszczad et al. (2025) – who have instead explored how epistemic living spaces shape the ways in which scientists articulate the societal relevance of their research (see2.2.2.2). In comparison to overarching macro analyses like Mode 2 Science and Post-Normal Science, this approach had been surprisingly underrepresented until the strain of research that I attempt to contribute to started to follow it (Felt & Fochler, 2012). This is quite surprising, as this approach fits extremely well with the material semiotics sensibilities present in STS and informing this thesis. In fact, it directly connects with the longer STS line of research that, as already discussed in Section 2.1, attempts to arrive at a "fine-grained meaning [of] what the 'disunity of science'

[Galison & Stump, 1996; Hacking, 1992b] might actually mean in every-day research contexts" (Felt, 2009; p. 20).

3.2 Valuation Studies

Valuation studies is a conceptual approach that draws together several disciplines and sensibilities in order to study social practices "where the value or values of something is established, assessed, negotiated, provoked, maintained, constructed and/or contested" (Valuation Studies Board of Editors, 2020; p.1). Importantly, value here is intended in a broader sense than strictly monetary or economic. Moreover, in this definition it is valuation (as a practice) to be foregrounded over value (as a noun). Indeed, partly diverging from classical works in social sciences, the focus of valuation studies is neither on values in abstract – intended as something individuals might hold as elements of their ideology and that might guide their actions and choices – nor on the normative assessment of worth. Rather, valuation studies adopt a relational understanding of value and focus on how value is constructed and negotiated through practices of valuation (see, Asdal et al., 2024; Lamont, 2012). Formulated in these terms, this research direction is relatively new, at least in the sense that it is only since the early 2010s that previously sparse and uncoordinated research efforts began to coalesce around a coherent sensibility and language (see, Asdal et al., 2024; Lamont, 2012). Since then, the community of scholars interested in these topics and approaches kept growing, with many advancements and contributions both in empirical and in theoretical terms. Besides the influence of material semiotics and ANT (see, 3), valuation studies takes inspiration from the work of American and French pragmatists (see, respectively, Dewey, 1939; Boltanski & Thévenot, 2006) and has ties with other areas of research within STS – most notably, for the purposes of my work, sociology of quantification (see 2.3.1).

While there are different ways to indicate this area of studies, I have chosen the term 'valuation' to refer to something which is broader than 'evaluation' and also encompasses 'valorisation' (see, Lamont, 2012; Heuts and Mol, 2013; Vatin, 2013). This choice emphasizes that valuations often do not happen 'from a distance' and that valuators and valuees affect each other through complicated and bidirectional interactions. Moreover, this choice is rooted in a conceptualization of valuation that is as much concerned with the assessment of worth as it is with the performance and enactment of worth. Valuation, finally, does not only refer to the fact that formulating a judgement about something adds or subtracts value to it – 'valorises' it – but also to those instances in which valuation implies an active transformation of its object (Heuts & Mol, 2013). Based off this understanding, valuation studies are concerned with a wide array of practices, often embedded within and instrumental for other activities. This includes, of course, the valuation of scientific work by scientists working at the CSH. In the following three sections, I will expound upon the arguments and findings from the valuation studies literature that I find more relevant for my research (see 3.2.1; 3.2.2). Informed by this discussion, I then move on to define the valuation studies conceptual tools that I will use in my work and to explain how my thesis is positioned with respect to existing literature (see 3.2.3).

3.2.1 Valuation Situations: Evaluative Principles, Hierarchies and Heterarchies

The multiplicity and situatedness of practices of valuation bring attention to 'valuation situations' or 'valuation moments' (Antal et al., 2015): specific instances in which something is being valued. In order to make sense of the messiness and tensions that characterize such 'situations', scholars have introduced a nuanced vocabulary and a rich conceptual toolbox. The language of 'valuation constellations' (Waibel et al., 2021), for example, is very helpful to unpack the different 'positions' ('valuators', 'valuee' and 'audience') and 'relations' ('valuation' and 'observation') that compose any valuation process. Moreover, in the literature a variety of terms have been used to indicate the logics that shape valuation situations – i.e. those principles or registers that actors refer to in order to perform a valuation or make decisions within a valuation context. While Boltanski and Thévenot (2006) proposed to deductively classify actors' choices in valuation processes through six predetermined 'orders of worth', Stark (2009) and Heuts and Mol (2013) have argued for the empirical and contextual reconstruction of 'evaluative principles' and 'registers of valuation', respectively. For the purpose of my work, I will stick to the latter approach and to the term 'evaluative principle' (Stark, 2009; see also Fochler et al., 2016).

Another important distinction that will be key for my analysis is that between hierarchical and heterarchical contexts of valuations (Asdal et al, 2024; Lamont, 2012; Stark, 2009). While the first describes a situation in which one 'evaluative principle' dominates over the others, the latter denotes instead a more balanced distribution of authority among different 'evaluative principles'. Many scholars have observed that heterarchical settings might be preferable to hierarchical ones. Hall and Lamont (2013), for example, have argued that the coexistence of multiple registers of evaluation is an important factor to increase social resilience. Stark (2009), on the other hand, who studied heterarchical settings in organizational contexts, has argued that they facilitate cognitive work and exploration. The distinction between hierarchical and heterarchical valuation is particularly relevant for any analysis of scientific valuation, since the narrowing of evaluative repertoires can be detrimental to the fruitful practice of interdisciplinary and transdisciplinary research, and, in general, to life in academia (see, Felt et al. 2013; Felt et al., 2016; Fochler et al., 2016; 2.2; 2.2.2).

3.2.2 The Stabilization of Valuations: Valuation Regimes

While always situated, valuations are also influenced by overarching factors, both material and semiotic (Asdal et al., 2024; Lamont, 2012). What is more, valuations are often interlocking, so that judgements formulated in one context can 'travel' easily to others (Waibel et al., 2021). It is through these systemic elements that valuations manage to remain stable across valuation situations. And it is exactly in virtue of such overarching factors that asymmetries between different logics grow and become 'durable', as specific 'evaluative principles' come to be laden with power more than others (see also the discussion in Section 2.4). In order to capture such dynamics, valuation studies scholars have introduced several concepts. Inspired by ANT, some have brought attention to the 'tools' and 'devices' involved in valuations (Asdal et al., 2024; Doganova, 2019; Karpik, 2010), while others have instead emphasized the role of 'rules' and 'infrastructures' (Lamont, 2012; Waibel et al., 2021). An important point reiterated in the literature is that each and every one of these components ('tools', 'devices', 'rules', 'infrastructure') is not neutral, but it is rather inscribed with specific standards and logics. While this can facilitate valuation in many instances, it also makes it narrower, less robust, and less transparent – especially if such mechanisms are implicit or black-boxed.

To refer to the totality of the stabilizing forces that influence a certain category of valuation situations, furthermore, authors have proposed slightly different concepts. For example, in the context of financial markets, MacKenzie and Spears (2014; see 2.3.2) talk about 'valuation cultures'. In my work, though, I have decided to stick to the notion of 'valuation regimes' (Fochler et al., 2016) – which "point[s] to the broader discursive, material and institutional background [that] concrete evaluation draws on" (Fochler et al., 2016; p.180). Thereby, valuation regimes can be understood as laying out the conditions and landscape inside of which concrete valuation situations unfold. Of course, just as specific valuation situations can be either hierarchical or heterarchical, the same applies to valuation regimes in their entirety. As described by Fochler et al. (2016), importantly, valuation regimes not only include discourse, practices and infrastructures, but also the people that live within them, their compliance and their resistance. Defined in these terms, the analytical lens of valuation regimes is particularly well-suited to study valuation practices within specific epistemic living spaces (see 3.1) – in my case, the CSH.

3.2.3 Valuation Studies in my Research

In my work, I decided to adopt a valuation studies approach in order to empirically reconstruct the situated logics (i.e. the 'evaluative principles') and the overarching forces (i.e. the manifestations of specific 'valuation regimes') that shape scientists' practices at the CSH. In doing so, I assumed actors to always strive in their work for something they perceive as 'good science', at least at an ideal level, and in the worst case to settle for something they at least would describe as 'good enough science'. In other words, for me the deployment of a valuation studies perspective is rooted in the conviction that most people act in good faith and need to justify their actions to themselves and others. Of course, this does not mean that I believe everyone to always do 'good'. What I am saying is simply that I believe everyone to act under the conviction of doing 'good' and that it is in light of this that the question of how we valuate, both individually and collectively, becomes of extreme relevance. From this perspective, issues concerning valuation are also key to understanding how certain practices are more common than others, how power and asymmetries become entrenched, and, generally, how reality unfolds. Overall, I find this kind of thinking much more rich, interesting and nuanced than arguments that try to explain away social reality (and especially its eventual shortcomings) as the result of evil or scheming, or as the all-defeating march of capitalism, positivism, imperialism and what not. In line with a material semiotic sensibility (see 3), I believe the study of valuation practices to bring us a step closer to explain the existence and consistence of such macro-constructs – not the other way around.

In taking this route, my work directly owes to pre-existing literature. First, with respect to the conceptualization of 'good science', I follow Asdal et al. (2023) and Heuts and Mol (2013) – who have respectively focused on the concepts of 'good economy' and 'good tomatoes'. The 'good economy' is an analytical tool used by the authors to investigate "how economic practice is entangled in versions of the good" (Asdal et al., 2023; p. 1) and put the finger on the ethical and normative dimensions entailed in the contraposition between competing economical paradigms. Heuts and Mol (2013), on the other hand, provide a compelling account of how different experts go about their valuation of tomatoes. In Heuts and Mol's (2013) analysis, valuation is an eminently messy process, which, even in the most mundane of its realizations, is characterized by frictions and inconsistencies between different 'registers of valuing'. Crucially, Heuts and Mol (2013) stress that valuation is a process that does not guarantee control nor success. In simple terms: searching for 'good tomatoes' does not guarantee that external constraints or other unsolvable tensions will not jeopardize the goodness of tomatoes. To make sense of this discrepancy, the two authors mobilize the framework of 'care' and argue that "caring is an activity in which valuing is implied" (Heuts & Mol, 2013; p. 130). From their point of view, 'good tomatoes' are not given and need to be developed and nurtured through an ongoing and open-ended engagement that can be compared to an act of care – be it performed by a consumer through a specific cooking preparation or by a producer through the deployment of certain growing procedures. The situation I experienced in my empirical work strongly resonates with Heuts and Mol's (2013) analysis and it is from this perspective that I consider the striving for 'good science' of scientists at the CSH as an act of care: an attempt to valorise their research and caring for the quality of their work. Yet I also encountered tensions and constraints to the realization of 'good science', as well as different imaginations of it. Therefore, I will additionally make use of the notion of 'good enough science' as a counterpoint to the ideal of 'good science': to indicate the result of the messy interaction between different 'evaluative principles', the care invested by scientists, and their friction against broader 'valuation regimes'. Second, my work also builds on and speaks to previous research that has focused on valuation in research contexts and their consequences for scientific practice (Felt et al., 2013; Felt et al., 2016; Fochler et al., 2016; see also 2.2; 2.2.2). A recurring theme in this strain of literature concerns the detrimental narrowing down of 'evaluative principles' in contemporary academia – which engenders a hierarchical, rather than heterarchical, valuation context. In particular, my analysis of the CSH can be considered as a comparative study with respect to Fochler et al. (2016) – as it largely confirms (see 7; 7.4) their results by extending the analysis to a different field of study (complexity science instead of life sciences) and to a new epistemic living space (the CSH).

4 Research Questions

In this section, I precisely lay out my research questions against the backdrop of the literature and theoretical framing I introduced above. To begin with, my main research question is heavily informed by the combination of an 'epistemic living spaces' and of a valuation studies approach. It reads:

MQ: How do scientists working at the CSH narratively reconstruct and valuate 'good science'?

The phrasing 'narratively reconstruct' is directly borrowed from the definition of epistemic living spaces and I have chosen in it in order to point to the fact that my research project gives center stage to the perceptions of scientists working at the CSH. Specifically, the focus is on how they construct, assess, and understand value – both in practice and at a symbolic level. In line with the valuation studies approach discussed in Section 3.2, the term 'good science' is not taken here as a pre-existing and reified category, but rather as an emerging and relational one. Moreover, I do not only restrict my analysis to 'good science' as an ideal category, but I also aim to trace any tensions that scientists might perceive between normative imaginations of 'good science' and its practical realizations: 'good enough science' (see 3.2.3). I further assume that what researchers understand as 'good science' is not determined in purely epistemic terms but rather accounts for and relates to all the five dimensions that characterize 'epistemic living spaces' (epistemic, material and spatial, temporal, symbolic and social). As it stands, this research question is too broad, and I have therefore specified its analysis to three sub questions. The relation between the main question and its sub questions is that the latter provide the three specific 'themes' or 'areas' in which I aim to trace how scientists valuate and make sense of 'good science' at the CSH. In other words, I basically understand the main research questions as the translation of the theoretical framing of my thesis, and the three sub questions as the ones that actually point to the topics I want to address empirically. Informed by the literature review, I have selected three sub questions:

> **SQ 1:** How do scientists working at the CSH orient themselves in this particular institutional space?
>
> **SQ 2:** How do scientists working at the CSH navigate interdisciplinarity in practice?
>
> **SQ 3:** How do scientists working at the CSH articulate the societal relevance of complexity science?

Taking stock of my SOTA, the trajectory of complexity science emerges to be intimately connected to the broader contemporary transformations in the science and society relations. Specifically, complexity science – with its powerful jargon, its flexible institutional structure and its 'innovative' epistemology – can be parsed both as a result and a driver of such transformations (see **Table 1**, p.21; **Table 3**, p. 58). This correspondence has so far been overlooked in the literature, even if both Williams and Li Vigni devoted part of their work to the broader societal context in which complexity science was initially developed and then practiced. My three research sub questions are, therefore, an attempt to probe this correspondence empirically. To be more precise, SQ 1 addresses the fact that contemporary academia is characterized by many new institutional configurations for the production of knowledge, of which complexity science and the CSH are eminent examples. The verb 'orient' I used here explicitly refers to the 'orientation work' introduced Felt and Fochler (2011; see 3.1) in the context of 'epistemic living spaces'. SQ 2, for its part, is a way of tracing how the epistemic 'essential tensions' of interdisciplinary collaborations are navigated by scientists at the CSH (the structural and affective tensions are, instead, addressed by SQ1). The answer to SQ2 hinges upon gauging the extent to which Li Vigni's 'imperialistic' critique to complexity science applies to the CSH (see 2.4.3.2). Moreover, it also entails a careful analysis of the role of data and quantitative methods in the integration of and mediation between different disciplines. SQ 3, finally, addresses the fact that the CSH emphasis the production of 'societally relevant' knowledge much more than complexity science traditionally did, arguably as a response to the broader societal pressures on scientific production. The verb 'articulate' is borrowed from Bieszczad et al. (2025; see 2.2.2), a work that focuses on the articulation of societal relevance in another field, deep sea research, which is "currently in transition regarding its understanding of societally relevant research" (Bieszczad et al., 2025).

5 Methods

There is a sense in which my whole work revolves around the question of how to deal with complexity in scientific practice. It would therefore be rather unreflective to not devote some space to explain how I have grappled with this issue myself. Indeed, complexity science and the CSH are themselves 'complex systems' – and therefore call for careful (and caring) empirical engagement. Put simply, studying how scientists at the CSH deal with complexity does not relieve me, as an STS student, from the burden of complexity. As discussed in the Section 1.2, the 'complexity turn' (Urry, 2005) has taken science by storm in the last few decades, engendering many transformations and prompting many scholars and fields to refine their tools in order to deal with the messiness, uncertainty, and multiplicity that come with complexity. Complexity science is but one of the possible 'answers' to complexity, a very solid and arguably successful one, but by no means the only one. Therefore, I would like to begin this section by providing my modest interpretation, based on the work of Law (2004) and Law and Urry (2004), of what an STS answer to complexity implies for social science method.

5.1 An STS Methodological Thrust into Complexity

"In a complex world there are no innocent 'methods': all involve forms of social practice that in some way or another interfere with the patterns of the physical or the social."

(Law & Urry, 2004; p.402)

"As a framework, method itself is taken to be at least provisionally secure. The implication is that method hopes to act as a set of short-circuits that link us in the best possible way with reality, and allow us to return more or less quickly from that reality to our place of study with findings that are reasonably secure, at least for the time being. But this, most of all, is what we need to unlearn. Method, in the reincarnation that I am proposing, will often be slow and uncertain. A

risky and troubling process, it will take time and effort to make realities and hold them steady for a moment against a background of flux and indeterminacy."

(Law, 2004; p.10)

Both Law (2004) and Law and Urry (2004) agree that in a complex world social science methodology needs to be updated. This does not mean that standard methods are a problem in and of themselves, but rather that the issue stems from the "normativities that are attached to them in discourse about method" (Law, 2004; p. 4). The main point is that much of standard social science methodology has been developed under the assumption that "the world is properly to be understood as a set of fairly specific, determinate, and more or less identifiable processes" (Law, 2004; p. 5), but what happens if this is not the case, if the world is a place more complex than this assumption grants? Partly, this question – which, again, largely overlaps with the general interest of my thesis – can be indexed as a philosophical discussion about reality and complexity. My short answer, in line with a material semiotics perspective (see 3) and with Law (2004) and Law and Urry (2004), is rooted in a relational understanding of reality – one which, crucially, is also very much in line with complexity theory. It is complexity theory, after all, to denounce reductionism and to foreground the relations in a system over the elements of that system. But what does this mean in practice?

From the relational nature of social reality, both Law (2004) and Law and Urry (2004) derive the performativity of methods: how "they can help to bring into being what they also discover" (Law & Urry, 2004; p. 393). As a consequence, different methodological approaches do something more than simply producing different perspectives on the same world and we, as researchers, should pay attention to which worlds we "want to help to make" (Law & Urry, 2004; p.391). This does not mean that anything goes, that we can simply tell any story we want or choose our happy ending, but simply that no method is innocent, that, whenever we engage in research, we immediately become committed and colluded, that there is no objectivity and no god trick (Haraway, 1988) to relieve us. While this is nothing new from an STS perspective and even overlaps with some of the founding motivations of STS as a field (e.g., knowledge as situated, see 2.1), what I am arguing here is that 1) this position can be understood exactly as a way of dealing with complexity and is therefore directly relevant to the topic of my thesis, and that 2) understanding knowledge as situated, reality as relational and method as performative should not only inform our analysis of the scientific practices we end up studying – in my case complexity science – but also, and maybe even more importantly, our own scientific practice and method. In a nutshell, what I bring with me from this discussion – what I choose as my way of dealing with complexity in knowledge production – is to rethink method not as a shortcut to get to "bankable guarantees" (Law, 2004; p. 9) or to reality itself, but rather as a messy, slow, and uncomfortable practice. It is to embrace provisionality and uncertainty, "heterogeneity and variation" (Law, 2004; p.6) and to devote care and humility to the troublesome process of writing this thesis – paying attention, at the same time, to the little bits and pieces of my empirical field that I "want to help to make" (Law & Urry, 2004; p.391).

5.2 Overall Methodology: Ethnography as Aspiration

Ethnography is defined as "participating, overtly or covertly in people's daily lives for an extended period of time, watching what happens, listening to what is said, asking questions [and] collecting whatever data are available to throw light on the issues that are the focus of the research" (Hammersley & Atkinson, 1995; p.1). Hence, ethnography has to be intended as a social science *methodology* – in the sense that, unlike a simple method, it involves "theoretical, ethical, political, and at times moral orientations to research, which guide the decisions researchers make, including their choice of methods" (Harrison, 2020; p. 337). As such, as a sensibility and a set of orientations, it is ethnography that served as the main inspiration of my methodological approach. Yet, in order to avoid a certain tendency to overuse the term and the risk of watering down its meaning (Harrison, 2020), I deliberately do not want to describe my thesis as a fully-fledged ethnography of the CSH. This is because ethnography prescribes a level of engagement with the empirical field which I could only aspire to in practice – mostly because of a lack of time and material resources. Nevertheless, I have tried to follow ethnography's invitation to patience and care and to an extended and comprehensive involvement with the empirical field. Informed by ethnography, I also have tried to interpret my work "contingently, as a form of learning, rather than absolutely, as a form of representation" (Whitaker, 1996; p.1) and to produce accounts that evade linearity and straightforwardness – favouring thickness (Geertz, 1973) and multiplicity over narrowness and definiteness. In a nutshell, I have aspired to those characteristics of ethnography that make it "most at home in spaces where complexity, nuance, and betwixt-and-betweenness are valued" (Harrison. 2020; p.330). Since those very characteristics also make ethnography increasingly at odds with "neoliberal academic environments" (Harrison, 2020; p.330) – of which the CSH and the University of Vienna are both, at least partly, examples – taking ethnography as a methodological aspiration for my thesis is both a form of resistance and an offer of dialogue: a second and to some extent meta level way of interrogating myself and the reader on how to deal with complexity in research.

Concretely, having chosen ethnography as an overarching methodology also informed my selection of methods. I have opted for a purely qualitative multi-method approach, with semi-structured interviews representing my main method (see 5.6), participant observation representing my secondary method (see 5.5), and a mix of document analysis and mapping (see 5.4; footnote 25) as supporting empirical tools. Most of all, this is an attempt to provide a thick and fine-grained account of the CSH, approaching it ethnographically from a multiplicity of perspectives. The choice of deploying a combination of interviews and participant observation mediates between a theoretical 'epistemic living spaces' framework – which is person-centred and based on narrative re-constructions – and an ethnographic informed focus on 'unmediated' observation. This choice, indeed, takes into account that, on one hand, "there is usually some disjuncture between what people do and what they say they do [and that], in an interview setting, people are more likely to shade their representations of behaviors toward cultural ideals" (Harrison, 2020; p.345); and, on the other hand, that an over-emphasis on observation as a privileged way of knowing might devalue invisible work (Garforth, 2012). Before explaining in more detail why and how I employed each method in my thesis, I begin with a short explanation of the practicalities and timeline of my empirical work. Finally, I will conclude by reflecting on my positionality and on issues of access and ethics (see 5.7).

5.3 Practicalities and Timeline

I carried out my empirical work between June 2024 and October 2025. The first few months, from June until November 2024, were dedicated to familiarizing myself with the CSH – a phase which I would describe as consisting of 'background work'. In this phase, I mostly explored and partly analyzed material available online concerning the CSH, began the document analysis and the participant observation, and carried out a mapping of the working groups, projects and fundings active at the CSH (see footnote 25). From December 2024 until the end of March 2025 is when I engaged most intensely with the empirical site of the CSH. This was the 'core phase' of my empirical work, during which I carried out all the interviews and the vast majority of the participant observation. After the 'Core Phase', the last phase of my empirical work mostly consisted of analyzing the empirical material I had gathered. Moreover, in this phase I also continued my empirical engagement with the CSH by taking part in two complexity-science-related workshops, each organized by one of my interview partners. Since I did not properly collect data, I do not consider the participation in these two workshops as part of my participant observation. Rather, it represented an important step in reflecting and articulating my positionality with respect to this research project and the field of complexity science more broadly (see 5.7).

It is important to note that any distinction between research phases and, especially, between different methods is somewhat artificial – only possible to reconstruct *a posteriori* but much more confused and ambiguous during the research process. In particular, as in any multi method research project each method feeds into the others and it is thereby hard to establish hard boundaries, hierarchies and timelines (see, e.g., Asdal & Reinertsen, 2021). In my case, this effect was compounded by the fact that the totality of the 'core' empirical work was carried out in the same rooms, corridors and stairs and focused on one relatively small and closely tied community. For example, I would regularly meet my interviewee partners during the talks I attended and often run, during interviews, into references to other interview partners or documents I had chosen to analyse. The fact of carrying out my research on one closely tied, relatively small, and spatially constrained community also implied other complications, for example in terms of ethics (see 5.7) and in terms of generality of my results (see 8). At the same time, it also facilitated the ethnographic approach to complexity science, affording a direct entry point into both formal and informal aspects of what it means to do research at the CSH.

	'Background Work' 06/24-11/24	**'Core Phase'** 12/24-03/25	**'Analysis and Follow-up'** 04/25-10/25
Document analysis			
Mapping			
Participant observation			
Interviews (data collection and analysis)			

Table 4 *Heat map of the different phases of the empirical work (horizontal) and different methods employed (vertical). A darker colour corresponds to a higher work intensity.*

5.4 Familiarization with the CSH: Document Analysis and Mapping

The first step of my research was aimed at familiarizing myself with the hub and mainly consisted of the analysis of parts of its website (5.4.2) and of a complexity science textbook written by three scientists working there (Thurner et al., 2018; 5.4.1). Informed by this first exploration, I also produced a mapping of the CSH research areas and groups, complemented by their respective connections to funding sources (see 5.4.3; footnote 25). In line with my overall methodology, I have taken an ethnographic approach to the documents I have analyzed and thereby consider them "integral part" of my empirical field (Asdal & Reinertsen, 2021; p.25). I decided to adopt a 'practice-oriented' document analysis sensibility (Asdal & Reinertsen, 2021), as it fits particularly well the theoretical framing of my thesis. In this framework, indeed, documents are conceptualized as both material and semiotic objects, lively and embedded in a web of relations and practices that has an effect on them and on which they have an effect. Of the different methodological moves that Asdal and Reinertsen (2021) propose, in particular, I have found two to be the most relevant for my work: 'documents tools' – which invites to consider documents as "something we make use of in order to achieve, realise, establish and create" (Asdal & Reinertsen, 2021; p.148) –, and 'documents texts' – which instead invites to consider documents as rhetorical constructs, having authors, audiences, layouts, lines of argumentation, and narratives. The mapping I produced is, of course, a document itself – but here the relevant relation to keep in mind is simply that connecting it to me, its author (see Donnely et al., 2020, for a summary of recent discussions that conceptualize mapping as relational). Mapping was indeed an extremely situated research activity for me: it was motivated by my initial ignorance about basically anything concerning the hub and it was biased by my first impressions of it. In this sense, I would invite the reader to not take my mapping as informed by "[t]raditional (positivist) approaches [that] view maps as presenting some essential, unbiased truth about the world vis-à-vis the representation of space" (Fileborn, 2023; p. 344), but rather as an exercise whose value is to be found, most of all, in how it showcases the way I was thinking about the CSH in the Summer of 2024 and in how it fed, afterwards, into my subsequent research.

5.4.1 A Complexity Science Textbook

The textbook "Introduction to the Theory of Complex Systems" (Thurner et al., 2018) is based on a two-semester course held every year at the Medical University of Vienna (Thurner et al., 2018, p. vii) and is written by three scientists working at the hub (including the CSH president, Stefan Thurner – whom I later interviewed). As a complexity science textbook, this document can clearly be conceptualized as a pedagogical tool. Thereby, its analysis can shed light on how teachers from this community attempt to present their field to new students and what knowledge they deem most relevant. In this regard, it is important to note that virtually no study program devoted to the study of complexity science exists in the world, and that, therefore, any complexity science textbook is always written for an audience of 'outsiders', at least in some respects. This is for sure true in the case of the textbook I analyzed, as it was written for the students at the Medical University of Vienna.

In analyzing this text, I focused on the "Preface", on its first chapter "Introduction to Complex Systems", on sections 3.6, 4.10, and 5.7 (which are practical examples used to illustrate the applications of some of the methods introduced in the textbook), and on the conclusion "The Future of the Science of Complex Systems?". This choice was motivated by reasons of time, but also to the lower relevance of the skipped parts for my research interests. Indeed, while the parts in which theories and methods are introduced, which I skipped, are written in a dry and matter-of-fact style, the preface, initial chapter and conclusion all provide great insights into the authors' framing and understanding of complexity science as well as into how complexity and complex systems are defined within the community. In order words, these parts are very rich from a textual point of view and rely on a very expressive and articulated narrative. The three examples I selected for analysis, on the other hand, serve as a good illustration of how practical problems from other disciplines (i.e., in this case, biology and economics) are approached by complexity scientists. Thereby, they represented for me a first interesting and rather fruitful entry point into the concrete practice of complexity science. In analyzing this textbook I deployed a simple coding strategy, comprising of four codes: 'definition of complex systems', 'complexity science's methodological and epistemological framework', 'complexity science: role, scope, and relevance', and 'complexity science's cosmological and ideological tenets'.

5.4.2 The CSH Website

It is on the internet that I first started familiarizing myself with the CSH, using its website as a main source of basic information about it: history, mission, scientists, organizational bodies, research topics, groups and so on. While the whole website remained a point of reference throughout my research, in terms of analysis I only focused my attention on its 'About Us' section (Complexity Science Hub, n.d.a). This choice is motivated by the fact that – being clearly aimed at presenting the hub to outside visitors – the 'About Us' section of the CSH website represents a key instance of its identity construction. Importantly, I did not take the content of this webpage at face value but neither did I disregard it as 'fake' and simply detached from the actual scientific practices of the hub. Rather, in line with my ethnographic approach to documents, I considered it to be an integral part, one facet, of what the CSH really is. As of July 2025, the 'About Us' section of the CSH website was divided into four subsections: 'Who We Are', 'Our Purpose', 'Our Core Goals', and 'Why in Austria?' – each contributing different elements to the extremely innovative and socially committed image of the CSH that resulted from the webpage. In order to analyse this process of construction of an 'outward identity', I proceeded by trying to identify the narratives, attributes and tropes that were more insistently mobilized on the 'About Us' webpage, (see **Box 1**). In doing so, I followed Asdal and Reinertsen (2021) and conceptualized the 'About Us' section of the CSH website both as a 'document tool' – having the clear strategic function of characterizing the hub and positioning it with respect to the broader scientific and political landscape – and as a 'document text' – containing interesting and revealing rhetorical and stylistic choices.

5.4.3 Mapping the CSH

The mapping of the research areas and fundings at the CSH[25] not only helped me familiarize with the structure of the hub and with the names of active research projects and of scientists working on them, but it also allowed me to take a peek into the institutional connection and political and economic interests that might influence research at the CSH. The map (which I last updated in June 2024) is organized in four levels: on the top level, I put the macro areas in which research topics are organized (I used circles to indicate them). I adopted the names listed on the CSH website (Complexity Science Hub, n.d.h): "Societies in Transition", "Societies Under Stress", "Metabolism of Societies", to which I also added the macro area "Overarching" to cluster the research topics not belonging to any specific macro area. I colour coded the four macro areas and their connections in order to make their relevance and size more visible. On the second level, I put the 13 topics researched at the hub as of June 2024, I used squares to indicate them and also connected them with the research macro area they belong to (each research topic only belongs to one macro area). Note that, as I came to properly understand only a couple of months after having completed the mapping, research topics more or less coincide with research groups, each having a different 'lead' and a group of scientists working on it. On the third level, I put the 42 research projects active at the hub as of June 2024, using rhomboids to indicate them and connecting each research project to the research topic(s) it is categorized under on the CSH website (most research projects belong to more than one research topic). Finally, on the fourth level, I put the 16 funding institutions that were funding at least one of the research projects as of June 2024. I indicated them with stars and color-coded their connections with research projects in purple.

5.5 Participant Observation: The CSH Talks Series

Participant observation is one of the main methods used in ethnography. It entails engaging with a community to varying degrees, from full participation (just like, or as, a member of the community) to simple observation (Harrison, 2020). In my case, participation meant attending twelve talks taking place at the CSH and belonging to the series "CSH Talks". Usually on a weekly basis, the CSH talks are given by either resident or visiting scientists, last between thirty and sixty minutes and include both a presentation and a rather discussion afterwards. During the talks, I never asked questions or intervened, limiting myself to simple observation. Yet, I would always try

[25] The mapping is accessible at https://phaidra.univie.ac.at/o:2202046. For best visualization, it is suggested to download the file and open it with a pdf reader.

to take advantage of the setting in order to engage with people in the audience and exchange comments and impressions with them on the sidelines of the talks. This became progressively easier as I got more familiar with some of them and after the first interviews. Moreover, attendance always entailed walking around the hub's rooms (the CSH talks are held in the library), overhearing conversations, witnessing formal and informal exchanges among researchers, and other similar and interesting vignettes of life at the CSH. Overall, this resonates with the general observation that participant observation is often characterized by 'ill-defined parameters', is largely improvised even when carefully planned and often very hard to clearly distinguish from the other methods it is carried out in synergy with (Harrison, 2020). In participant observation, data collection most commonly takes the form of field-note writing (Harrison, 2020). In my case, I always took some time (typically around half an hour) right after the CSH talks, or at the latest on the same evening, to jot down notes and impressions. In this process, I mostly tried to keep track of conversations and small exchanges between the speaker and the audience, and of the content, tone and narrative of the talks.

Of course, the choice of carrying out participant observation only within the context of the CSH talks has its limitations. Delamont (2004; p.14), for example, suggests that "it is necessary to plan to observe systematically wherever possible. In a fishing village the researcher should not only observe fishing, but also net mending, the sales of the catch, boat repair, women's everyday lives, the days of men too old to go out to sea anymore, and the experiences of children". Yet, given the limited time and material resources at my disposal, the CSH talks turned out to be a very good location to get a feeling of the main discussions, questions and frictions that enliven the practice of complexity science at the hub. Indeed, these events are usually well attended, and they are absolutely non-hierarchical in structure, thereby favouring conversation and potentially initiating collaborations across groups and expertise. In this regard, they are similar in spirit to other formats traditionally hosted at the SFI (see 2.4.1). Moreover, within the specific context of the CSH talks, I did try to sample in a 'systematic way' (see Delamont, 2004). With the exception of one talk that I attended in June 2024, which I chose because of its manifest reflexive nature, the other talks I attended represent the majority of CSH talks held in February and March 2025 (see **Table 5**). The idea, which I believe was rather successful, was to sample the talks uniformly, without cherry-picking the ones that sounded more interesting or more controversial. Indeed, as it can be inferred from **Table 5**, the speakers of the talks I attended are rather diverse in terms of career level, research group and research interests – even if not in terms of gender (but that is more or less in line with the general composition of the CSH staff).

Date	Title	Speaker	Research Group/ Area	Position at the CSH
19.06.2024	How to Build the Perfect Research Institute?	Joana Gonçalves de Sá	Social Physics and Complexity (Lisbon)	External Faculty
04.02.2025	Modelling Fear of Crime	Rafael Prieto-Curiel	Human Migration, Crime in the Digital Age, Urban Sustainability	Faculty, lead of the Human Migration group
04.02.2025	Principia of Global Trade: Measuring Trade Mass in Global Networks with a Self-Consistent Gravity Model	Daekyung Lee	ASCII	Postdoc
11.02.2025	Modelling Remittance Decisions and the Impact of Disasters	Andrea Vismara	Human Migration	PhD student
11.02.2025	Quantifying the Stability of Refugee Populations: A Case Study in Austria	Ola Ali	Human Migration	PhD student
21.02.2025	I'm not Arguing I'm just Explaining why I'm Right —About the Dubious Origins of our Opinions	Reto Schneider		Visiting journalist
04.03.2025	Learning in a Space of Software Tasks	Xiangnan Feng	Transforming Economies	Postdoc
04.03.2025	Assessing Company-Level Impacts of Energy Transition Scenarios	Jan Hurt	Healthcare and Medicine, Supply Chain Science	PhD student
11.03.2025	What is Hindering the Economic Integration of Migrants Across the EU?	Ljubica Nedelkoska	Human Migration, Transforming Economies	Senior scientist, Lead of the Human Migration group
11.03.2025	Migration, Demographic Shifts, and Public Service Demand	Guillermo Prieto Viertel	Human Migration, Urban Sustainability	PhD student
25.03.2025	Intersectional Inequalities in Social Networks	Samuel Martin-Gutierrez	Algorithmic Fairness	Former postdoc
25.03.2025	What Does it Mean for a Ranking to be Fair?	Mauritz Cartier van Dissel	Algorithmic Fairness	PhD student

Table 5 *A recap of the CSH Talks I attended during my participant observation, indicating the date of the talk, the title, the area of research in which the speaker worked, the speaker, and their position at the hub.*

5.6 Interviews

Interviewing was the central empirical method in my thesis, and I carried out a total of seven interviews: one with Professor Stefan Thurner (the president of the CSH), three with members of the CSH faculty, and three with postdocs. Interviews represented the perfect fit for my overall theoretical framing – epistemic living spaces (see 3.1) – since they focus on the elicitation of meaning from interview partners through conversations, on their narrative reconstructions and on their (self)perceptions. As I have already tried to stress above, nevertheless, interviews were for me embedded in a broader ethnographic approach, and thereby inextricably enmeshed with the other methods I adopted.

All the interviews were semi-structured, meaning that I relied on a prepared interview guide consisting of several open questions, which I followed up with "probes seeking further detail and description" (Roulston, 2010; p.15). I reworked the questionnaire before each interview, tailoring it to the respective interview partner. Nevertheless, the first part was always devoted to a few questions about the interviewee's academic background, about how they had gotten interested in complexity science, and how they had arrived at the CSH. This, besides being meant to make the interviewee comfortable, linked directly to my first research sub question (see 4) as it was a way to better understand the academic composition and positioning of the CSH crowd. After the first introductory part, I proceeded to ask specific questions relating to my other two research sub questions and often referring more directly to the work of my interview partner. Perfectly exemplifying the ongoing and iterative nature of the research project (see Jensen & Laurie, 2016; p.5), after the first three interviews I ended up having to adjust the second part of my interview guide. In two of the interviews, I read and partially analyzed papers written by the interview partner before the interview and used them to inform the questionnaire. Moreover, in one case I asked my interview partner to talk about one of their papers and invited them to contextually draw or sketch on a printout I had brought with me. This approach was inspired by the tradition of visual methods and, more precisely, by the technique called 'object interview' (Pottinger et al., 2022; King et al., 2019).

Three of the interviews took place online and four in person. One of the in-person interviews, moreover, took place in two sessions. The length of the interviews varied quite substantially, with the three online interviews generally being substantially shorter than in person interviews (see **Table 6**). In order to get access to the empirical field, I initially reached out to people working at the hub in high level roles (including Professor Thurner) and to an acquaintance of mine employed at the hub. Of all the people I contacted first, I only ended up interviewing Professor Thurner. Yet, I still had a couple of informal chats with all the others and would like to thank them for their help and suggestions. Access, overall, was not extremely straightforward and even once gained it felt at times insecure. For example, several of my interviews were rescheduled, sometimes on very short notice. In one case, one person that had initially agreed to be interviewed ended up cancelling their participation all-together, again on very short notice. In sampling participants for my interviews, I tried to include scientists from different research groups (only two of my interviewees ended up belonging to the same group) and at different career stages. I decided to exclude PhD students because, since there are no study programs in complexity science, I thought they would be less representative (both at a conscious level and at an analytical level) of the complexity science community. Regular attendance of the CSH Talks ended up being another factor in my selection of interview partners – as I selected a couple of my interviewees after being struck by some of their comments in the talks. In one case, I also contacted an interviewee specifically because of their research, which I had found to be particularly relevant to my research foci. Before every interview, I obtained informed consent from my interview partners. The form I used allowed for granular expression of informed consent, meaning that interview candidates could choose between different options before signing the document (e.g., between being referred to with real name or after pseudonymization).

	Length	**Number of quotations**	**Number of codes** (counting multiple occurrences)
Interview 1 (online)	00:34:53	48	121
Interview 2	01:27:16	105	446
Interview 3	01:16:46	161	610
Interview 4 (online)	00:39:22	60	275
Interview 5 (online)	00:54:06	93	498
Interview 6	01:16:33	132	550
Interview 7	01:32:28	139	776

***Table 6** Recap of the interviews I carried out, including length of each interview, number of quotations and of distinct codes.*

5.6.1 Transcription, Coding and Analysis

I carried out a verbatim transcription of my interviews (Paulus et al., 2014) and used thematic analysis (Braun & Clarke, 2021) to analyse them. I began by feeding the audio files into a locally run AI tool for transcription (Descript) and then manually reviewing the outputs each time. I coded my interviews using the Desktop version of Atlas.ti. My first attempt was to develop codes based on my research questions and to thus deductively code all my interview material. Soon I realized this would not work and switched to inductive coding. This entails selecting sentences or chunks of text as quotations and then attaching to them one or multiple codes – i.e. “a word or short phrase that symbolically assigns a summative, salient, essence-capturing, and/or evocative attribute for a portion of language-based or visual data” (Saldaña, 2021; p.5). I carried out three rounds of coding. In the first round I simply went through the interviews and defined new codes as I proceeded. In the second round, I reviewed all the interviews in light of the codes that emerged in the first round, adding or removing codes from already existing quotations and, only in a few cases, modifying the quotations themselves or adding new ones. In the third round, I first reviewed the codes and defined them with a precise description. Then, I went back to the transcripts and checked the pertinence of the quotes attached to each of the codes. In this step, I realized some codes were inconsistent and either got rid of them or merged them with other codes. Moreover, I also eliminated some codes form many of the quotes. Unfortunately, for lack of time I only managed to carry out the third step of coding on parts of the codes. Finally, I grouped the resulting codes into ‘themes’, the latter being categories that “[capture] something important about the data in relation to the research question and [represent] some level of patterned response or meaning within the data set” (Braun & Clarke, 2006; p.10). Through the process of coding and analysing my interviews, I ended up with 105 codes, and 9 themes (see **Table 10**in Section 10). Overall, both quotations and codes have a rather uniform density across interviews, with the exception of Interview 1 – which, besides being markedly shorter, also resulted in a much lower density of quotations and codes (see **Table 6**). Yet, for each individual code, the density distribution can be very uneven across interviews, since, as it is to be expected, each interview ended up focusing on slightly different topics.

5.7 Positionality, Access and Ethical Considerations

My background is in mathematics, having devoted the better part of ten years to a bachelor’s and master’s degree in theoretical mathematics in Rome and then to a PhD in mathematical physics in Vienna. It is only in the last four years that I got interested in social sciences and have dived in the study of STS through the master’s course for which this work is the thesis. This trajectory has faced me with the strong contraposition between two very different modes of producing knowledge. For simplicity, and even if this dichotomy does not neatly hold in practice, I would say that this trajectory made me experience both ‘quantitative’ and ‘qualitative’ research. Much of my motivation to study the CSH, therefore, came exactly from the tensions inherent to my academic trajectory. Indeed, the CSH both heavily relies on mathematical and physicalist tools and ‘applies’ such methods in a way that, during my studies in theoretical mathematics, I couldn’t even begin to imagine. In this sense, I got interested in complexity science as a field standing somewhat in between by original background and STS.

For these reasons, right from the start, I have felt at the same time very much at home at the CSH and very much alienated by it. On one hand, I could rather easily understand the language deployed by the scientists and their way of reasoning – I felt, very sharply, that I could be one of them. Moreover, in more than one occasion my background helped me to get access to the field, as scientists often rapidly shifted their attitude towards me as soon as they were made aware of my background. Indeed, already having completed my PhD helped to revert the power relation inherent to interviewing high level academics (Perera, 2021). Moreover, I believe the topic of my PhD made my interviewees feel a commonality with me: suddenly, I wasn't an external researcher studying them with an 'alien gaze' anymore. On the other hand, the study of STS had changed my perspective compared to when I had just finished my PhD. Some simplifications, approximations and approaches that would have felt 'natural' just a couple of years before immediately rang problematic to me in the accounts of the CSH researchers. This engendered interest and at times opposition in me, but also an ambivalent sense of guilt coming from a sense of 'breaking the trust' of my interview partners. These feelings were reinforced by the fact that the CSH is a small community, which allowed me to feel more easily included, but also meant the ethical stakes were higher. Even if I believe in the end this was never a concrete possibility, the idea that someone could to some extent get in trouble or even just create tensions with some of their colleagues because of my work was somehow always a little present in the back of my mind.

The more I tried to not get stuck in simple binaries like insider/outsider, right/wrong and quantitative/qualitative, the more I was irremediably brought into the fray by my multiple background. The distinction between insider and outsider, which is very relevant in any ethnographic approach (Harrison, 2020; Folkes, 2023), affected my work deeply from start to finish. With Folkes, I want to reflect on how this positionality was relational and always in flux, changing me and the field as I moved through it. My reflections culminated in my participation to two workshops organized by two of my interview partners. In one of them, I really participated as if I was a member of the community: taking part in breakout sessions and brainstorming with other researchers. It gave me an eerie feeling: it was something completely new – neither maths, nor STS – but my participation was influenced by both. I belonged and I didn't. My ideas were agreeable but also very much out of tune. Most of all, I realized that working on this thesis had had an effect of me – that I was both the subject and the object of my research.

6 Empirical Findings

The organization of this chapter revolves around the analogy I already used in 1.2Section 1.4, according to which the CSH can be explored as a building under-construction. Thereby, I have divided my material into three sub sections, which progressively investigate different aspects of the 'CSH-building'. The first section, the most concrete, relates directly to the empirical site of my research, the CSH, and serves to introduce the following two. It contains a brief history of the CSH and expounds upon its organization and research (see 6.1). I have named it 'The CSH: Foundations, Layout, Piping, and Outer Walls' to highlight the fact that, in my view, it serves to introduce the shell of the 'CSH-building', its history, its basic structures and its external appearance. Section 6.2 broadens the perspective to complexity science and to how the CSH and the scientists working there relate to it. In this section, I go over the empirical material about the definition and composition of complexity science, its characteristics, and the sense of (not) belonging of my interviewees. I have named the second section 'Complexity Science: The Construction Material', to highlight the importance of the notions of complexity and complexity science as building blocks of the 'CSH-building'. Finally, Section 6.3 provides a much finer grained account of the practice of complexity science at the CSH, specifically by reviewing empirical material that directly relates to epistemology. Moreover, it also introduces the category of 'good science', accounting for how the scientists working at the hub (e)valuate research: what they strive for and what they assess as valuable scientific work, as well as the challenges and impairments they experience on the way to achieving it. I have named this last section 'Doing Research at the CSH: The Life in the Building', as I understand it as an account of the lively practicalities that shape research inside the 'CSH-building'.

6.1 The CSH: Foundations, Layout, Piping, and Outer Walls

To introduce the 'CSH building', I will mainly focus on its history, its organization and its research areas and topics. These three aspects are all intertwined and are therefore best explored contiguously. On one hand, as I will try to substantiate below, the assemblage of the institutional configuration of the CSH was a result of both historical contingencies and deliberate organizational design. On the other, historical and organizational elements are evidently also influencing the CSH research topics and areas. Through this whole discussion, it will already emerge that the CSH shares many of the general characteristics of complexity science (see 2.4), but that, at the same time, it also introduces some novelties and twists. I will begin by reviewing the history of the CSH (6.1.1) and then move on to discuss some of its basic organizational structures and characteristics – i.e. its relationship with other academic institutions (see 6.1.2), its funding strategy (see 6.1.3), its connection to the international scientific community (see 6.1.4) and its outreach and public relation structures (see 6.1.5). Finally, I will quickly introduce the research topics and research types that scientists at the CSH engage in (see 6.1.6). **Table 8** at the end of this section collects the results of this preliminary exploration of the CSH.

6.1.1 The Foundation of the CSH

"At that point Stefan [Thurner] was building the center and he reached out about this... he had this really cool proposition of starting a group [...] which was not exactly what I was doing, but close enough. He said that I could build this group here. [...] I think I liked what he was building, even though I didn't know that yet. I would learn over time how this would actually work."

(Interview 6)

The history of the CSH proper starts in 2015, when a cooperation between the Technical University (TU) Wien, the TU Graz, the Vienna Medical University and the Austrian Institute of Technology (AIT) was officialised (Der Standard, 2015). Scientific operations only began in 2016, though, and it is therefore common to hear or read of 2016 as the year the CSH was born (e.g., Complexity Science Hub, n.d.a). Of course, the inception of the hub has deeper roots. According to Stefan Thurner, president of the hub since its foundation, a somewhat complexity-related community already existed in Vienna in the 2000s. This included the University of Vienna theoretical chemistry group of Peter Schuster and Peter Stadler and the people collaborating with Martin Nowak and Karl Sigmund on mathematical biology. These scientists, in the words of Thurner, "would maybe not have associated with complexity science", but were working on pretty much the same systems, using the same kind of math and concepts, and, very often, with an overlap even in terms of collaborations. Moreover, Thurner himself had already been working on complexity for a few years. Coming from theoretical physics, he had gotten exposed to complexity science through the work of Geoffrey West on scaling phenomena (see for example, West, 2018), by whom he was also invited to visit the Los Alamos National Laboratory (LANL). For reasons of national security (no foreigners were being admitted to LANL because of a recent theft of hard disks), Thurner ended up at the SFI instead – what will turn out to be a deeply transformative experience and, in some sense, the first seed of a long-range influence of the SFI over the CSH.

"*I had to go to the Santa Fe Institute for a few months. I had no idea what that was, but I found all my idols, all my heroes there, like Murray Gell-Mann and Brian Arthur and Stuart Kauffman... all these guys that I've known from popular books since I was a kid. They were all there and what*

I've seen there is that that those guys were all working on problems that they were interested in, and I found out that so far in my life, I had always worked on problems that other people were interested in. [...] This was super fascinating [...]. So, I tried to reflect if I could do that too, and I got drawn into complex systems from that time."

(Interview 1)

Once back in Vienna, over the course of about ten years, Thurner progressively ventured into interdisciplinarity by completing a second PhD in economics, "was still producing physics papers, but more and more [...] was with the biophysics people" and finally shifted his research focus to complex systems. During this transitional phase, he was also facilitated by a favourable academic standing, as he had already got tenured at thirty-one and "had a fantastic boss" who knew the SFI, supported his interest in complexity science and let him research the topics he found most fascinating. By the 2010s, the transformation was complete, and Thurner's efforts had started to bear fruit: in 2009, the Vienna Medical University created the "Institute for Science of Complex Systems", still active today (Medical University of Vienna, n.d.), and appointed Thurner as its Head. Yet, there were also tensions. In fact, as Thurner explained to me, the atmosphere in Vienna was not so favourable to the establishment of a complexity science research institute like the CSH and transitioning to complex systems science also entailed a series of personal struggles for him.

"If you start dedicating more and more time to new things, you have to give up, sooner or later, other things. [...] So, of course you pay a price, but it was not an existential price... but you lose friendships and people start talking about you in not friendly ways. Making change has a price."

(Interview 1)

This created a situation that could be described as 'on the edge of complexity' – to borrow a term, 'edge of chaos'[26], so dear to the complexity science community. Indeed, complex systems science had already reached a good level of concentration in the Viennese academic landscape, but more work was still required to reach the critical mass that would trigger a phase transition and the establishment of the CSH. This ambivalence is maybe best exemplified by a major

[26]In physical terms, a system is defined as chaotic if a small variation in initial conditions causes a big variation in evolution trajectories. Conversely, a system is defined as stable if its time evolution is robust with respect to changes in initial conditions. The edge of chaos is defined as that regime in which a system can easily switch between a chaotic and a stable behaviour. In complex systems science, the edge of chaos has a particular relevance due to the fact that it is assumed to explain how a wide range of systems (physical, biological and social) alternatively display chaotic or organized behaviour.

setback in the founding story of the CSH: according to Thurner, the first attempt at establishing a complexity science research center in Vienna failed around the year 2013. At that time, the then president of the Nanyang University of Technology (NTU) in Singapore, Bertil Andersson, was trying to establish a new Santa Fe Institute in Asia, for which he also envisioned a European outpost. Even if for a time it seemed Vienna could be that outpost, in the end things did not work out – notwithstanding Andersson's impressive network of acquaintances (he had previously been president of the European Science Foundation – ESF) and the involvement of other prominent Viennese figures, including the mayor of Vienna and Helga Nowotny[27]. Nevertheless, this first attempt was crucial to lay the foundations for the final establishment of the hub – which happened less than two years later with the active involvement, again, of both Thurner and Nowotny. Importantly, right from its inception, the hub had, at least at a rhetorical level, a markedly innovative and 'unique' footprint – both in epistemological and in organizational terms.

"There was a set of people, maybe a handful of people, including Helga, including a former Vice Chancellor of Austria, including two rectors of Viennese universities, and me... and we decided we can build up a small center in Vienna, without Singapore, without the NTU involvement. We can just try to keep it very small, keep it very nice, much smaller than the Santa Fe Institute. Maybe we could have [...] a platform for 20 people that have a real desire to improve the world through science with the newest digital possibilities and big data."

(Interview 1)

6.1.2 The Relation with the Other Partner Institutions and the CSH Governance Board

Back in 2015, the CSH was conceived of as a collaboration network with and between several Austrian scientific institutions: the AIT, TU Wien, TU Graz and the MedUni (Der Standard, 2015). By now, this has grown to include Boku, the Central European University (CEU), the Donau Universität Krems, the Interdisciplinary Transformation University Austria, the University of Veterinary Medicine Vienna (VetMedUni), and the Wirtsschaftsuniversität Wien (WU) (Complexity

[27] Helga Nowotny, founder and former president of the European Research Council (ERC), is herself a central character in the history of the CSH. Currently, she is Chair of the Scientific Advisory Board of the CSH. Nowotny is also a renown STS scholar, among the proponents of the Mode 2 Science paradigm, and has devoted much of her career to the study of complexity and of the interactions between science and society (see 2.2).

Science Hub, n.d.b). This network is undoubtedly one of the assets of the CSH, in terms of both symbolic and material resources.

"It's very nice to have the universities as partners. For many reasons. For the students, for the infrastructure, for the reputation, for giving the degrees… for lots of support universities can give you: in terms of legal advice, of computation power, all things that in the beginning we didn't have at all… and many things where we still are very much indebted to universities like technical universities – hosting many of our servers, for example."

(Interview 1)

The body that governs the CSH and effectively manages this collaboration is the CSH governance board – composed by the directors of the member institutions, Thurner as scientific director, and Philipp Marxgut as managing director. Yet, as Thurner explained to me, the CSH itself is rather independent and ultimately has the last say to decide how to carry out research.

"Content wise, our board never intervened. That's great. They maybe say: "okay, we, as the Medical University, would like this in that direction". But other than this, there is no intervention… which gives us independence."

(Interview 1)

This predicament is the result of a precise historical trajectory. Initially, indeed, the collaboration was supposed to be based on direct economic support from the other universities, which would be hiring the new people to be employed at the hub. Tensions arose, though, according to Thurner as the result of a contraposition between the innovative drive of the CSH and a rigid academic establishment. Finally, a much looser format was agreed upon – crucially one that shifted the burden of hiring scientists onto the CSH but, at the same time, also granted it more room for manoeuvre and a rather strategic positioning with respect to its university partners.

"We came up with this scheme that every university partner must bring in two positions: one senior position, one PhD position. […] It should not be existing staff. We would look for a great person and try to hire this person, not just relabel existing university people. […] We wanted a new crowd. This was a very hard process, because, even if the university rectors and presidents liked the idea and supported it very much, university departments don't like the idea of having an institution they cannot control. […] There were some departments that understood […], but most of the departments hated the idea and actively or passively tried to not make this a success. So, we decided to apply for funding ourselves and hire people as the hub. That proved much more effective, because then we didn't have this internal friction. We turned the problem around. We

are hiring people and universities can take a look at them for five years or something… and if the universities like them, they can take them over as professors very easily. So, for the universities, I think we're now a little bit like a head-hunting device."

(Interview 1)

6.1.3 Funding of the CSH: Different Sources and Different Types

With university partners only providing organizational and infrastructural support, and dedicated funding schemes not being available within the complexity science community – the hub's existence and reproduction is predicated on the establishment and nurturing of other sources of funding. Through the years, the hub has managed to solve this problem with a mix of public and private funds and by complementing its embeddedness in the Austrian academic landscape with an equally impressive connection to policy makers, stakeholders and other non-academic institutions. In many ways, this situation resonates with Williams' (2012) analysis, which identified 'quasi-stable funding', "pure science with market dynamics" (Williams, 2012; p. 153), as one of the main characteristics of the 'quasi-discipline' of complexity science (see 2.4.2.1; **Table 3**).

"The connection with stakeholders, let's call them stakeholders, it's very important. And I'm happy that Stefan is our director, he is really active and tries to involve politics, politicians, and general stakeholders."

(Interview 2)

Leveraging the favourable Austrian political context, the hub seems to have managed to cut out a somewhat consulting role for itself, whereby stakeholders and policy makers trust and finance the hub so that, through a "bunch of policy projects" (interview 6), it can provide 'complexity science solutions' to inform their decision-making.

"Austria enjoys a long history of progressive governance whose spirit we rely on today to identify challenges and opportunities in society that will benefit from complexity science solutions. Our government officials and public administration are ready to explore new ideas backed by rigorous science. […] Growing numbers of organizations, institutions, and companies across the country look to the Complexity Science Hub to use complexity science in their decision-making."

(Complexity Science Hub, n.d.a)

Such a well-structured mechanism of funding and collaboration is not incidental, but rather the result of an incremental historical process – whose roots can be traced back to the very first days of the hub.

"Stefan needed to start somewhere. He needed someone who could show off things. So, someone with a background in physics, who could play with web development and could do some physical demonstrations. So, I was, in fact, the first one employed here. [...] And it was super nice to demonstrate these complex systems. [...] It was public... there were stakeholders, some politicians... now I don't even remember. We were in the phase that we had some basic funds to start things, but of course we needed some more funds to go on. I mean, we started with one person, we are now more than 70. Okay? You need to start somewhere."

(Interview 2)

To provide more detail to this discussion, I turn to my mapping of the research groups and fundings of the CSH (see 5.4.3; footnote 25). From it (last updated in June 2024) it emerges that the hub basically relies on three different categories of funding institutions (Complexity Science Hub, n.d.d): 1) the public bodies devoted to funding scientific research: the European Commission (specifically through the ERC), the Austrian Research Promotion Agency (FFG), the Austrian Academy of Sciences (ÖAW), the Austrian Science Fund (FWF), the Vienna Science and Technology Fund (WWTF), and the Ludwig Boltzmann Gesellschaft; 2) the public institutions that are not specifically designed to support research but that have either funded the hub as an 'institution of interest' or directly commissioned research projects to the hub: the Austrian Federal Chancellery, five Austrian Ministries, the Bundesdenkmalamt, the city of Vienna, and the Austrian National Bank (ÖNB); and 3) private institutes, companies and funds: Voestalpine, the Union Bank of Switzerland (UBS), NOMIS foundation, the Alan Turing Institute, and the B&C Privatstiftung. Moreover, two different types of funding are active at the hub: project funding – tied to research projects –, and basic funding – granted to the CSH in order to establish, maintain, and expand its activity. To the basic funding of the CSH belong two 'Core Fundings' – respectively from the City of Vienna (Complexity Science Hub, n.d.j) and from the Austrian National Bank (Complexity Science Hub, n.d.k) – and two funding schemes aimed at the 'Foundation and Expansion of the CSH' – respectively from the FFG (Complexity Science Hub, n.d.l; n.d.m) and from the Austrian Federal Ministry of Innovation, Mobility and Infrastructure (Complexity Science Hub, n.d.n).

Basic Funding	Project Funding		
Public funding to the CSH as an institution of interest	**Public Funding for General Purpose Research**	**Public Funding for Commissioned Research**	**Private Funding**
e.g., City of Vienna (Core Funding), ÖNB (Core Funding), FFG (Foundation and Expansion of the CSH), Austrian Federal Ministry of Innovation, Mobility and Infrastructure (Foundation and expansion of the CSH, CSH PostDoc Program)	e.g., European Commission (ERC), FWF, Austrian Academy of Science	e.g., Austrian National Bank, Austrian Federal Ministry of Interior, European Commission (Asylum, Migration and Integration Fund – AMIF)	e.g., Voestalpine, UBS, Alan Turing Institute

Table 7 *Schematic representation of the sources and types of funding at the CSH, with examples.*

6.1.4 Scientific Advisory Board and External Faculty: International Networks, Pedagogy, and Recruiting

Besides the CSH Governance Board, there are two organs that are particularly important at the CSH. For matters concerning recruiting and research planning, the most relevant body is the Scientific Advisory Board, which is chaired by Helga Nowotny and includes an impressive list of scientists from the most renowned universities in the world – among which the SFI is very well represented. The Scientific Advisory Board is complemented by the CSH external faculty (Complexity Science Hub, n.d.c), which provides a relatively soft form of support – enabling research collaborations and exchanges – and most of all make the hub into an 'institute without walls' like the SFI before it (Williams, 2012).

"And an important institution at the hub was this external faculty: a set of, I think, 50-60 people that we would like to collaborate with: those people in the world that we think would be great if we could think about problems together. They're great help. Many of them actively collaborate every year, visit us, make workshops – brainstorming workshops – interact with our students, host our students, send their students. That makes us a hub."

(Interview 1)

Together, these two bodies include a huge pool of renowned scientists and crucially contribute to position the CSH as a central node – a hub – in the international network of complexity science. This effect is compounded by the fact that the institute is also very active in creating venues for the community to connect and grow. For example, many talks (e.g. 'CSH Talks', see 5.5) and workshops (CSH workshops) are organized at the CSH, as well as symposia, summer and winter schools, and conferences. The core spirit of such events, whether meant as internal or external, seems to be that of stimulating originality through horizontal formats of discussion and exchange – again directly connecting with the style and spirit of the SFI[28] (see Williams, 2012, and Li Vigni, 2023; see 2.4.1). Among many other advantages, being so well embedded in the international complexity science community also emerged from my empirical material as one of the driving factors of the CSH recruiting success.

"I got to know the CSH through workshops, essentially. When I published my first paper [...] I presented it at a conference [...]. And then I met [a CSH faculty member] who was presenting at my same session [...] And so then we chatted, we met, we knew each other's work and then they invited me to the workshop they were organizing."

(Interview 7)

"The way I came here was because there was a school, a summer school [...] My current boss was teaching in the school [...] and other researchers that are here from the hub... and was taking place here at the hub."

(Interview 3)

6.1.5 The CSH Communication Team: Outreach and Public Relations

The last organizational unit of the CSH that I want to discuss is its communication team – whose work underpins most of the CSH successful public relations strategy. Importantly, this unit should not be considered accessory to the activity of the CSH, but vital. In part, this is surely a reflection of the funding needs of the CSH, but, at the same time, it can also be understood as a manifestation of broader transformations in the relation between science and society (see 2.2). Moreover, it also fits Williams' (2012) finding that complexity science has brought about a new style of communicating and 'writing' about science (see footnote 14).

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[28] Another staple of the CSH, which promotes a similar ideal and is also borrowed from the SFI tradition, is the 'Teatime' taking place every afternoon at the hub – to which all employees are welcome to take part and that I was personally able to attend on one occasion.

"It's very important that you have the way of communicating your results for a broad audience. That's why we have a very strong team of public relations and the website press releases."

(Interview 2)

Remarkably active for a still relatively small institute, the CSH communication team in practice produces an impressive amount of material – including news updates on 'everything happening at the CSH' (Complexity Science Hub, n.d.e), digestible summaries of published papers, appealing and interactive data visualizations (Complexity Science Hub, n.d.f), and teaching material for primary and secondary schools (Complexity Science Hub, n.d.g). Moreover, it operates in close contact with scientists themselves – who in turn benefit from a wider distribution of their work.

"If there is someone who can turn your symbols into something that can be understood, your paper is much more read than other papers, no? [...] that will enhance the citation of your paper. It's a [...] symbiosis sort to say. [...] Of course, there is a small overhead: you have to prepare nice pictures, trying to explain Anja [Böck, head of communications at the hub], *who is not a physicist, the main purpose of the paper. So, you are forced to explain things in a simple way."*

(Interview 2)

A paradigmatic example of the activity of the CSH communication team is the 'About Us' section of the CSH website. In order to present the hub to the outside, this webpage mobilizes a series of supposed attributes and characteristics and thereby constructs a specific and appealing identity for the CSH – in many ways, a 'CSH brand'. First of all, there is a clearly future-oriented temporal dimension to the rhetoric used in the 'About Us' webpage, as the CSH is insistently framed as 'innovative', and associated with expressions like forward-thinking', 'forward-looking', and 'go future'. Overall, the hub is presented in a position of radical break with respect to the past, transcending academic tradition both in disciplinary and in structural terms. A similar framing is also reserved to methodology, as the CSH is described as deploying 'pioneering' and 'novel' "paradigms, approaches, methods, and tools". Concretely, these are presented to be highly inter- and transdisciplinary and to be relying on a unique combination of "qualitative expertise from diverse disciplines with a profound understanding of quantitative methods and modelling techniques". Through collaborations "with practitioners and policymakers in the public sphere, as well as private-sector entities, to understand the challenges and risks confronting diverse facets of our society", the hub is tasked with a quasi-activistic mission to "benefit society and the planet" and solving issues "that matter now" and "that cannot be addressed successfully in any other way". The emphasis on applications is also reinforced through the use of expressions like 'hands-on experience' in association with the research of the hub. As it is to be expected, complexity and complexity science also play a crucial role in the outward presentation of the CSH. To begin with, following the very common narrative that I have already introduced in 1.2, the world is presented as "becoming ever more interconnected and interdependent – and thus complex", and complexity science is thereby framed as the right epistemological innovation in order to tame complexity and potentially even harness it as an opportunity. In this regard, data and quantitative methods are not only presented as fundamental to understand complex systems, but also as the unifying and bridging forces holding together any possible friction stemming from the diversity of expertise and approaches the CSH claims to encompass. Finally, the CSH is also described as 'open', 'independent', and ethically committed – contributing to produce a rather progressive image of the hub and of its mission.

Box 1 *The 'About Us' section of the CSH website*

6.1.6 Research Areas and Topics at the CSH

"And it's a great place to do both theoretical work but also applied testing and applications in the real world."

(Interview 5)

Back in Summer 2024, when I carried out the mapping of the research topics at the CSH (see 5.4.3; footnote 25), research at the hub was organized into three main macro categories: 'Societies in Transition', 'Societies Under Stress', and 'Metabolism of Society', with two extra stand-alone research topics that I gathered into the additional category ‘Overarching’, namely: ‘Foundations of Complex Systems’ and ‘Data Analytics’. The first three macro areas, as well as the topic ‘Data Analytics’, are all concerned with the study of society. ‘Societies in Transition’ collects research projects that deal with the transformation of society, in particular in reference to technological, urban and economic development. ‘Societies Under Stress’ collects research projects on phenomena that are understood as destabilizing for societies (namely, migration, polarization, and social complexity). Finally, ‘Metabolism of Societies’ collects projects on societal production and reproduction (specifically on economics – supply chains and finance – and healthcare). The topic ‘Foundations of Complex Systems’, instead, is a sort of *unicum*. With only a couple of projects belonging to it, but still involving a large number of scientists, this topic is led by Thurner himself and seems to differ from the others both in terms of functioning and in terms of scope.

“Our foundational research contributes to diverse research areas at the CSH and beyond. This encompasses specific challenges in mathematics, artificial intelligence, and statistics, as well as simulation techniques that aid in understanding collective phenomena, ranging from systemic risks in financial markets to the fragmentation of societies.”

(Complexity Science Hub, n.d.i)

Taking stock of this quick analysis and integrating it with the rest of my empirical material, a few characteristics of the research topics and areas active at the CSH begin to emerge rather clearly. Firstly, the great variety of disciplinary backgrounds of scientists, approaches and topics – ranging from economics to geography, from psychology to sociology. In this regard, though, while the CSH seems to be “a very open place” (interview 6), there are also pretty heavy imbalances, with most scientists having a background in the natural sciences.

“What I like here is we have many different branches, that's also why we are very interdisciplinary. We have psychologists, sociologists, economists, physicists, biologists, whatever you mention. We don't follow only one path, but we really try to see complex systems as a whole.”

(Interview 2)

“Natural science dominates at the hub, together with data science. [...] But there are also people like me who came from other disciplines and have their original training. [...] So, I'm not the only one at the hub... which is nice.”

(Interview 6)

Second, the strong focus on societal issues, with an inclination towards the development of real-world applications and solutions.

"Everybody at the hub is actually very interested in social systems. Everybody's studying social systems: supply chains or cyber-crime or health systems or financial systems, all of that is a social system."

(Interview 5)

Finally, the existence of at least one research niche devoted to the development of theories and methodological tools that, addressing complexity at a more general and abstract level, can then be applied to a multiplicity of case studies and contexts. While the first two of these characteristics are foregrounded in the outward presentation of the CSH (see **Box 1**), the third seems to instead be kept in the background. Moreover, while the breadth of topics and the interest in generalizing theories are quite in line with the overall characteristics of complexity science (see 2.4; **Table 3**), the societal focus seems to be a rather specific twist introduced by the CSH. Moving forward, it will be an open question to understand how all these tendencies coexist at the hub and are navigated by scientists (see 6.3).

Relations with other academic institutions	**Funding**	**Connection with international community**	**Public Relations and communication**	**Research**
Partnership with many Austrian universities and academic institutions (e.g., AIT, TU Wien, TU Graz, Boku, Medical University of Vienna) <u>Loose collaboration format</u>: managed by the CSH governance board (but very little intervention), no direct funding, universities provide infrastructural and	Mix of public and private funds <u>Three different types of funding</u>: funding directly granted to the CSH for its establishment and growth, funding to research projects, commissioned research Shares characteristics with the 'quasi-stable funding' of	Remarkable connections with international scientific community, especially of complexity science Exemplified by the high profile of the CSH Advisory Board and External Faculty Organization of workshops, conferences, summer and winter	Very active communication team, working in close contact with scientists CSH 'About Us' webpage: CSH presented as innovative and progressive, focusing on societal issues through quantitative methods and data	Great breadth in terms of disciplinary approaches and topics (in line with complexity science) <u>Applied research</u>: overall focus on societal issues, development of real-world applications and solutions (seemingly novel with respect to complexity science)

symbolic support, CSH functions as a 'head-hunting' device for universities	complexity science (Williams, 2012; see 2.4.2.1)	schools etc. (SFI style) This connection makes the CSH a hub, an 'institute without walls' like the SFI (Williams, 2012)	Resonates with Williams' (2012) finding that complexity science has brought about a new way of communicating and 'writing' science	<u>Foundational research</u>: research niche working on general theories and methodological tools (in line with complexity science)

Table 8 *Schematic representation of the basic organizational and institutional characteristics of the CSH.*

6.2 Complexity Science: The Construction Materials

"I don't know if you have checked the map of complexity... It's huge, it's a webpage and it shows all the fields that are inside complexity science... because complexity science it's very complex also to define... is it really a science? Like what is the nature of it? What is inside it?"

(Interview 3)

In this section, I will zoom out to complexity science, a fundamental ingredient of the CSH identity that lends it much of its authority and organization. Doing this will provide more information to understand the positioning of the hub and prepare the groundwork to later explore what really goes on inside the walls of the 'CSH-building'. A central observation is that, as already discussed in Section 2.4, the difficulty of defining what complexity science is has been an ongoing and characterizing trait of the complex systems community. Here, I collect the traces of this struggle that emerge from my empirical material, and I try to reconstruct this 'emerging discipline', 'quasi-discipline' or 'scientific platform' (see 2.4.2) with respect to a series of different characteristics. Importantly, the techniques of definition my interviewees resorted to are all but straightforward, as complexity science was defined both in positive and in negative terms, and often in rather ambiguous ones. In Section 6.2.1, I begin by collecting the most direct attempts of definitions of complexity and complexity science. Section6.2.2, with its subsections, contains a brief overview of what emerged from my interviews as characterizing the scientific approach of complexity science. In Section 6.2.3, I report on empirical material relating to scientists' perceptions of the disciplinary status of complexity science and to how they make sense of their positioning within the field. Finally, Section 6.2.4 serves to discuss what, overall seemed to be the single most defining trait of complex systems science: its ability to provide a venue for 'getting around boundaries' (Williams, 2012; see 2.4.2.1).

6.2.1 Definitions or Lack Thereof

"When I think about complexity science, I never conflate it with real life complexity. For me, complex systems – and I think it's a quite standard definition – are systems with a lot of interactive elements, relatively simple properties and interaction rules that give rise to emergent properties. And that's still ambiguous, but a relatively well-defined concept. Complexities in the real world are completely independent of this definition."

(Interview 4)

My interview partners used a variety of expressions to describe complexity, yet no one provided a precise and univocal definition. In fact, to find a relatively precise – albeit extremely broad – definition, I had to turn to the complexity science textbook I analyzed (Thurner et al., 2018; see **Box 2**). This should not be understood as a way for my interviewees to sweep an inconvenient problem under the rug, though, as each of them was very aware of the ambiguity of the term and didn't seem to consider it to be a limit or a flaw. In line with what I discussed in Section 2.4.3.1, most of my interview partners resorted to the evocative and mathematico-physicalist jargon of complexity science in order to put the finger on their object of study. Indeed, complex systems were most commonly qualified as those exhibiting 'emergence' and non-trivial interaction structures.

"To some extent the definition of a complex system is not super clear, right? So, there are people that have their own definition. For me, I would say the words I'm using are the keywords. And, usually, the one you hear from most people is 'emergence'."

(Interview 7)

Interestingly, 'complexity', intended as the object of study of complexity science, seemed to assume a dual status with respect to reality: at the same time able to encompass virtually anything, as "everything is a complex system" (Interview 3), and absolutely nothing, being "completely independent" (Interview 4) of real-world complexities.

Again, this is not an imprecision but rather an instance of the 'slippage' (*sensu* Williams, 2012; see 2.4.2.1) that characterizes complexity science. Correspondingly, complexity science was also presented as hard to pin down, as a sort of umbrella "science of the world" (interview 5) with little descriptive bite.

"What is complexity science anyway? It seems like science of the world. Everything is a complex system, so I don't find that very descriptive."

(Interview 5)

A paradigmatic example of definition of complexity is given in the book "Introduction to the Theory of Complex Systems" (Thurner et al., 2018), written by three scientists working at the CSH. In its most compact form, it reads "complex systems are co-evolving multilayer networks" (Thurner et al., 2018, p. 22), a sentence that the authors proceed to unpack into ten statements. In sum, complex systems are systems with many elements (statement 1, Thurner et al. 2018, p. 22), that interact through several time-dependent interactions (statement 2 and 3, Thurner et al. 2018, p. 22) – which also depend on the state of the system itself, i.e. statuses and interactions 'co-evolve' (statement 5, Thurner et al. 2018; p. 22). They are usually non-linear (statement 6, Thurner et al. 2018, p. 22), context-dependent (statement 7, Thurner et al. 2018, p. 22), and path dependent (statement 9, Thurner et al. 2018, p. 22). Finally, complex systems often have memory (statement 10, Thurner et al. 2018, p. 22). Clearly, the language is directly borrowed from mathematics and physics. Moreover, it is also important to note that this definition is extremely broad – including virtually anything that is not a laboratory setup, including societies, ecosystems, the stock exchange, the internet, a living body and so on.

Box 2 *A definition of complexity*

Ambiguity and 'slippage' were also consciously identified by my interviewees as main traits of the disciplinary status of complexity science, confirming the findings of Williams and Li Vigni (see 2.4.2). Yet, it is also interesting to note that a sense of change was shared by more than one interviewee: a feeling that, whatever its nature, complexity science "doesn't feel like this weird niche or subfield anymore" (interview 4) and is progressively becoming more institutionalized thanks to devoted "master's degrees", "journals" (interview 7) and "funding" (interview 4).

"Whether or not complex systems is a discipline is to me not clear. And I say this because I have two contrasting views on this. On one hand, it is clearly a discipline in the sense that it has a set of methods and viewpoints on the world that are shared across anybody who's doing complex systems. On the other hand, because it is a method of inquiry rather than an object of inquiry, it does tend to just overlap multiple disciplines."

(Interview 7)

6.2.2 The Methodological Specificity of Complexity Science

In the face of the general lack of precise definitions, my interview partners did share the opinion that complexity science represents a definite break with respect to traditional disciplines. Here I discuss how my interviewees discussed this difference in methodological terms. For example, one of them compared it to data science and stated that "if you truly believe your system is complex, a lot of tools from data science might not apply" (interview 7). With respect to traditional statistical approaches, argued another, in complexity science you have to be much more careful about 'outliers' because, as a "consequence of the complexity and of the networkness of a system" (interview 1), they cannot be discarded and actually 'define' or 'drive' its behavior. With respect to physics, where in many cases it is possible to isolate processes and phenomena, "in a social system [...] there are always things influencing each other" (interview 6) and therefore a simple correlation, which is of great relevance in physics, is not at all surprising when studying a complex social system. With respect to economics, complexity science 'data structures' were presented as being both more comprehensive and more fun and interesting.

"In the complexity science crowd, I actually think that the data structures that they work with are much more interesting than what the economists are working with. A very detailed description of an economy in thousands of industries is much more fun than saying you have a structural transformation from agriculture to manufacturing to services. That loses all of the interesting details. And then the tools that the economists have, they fall short."

(Interview 6)

In other words, even if not precisely defined, the approach of complexity science assumed a more concrete, even though still piecemeal, appearance *vis à vis* the techniques and approaches normally used by other disciplines – which a 'true acknowledgment' of complexity makes obsolete or incomplete. Altogether, thereby, a common opinion shared by my interviewees is that complexity science is epistemologically innovative, a "new school of thought" (interview 7). Remarkably, all the transformations complexity science seems to call for directly affect the practice of quantitative methods, and only of quantitative methods, and are thereby all articulated in quantitative terms.

"To some extent, I think that we are a new school of thought that says, 'hey, you cannot approach certain topics in this reductionist approach'. In some sense, we are not new to this. Within the social sciences and within economics, for example, people have been saying this for much longer. We are just the ones that are trying to bring the mathematical tools to back that idea up."

(Interview 7)

In particular, there are a couple of methodological characteristics of complexity science that were mentioned more consistently as setting it apart from most other disciplines and as being directly related to complexity: the role of data, experiments and predictions.

6.2.2.1 Experiments and (Big) Data in Complexity Science

"I also have this experience: you study electrons, you go to the next office where they do experiments, you say 'look I found these things. Is it right?' They do the experiment and that's it... in [the study of] society, not."

(Interview 2)

In general, compared to other disciplines, in complexity science "experiments are relatively rare" (interview 4). This was presented to me as a consequence of the complexity of the systems under study and, often, of their size – which make experiments "very hard to design" (interview 7). Yet, because of the diversity of disciplinary backgrounds, many different attitudes co-exist within complexity science in this respect – with widely varying perspectives in terms of feasibility, validity and relevance of experiments, but also of their function and aim (e.g. using them for data collection or only for validation purposes). Overwhelmingly, though, what emerged from my research is that the scientists at the CSH do not feel they can consistently rely on experiments in their work. Nevertheless, this does not mean that my interviewees felt detached from empirical evidence. In an interesting twist, experiments were rather displaced than forgotten, becoming a source of inspiration instead of a form of validation. In their place, modelling, simulations and 'computational work' take the center stage in providing rigor and explanations.

"I do think that a lot of the inspiration for complex systems still comes from empirics. Even if you look at the history of complex systems, most papers and most methodological improvements came from a specific system that was under study. It wasn't the classical kind of thought experiment of the mathematician. It was more, 'hey, this system is different from the way it is normally described. We need new mathematics to be able to understand that'. And so, models were developed in order to do that."

(Interview 7)

"Whilst in mathematics you have that kind of rigor of demonstration, in these kinds of things the rigor comes from the simulations. You really do computational work, it's very algorithmic."

(Interview 7)

Note that, as complexity scientists almost "never do experiments [and] mostly try to find the data" (interview 7), data gathering assumes an incredibly prominent role. Indeed, my interviews often used the term 'data hunting', which is quite telling of the emphasis put on the activity, and data was generally discussed as a relatively scarce resource, especially 'good data' (see 6.3.2.2). Yet, according to another narrative that I encountered quite consistently big data and computer simulations are to be considered the way to make social systems “more and more experimental” (interview 1). From this perspective, “the centuries-old data problem of the social sciences is rapidly disappearing” (Thurner et al., 2018, p.19), thanks to the sheer increase in data collection in virtually any dimension of life and sociality. For the supporters of this point of view, the need for experiments can be completely bypassed thanks to simulations and ‘data struggles’ either do not exist or will soon be overcome.

“In many real-world situations there is only a single history, and this cannot be repeated. […] Computer simulations allow us to create artificial histories that are statistically equivalent copies. […] The possibility of creating artificial histories solves the problem of repeatability. With the current availability of computer power and high-dimensional and time resolved data, computational limits or data issues no longer pose fundamental bottlenecks for understanding complex systems.”

(Thurner et al., 2018, p.26)

6.2.2.2 Predictions in Complexity Science

“Complex systems are very difficult to predict, for various reasons. There is a lot of noise and, even if it's deterministic, there are initial conditions.”

(Interview 5)

One of the main characteristics of complex systems is their unpredictability. In general terms, this predicament is related to ‘uncertainty’, but it can definitely be specified further. When explaining the difficulty of forecasting and predicting complex systems, my interviewees referred to “noise” (interview 5), to the difficulty of knowing the ‘initial conditions’ of the system, to the non-linear dependence of the evolution of the system from ‘initial conditions’, and, maybe most characteristically of all, to the fact that “it’s not only that interactions matter, but that the law of interaction changes with time” (interview 7). This last point is often described as ‘co-evolution’ or ‘adaptability’ within the community of complexity science, and it encodes the idea that complex systems adapt as they evolve.

“The reason why I think any estimation it's like a ballpark figure is because the extent to which the system can adapt to shocks is huge, to the point in which you can be completely wrong. […] So, we can model in the sense that we can make assumptions, scenarios, about how things will go. But if you ask me to predict it I would say no, it's not possible.”

(Interview 7)

The status of predictions in complexity science was often framed to me as an element of break with respect to traditional disciplines and as a driver of transformation of scientific practice – for example by changing what is considered to be an acceptable modelling technique.

"I'm not a forecaster, also because I think there's too much going on in the world to forecast very far. There is always this debate about how important it is to have a good prediction. And this is also something that the social scientists and the complexity science crowd don't agree on."

(Interview 6)

"A lot of the models that exist in economics are these kinds of auto regressive models where you use the past of the time series, of GDP or something like this, to infer the parameters of the model and then you use this model parameters to make predictions. I fundamentally think it's wrong. Because I think that if the system is complex [...] suddenly the past is no longer really informative of the future."

(Interview 7)

Nevertheless, this does not mean that predictions are completely crossed off the epistemic register of complexity science. Rather, my interviewees stressed that in complexity "you can make predictions, but at a different level of observation or of a different kind" (interview 7). Interestingly, they also rushed to explain that "it's not like that it becomes less quantitative of a science" (interview 7). Overall, this is a perfect example of how complexity scientists perceive themselves as "just the ones that are trying to bring the mathematical tools to back that idea up [that reductionist approaches don't work on many systems]" (interview 7). From the perspective of complexity science, uncertainty does not need to halt quantitative approaches as it can be quantified and, at least to some extent, explained quantitatively – as long as more refined and more humble quantitative approaches are deployed (e.g., 'scenario building').

"Scenario building is really key to complex systems in practice, because the space of possible scenarios is huge and there is no point in really trying to have something which is predictive. You make reasonable assumptions, you create scenarios which should be realistic, but you know that they are limited in scope and, in this sense, it becomes a much softer kind of science."

(Interview 7)

6.2.2.3 Modelling Complexity, Looking for Simplicity

"So, this is the challenge. Usually what I've seen in complex system modeling is that you try to have the least in your model... it's a very complex model, but the specification of the model, per

se, is very simple – the elements of it have very simple rules. Because you want to show that you can get complexity from very simple rules. And this is because you're trying to figure out what is the essential element of the system. And this is hard. It's not an easy task."

(Interview 7)

From the previous two sections, modelling emerges as a central practice in complexity science: in some sense a substitute for experiments, thanks to simulations, and as a tool mediating the expression of a new type of predictions. Yet, as discussed in Section 2.3.2.2 and in Section 2.4.3.2, modelling complex phenomena entails a series of specific and very thorny issues. The one my interviewees identified more consistently is the tension between simplicity and complexity. According to them, complexity science is characterized by the attempt to reach a fine balance between the simplicity of the model and the complexity of the system it describes – an attitude that actually dates back to the founders of the discipline[29].

"Obviously it is not exactly reproducing the mechanisms… but at least it shows that very simple models can reproduce some patterns that we can see in the real social world. Like polarization, consensus and so on… and then it shows that it doesn't have to be a cognitively complex model to produce complex phenomena, it can be very simple."

(Interview 5)

"It's not a like a polar opposite. You don't understand anything about the real world because you don't reproduce absolutely everything or you understand everything because you understood this simplified version of the real world. It's that you found this simplified version that still lets you understand some basic things."

(Interview 4)

The overall tendency in navigating this tension seemed to be skewed towards simplicity rather than complexity, possibly following an approach borrowed from physics and, arguably, motivated by a reductionist impetus. A similar appreciation of simplicity as a 'magical' carrier of complexity also surfaced in one of the CSH Talks I attended, where the speaker explained that all the results

[29] Murray-Gellmann himself, one of the main personalities behind the foundation and expansion of the SFI and one of the most famous complexity science popularisers, reportedly stated, during one of the 'SFI founding workshops', that the world we see around us is merely the result of "surface complexity arising out of deep simplicity." (quoted in Pines, 1988; p.3).

presented were “answers to very simple questions” and advocated in favor of ‘asking simple questions’ as the best approach to scientific inquiry.

“You can complicate things, but physicists tend always to limit, to reduce the number of parameters you need. That's the first law.”

(Interview 2)

6.2.3 Orienting Oneself in Complexity Science: Self-definition and Sense of Belonging

“Our field is not a field in the traditional sense. I think that nobody has the ‘right’ background.”

(Interview 7)

Arguably, exactly because of the properties of complexity science I discussed so far, ‘feeling’ at home within the discipline is all but straightforward. Confirming the results of Li Vigni (2021a; see 2.4.2.2) and those of Molyneux-Hodgson and Meyer (2009; see 2.2.1.2), no one of my interviewees defined themselves as a complexity scientist. Rather, they used terms like “anomalous” (interview 7) to refer to their expertise and background. Generally, this sense of not-exactly-belonging was understood as a structural element shared by everyone working in complex systems, something that “might define most of the complexity scientists” (interview 6). At least in a couple of cases, though, it also appeared to be the result of the disciplinary composition of complexity science – heavily skewed towards mathematics and physics. This second interpretation is somewhat surprising. Indeed, at least at the CSH, there is actually quite a good balance between scientists with a background in mathematics or physics and those coming from other disciplines – opening up the question of whether other factors contribute to creating the feeling of being on an unequal footing with physicists and mathematicians.

“Most complexity scientists would come out of physics, I guess. And I don't have that background. So that's maybe why I think that I'm a little bit more adjacent than in the court.”

(Interview 6)

In most cases, my interviewees described their disciplinary identity as complicated or not well defined – often in terms of an ongoing negotiation, “a balance, a mixture” (interview 6) between the old and the new, at the same time in opposition to their original discipline and retaining some of its elements. In a similar and reversed fashion, they never qualified complexity science as a stable institutional ‘home’ and most commonly referred to it only as a direction of research, a topic of study, or a scientific sensibility.

"I don't know how to describe myself. I'm certainly not a classic [omitted disciplinary profile] anymore. That's gone. [...] But, in a pinch, I usually say that I study complex social systems."

(Interview 5)

"I sometimes think more about myself as a complexity scientist, especially when I talk with pure physicists, because, sometimes, I no longer can speak in the same terms with them. I don't mean technically, I mean epistemologically: in terms of the kinds of problems that we care about and what we believe is more important about research. The same happens when I speak with other people that are collaborators in complexity. Because it's interdisciplinary, it is common to collaborate with sociologists, political scientists, psychologists, et cetera. And still, I don't fully align with many people from these sorts of disciplines. Since I'm submerged most of the time in the complexity bubble, I think about myself mostly as a physicist."

(Interview 4)

In other words, my interviewees most clearly reconstructed their sense of belonging negatively, in opposition to traditional disciplines, rather than positively, in relation to complexity science. This resonates with the findings of both Williams (2012; see 2.4.2.1) and Li Vigni (2021a; see 2.4.2.2) and confirms complexity science, at least in the perception of the scientists participating in its project, as a place – a 'scientific platform' – that affords and even catalyzes opposition to scientific conservatism and disciplinary rigidity.

"*We tend to agree with other complexity scientists on how we disagree with the paradigm.*"

(Interview 7)

6.2.4 Connecting Things, Getting Around Boundaries

"*I think there is fundamentally a lot of people that approach complexity science with this desire to kind of transcend the limitedness of scientific endeavor. I think this is what motivates a lot of us. I think it was the same for me.*"

(Interview 7)

The sense of possibility and innovativeness associated with complexity science maybe most clearly emerged from my interviews in terms of the struggle to transcend disciplinary boundaries and 'connect things' – both across disciplines and across the boundaries of 'pure science'. In many cases, this organizational and epistemological gearing was connected directly to the properties of complex systems: only understandable by 'looking sideways' (see Williams, 2012; p. 179) and being open to follow multiple tracks.

"Scaling phenomena exist in the same way in particle physics as they do in biology. That was my 'habilitation' work, to see this same kind of strange statistics [non-Gaussian power law statistics] in these different fields and ask myself 'why'?"

(Interview 1)

"I imagine complex systems like fractals – not a grid, ordered. Complex systems are very interconnected, sometimes you have connections between concepts that you never thought were connected before. You have to be very open; you shouldn't always follow the same track."

(Interview 2)

6.2.4.1 Getting Around Disciplinary Boundaries: Affordances and Struggles

Generally, my interviewees agreed that remaining within the bounds of traditional disciplines is less productive and that "a lot is lost when people stay within their own discipline" (interview 5). Moreover, they also remarked that it can be much more 'boring' to always work on the same things. Moving between disciplines was often framed as the result of scientific interest and curiosity – and complexity science as one of the "very few places where you are allowed to do that" (interview 6).

"The thing that I very much appreciate is defining yourself by a question instead of a discipline. [...] The communities that exist in the classical disciplines don't fit the sort of arrow that I take through them."

(Interview 6)

In some cases, in virtue of its outsiderness, the whole field of complexity science is vested of a sort of trail-blazing mission, with the manifest aspiration of stepping out of current disciplinary paradigms.

"Understand the limitation of the paradigm and push that limitation. To me, if you're really trying to be a scientist, that's what you should do: figure out where it hurts and push those buttons. [...] So not just individually, but as a field, that should be our goal. [...] That would be good science."

(Interview 7)

Nevertheless, resonating with the results of Williams and Li Vigni (see 2.4.2), the mixed and flexible interdisciplinary positioning of complexity scientists was also indicated as a challenge by my interview partners – either in terms of reduced competitiveness and employability, or in terms of lower depth of engagement with new scientific debates.

“That’s kind of a disadvantage in science today because mostly scientists focus on one thing. They become super expert in that thing. They build a career on that thing. In complex systems, the risk is that, like me, you are very open to many subjects, and you are not considered an expert in any particular field. So, no one takes you as a professor, although you probably have the capabilities to make links where others wouldn't.”

(Interview 2)

“If I had to go for a habilitation, it would've to be either in something like methods for economics and social sciences, or it would have to be in something like computer science… because that's the way my publications are seen like… although it's difficult because we also published in physics journals and other disciplinary journals. So, I don’t know, it's not easy to say.”

(Interview 7)

What is more, a couple of my interviewees openly expressed frustration or criticism concerning the breadth and depth of complexity science’s scope, for example by lamenting an asymmetry between physics, mathematics, computer science, on one hand, and all the other disciplines, on the other.

“We claim to be very interdisciplinary, but it’s not enough to mix computer scientists with physicists or mathematicians. Computer science was born from those two fields. So, it's like the child talking with the parents only and not with the cousins. I think much more is needed.”

(Interview 3)

6.2.4.2 Finding Purpose in Complexity Science: Societal Interventions

This discussion assumes particular significance in light of the fact that one of the strongest motivations to work in complexity science was insistently presented to me as that it allows ‘natural scientists’ and people with a ‘technical’ background to work on societally relevant issues – “using numbers for analyzing social problems” (interview 3) and making “social science questions [become] addressable by natural science methods” (interview 1). In some sense, this is another form of ‘getting around boundaries’, one that my interview partners seem to find motivation and purpose in.

“I think a lot of people that come to complexity science are precisely motivated by that, not by the complexity of it, but by the ability of saying something about society. The average story that I hear from people that are more or less in the same field is that they were working on something very technical and then they got bored of it, they wanted to study something that had a wider

impact, more societal level impact, or to understand the world they live in, [...] to even design policy."

(Interview 7)

"I was a bit burnt out with what I called the physics of dead things. Discovering that there could be a physics of things that were alive, and in particular people, and that you could really learn and say new things about collective behaviours using the approaches that I was familiar with was very, very inspiring. That's why I decided to go in that direction."

(Interview 4)

In the most extreme cases, the intervention in society even becomes the "ultimate goal" of the theory of complex systems – which is presented as finalized "to eventually control and design systemic properties of systems such as the economy, the financial system, social processes, cities, the climate, and ecology" (Thurner et al., 2018; p.v). This confirms the ambivalent attitude of complexity science towards prediction, forecasts and control, already identified by Li Vigni (2020; see footnote 22) and which also surfaced in several instances in my research (see 6.2.2.2; 6.3.1.1).

6.3 Doing Research at the CSH: The Life in the Building

From the previous two sections, as well as from the State of the Art, what emerged is that complexity science is characterized by two fluidities: 1) the ability to move 'sideways', connecting and integrating different disciplines and areas of expertise, facilitated by its flexible and porous institutional structure; 2) maybe slightly less manifestly, the ability to move 'vertically' between different scales and depths of analysis – for example shifting between 'foundational' and 'applied' research, between the focus on specific systems and the focus on decontextualized and generalizable methods. This combination results in a sort of 'all-encompassing epistemology', which stretches indefinitely both in depth and in breadth and puts complexity scientists in the position to approach basically any kind of topic and at any scale of analysis.

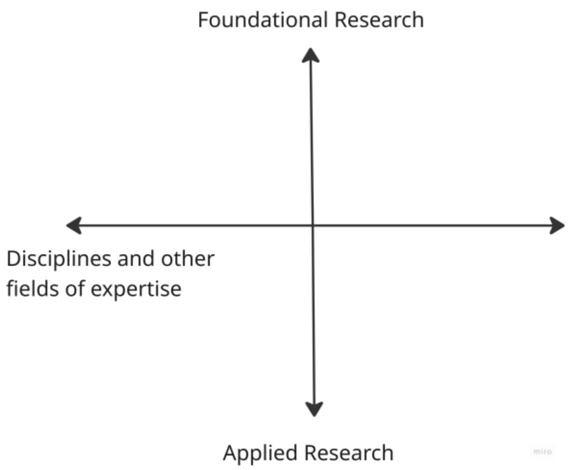

***Figure 1**: The 'all-encompassing epistemology' of complexity science is represented as a Cartesian plane in this schematic and admittedly simplistic representation.*

Importantly, the ability to move freely along these two axes seems to even be passed on to the individual researchers, at least according to some of my interviewees. Thereby, it is not only the collective epistemology of complexity science or of the CSH to be 'all-encompassing', but even complexity scientists themselves. In other words, the task of navigating these two axes is not only undertaken collectively by the CSH as an institute but is also part and parcel of researchers' individual scientific practice.

"I remember Stefan [Thurner] a few years ago in an opening speech, in an assembly that we had at the CSH, he said exactly that interdisciplinary doesn't mean that in a group of four people, each of the person has a different expertise, but that everyone has four different expertise in their brains, and they work together."

(Interview 4)

"I think everybody at the hub is very finely attuned to the actual reality of the social system that they study. They're not just slamming a model from another science to the system, but they really, in depth, understand how supply chain works, how Austria health system works, or, I don't know, organizations or economies."

(Interview 5)

From an STS perspective, this situation raises a series of questions. Therefore, Section 6.3.1 focuses on whether and how such a flexible arrangement is actually achieved in practice, and how different scales of analysis and perspectives are mediated between and evaluated by scientists. In Section 6.3.2, I tried to first pinpoint and then expound upon the factors that impair or complicate the navigation of the CSH's 'all-encompassing epistemology'. Finally, Section 6.3.3 collects all my empirical material concerning the 'societal relevance' of the CSH's research – which I have conceptualized as a sort of third dimension of the CSH 'all-encompassing' epistemology.

6.3.1 Navigating the Axes of an 'All-encompassing' Epistemology

6.3.1.1 Quantitative and Qualitative Approaches

"I think there is very important work that is not quantitative: qualitative descriptions of the world that look at cases, interview people, and come up with a narrative. In the end, even though that is super important, when you've created that narrative you want to figure out whether it is true in a probabilistic sense. [...] So, there's room for other types of research, but I think that making statements that cannot be tested quantitatively, that you cannot ask whether they generalize and then prove that they generalize... I think it cannot stop there. And when I say 'cannot', I mean I will not stop there."

(Interview 6)

The broadest distinction scientists have to navigate when doing research at the CSH is between quantitative and qualitative approaches. Overall, research at the CSH is "very quantitative, super quantitative" (interview 3) and researchers at the hub overwhelmingly adopt quantitative methods as the prime or unique tool of inquiry. Qualitative methods, on their part, are granted a supporting role at best. In this regard, differences in the scientists' backgrounds play a very important role. Researchers coming from the social sciences, even if they identify themselves as 'quantitative' or 'computational' social scientists seem to be much more likely to include qualitative work in their research, perceiving it as part of their background and expertise. Researchers coming from the natural sciences, on the other hand, usually do not diverge much from the techniques they are trained in. They tend to stick to quantitative methods and, in most cases, simply start applying them to different topics than in their 'home disciplines'. This is true regardless of the fact that more than one researcher, and not only among those with a background in social sciences, expressed appreciation and even admiration for qualitative approaches – arguing that 'good science' should include a combination of both qualitative and quantitative approaches and that "they should be in dialogue all the time" (interview 3).

"To understand the system, you need to do a lot of qualitative research and then you can start thinking, maybe, of some quantitative way to represent it. [...] When I see a paper that just has a

quantitative model, and I have such papers, I'm not satisfied because anyone can come up with an interesting quantitative model that kind of makes sense."

(Interview 5)

Clearly, this discrepancy between the imagination and the practice of qualitative methods is partly the result of systemic influences (see 6.3.2.1) as well as the result of the higher epistemic authority of quantification (see 2.3.1.2). Yet, researchers also justified the predominance of quantitative because of some of their epistemic properties. Among these, the ideas that quantitative statements are "unambiguous", "testable" and better suited for generalizations. Indeed, regardless of the fact that the position of predictions in complexity science is all but straightforward (see 2.3.2.2 and 2.4.3.2), my interviewees always connected the 'testability' of quantitative statements to their ability to "project trends that you expect over time", forcing scientists "to provide really precise predictions" (interview 5).

"Mathematical language [...] is the only way to express facts in a quantitative and predictive manner and to make statements that are unambiguous."

(Thurner et al., 2018, p. vi)

"I think the problem of small case studies is that it's difficult for representation, because they're very specific. With big data it's easier. I think for translation around different communities it is more difficult to use cases. In that case, it's better a quantitative approach."

(Interview 3)

6.3.1.2 'Working with' or 'Working on' Other Disciplines

"I advocate into doing that [developing an interdisciplinary expertise]. That's why I tried to read a lot and understand the jargon… because it's needed to have some communication. So, I think I like this bridge, I like being a bridge… and I think here we need more bridges."

(Interview 3)

The second aspect that scientists need to navigate in their research at the CSH is interdisciplinarity – both within the CSH and in collaborations outside of the CSH. My interviewees all expressed strong support for the integration of different knowledge. They generally identified the individual development of interdisciplinary expertise as a precondition for successful collaborations, stressing the importance of familiarizing oneself with the literature, theories and results from other disciplines and of becoming fluent in a new jargon. One of my interviewees, for example, shared the fear that if interdisciplinary collaborations are not practiced on a sufficiently solid common ground, the risk is that “someone, something, gets lost in translation and you arrive to useless models and misinterpretations” (interview 4).

“I always personally try to be very sure about what are the theories and the interpretations associated with the phenomenon I’m studying every time that I have projects with more interdisciplinary collaborators. [...] So, in my view, everyone has to be on the same page. I don't know if that's the, the position of every scientist in my field. I think it's the position of many, many scientists, at least in my group.”

(Interview 4)

At the same time, interdisciplinary practice also emerged as a complicated endeavor. My interviewees described it as time-demanding and as requiring care and resources. They pointed to the many frictions in the translation of concepts across disciplinary languages and some shared frustration about the fact that any attempt at filling knowledge gaps will never be comparable to receiving formal training in a new discipline. Finally, finding people that have the right skillset and that are actually interested in interdisciplinary collaborations was also mentioned as a challenge. All these problems, furthermore, seem to be compounded by the lack of time and academic resources as well as by the pressure to publish (see 6.3.2.1).

“You would need to be trained in the literature of a field, which requires a lifetime of training, plus know all the new methods, which also requires a lifetime of training. It's just too much to ask for a single person. The answer is, of course, collaborations. You just work with people that understand the field more than you do, or the methods more than you do. But then you need to find people who are willing to engage with you and that speak the same language that you do... So, it's much tougher.”

(Interview 7)

Between the high aspirations of interdisciplinarity and the mundane and structural challenges to properly practicing it, there is a vast space for issues to develop. In part, then, it is not surprising that, in the most extreme cases, a completely contradictory tendency exists among scientists at the CSH – a sort of short-circuiting of the problem: working on topics from other disciplines disregarding the existing literature and without collaborating with experts from that field.

“I think it's also ignorance, though. I've met people, and sometimes I've been at fault with this myself, that just don’t know the literature. You know, you start engaging with a topic, you think it's interesting, you read a couple of papers, you see a model you don't like because it’s too simple, and then you start modeling it and you don't fully engage with maybe 60 years of research that

have been done in the field – where they're perfectly aware of some of the things you might be saying… maybe they haven't put it in math, but you're not bringing anything new to the table."

(Interview 7)

"Also, for example, here [at the CSH] there is all this work in epidemiological models… and that was used during Covid without doctors or epidemiologists themselves."

(Interview 3)

How are these two contrasting views and practices reconciled within the same field and within the walls of the same institute? Largely, this seems to be predicated on the epistemic authority of quantitative methods – in this case playing out at a more nuanced level than in the quantitative/qualitative divide. Extending the discussion of Section 6.3.1.1, in fact, even within the realm of quantitative methods there seems to be different levels of purity and, correspondingly, of authority. In some cases, in fact, the deployment of the abstract methods of physics and mathematics seem to justify the straight-out appropriation of disciplinary topics. Nothing short than an 'imperialistic stance', as described by Li Vigni (2023).

"It's really cool that physics can enter other fields, but I don't think it's nice, this jeopardizing of the field and not talking with the people of the field that has been there for years."

(Interview 3)

"It is well known that the size of a country or the distance between two countries influence the amount of trade that exists between these two countries. And there is literature in economics that does it, but it does it with a method that it's a very linear regression kind of method. Then the physicist arrives, looks at the model and says: 'this is too stupid, this is wrong', and then builds a very complicated model that has exactly the same conclusions. And then the economists see it and dismiss it since there's nothing innovative."

(Interview 7)

6.3.1.3 The Spectrum of 'Applied Research' and 'Foundational Research': 'Caring about Context' or 'Caring about Methods'?

In order to better understand why different levels of authority are granted to different disciplines and how this affects interdisciplinarity, I believe it is first important to analyze the 'vertical axis' of the complexity science epistemology – starting with the contraposition between 'foundational' and 'applied' research (see 6.1.6). While this distinction barely appears on the CSH website, it emerged as more relevant during my research. How do scientists at the CSH make sense of it and navigate it? On the one hand, 'applied research' is described as being concerned with modelling specific systems (organizations, economies, governments) and inferring their properties from observables. In this case, the focus is on specific and contextual objects of study, and the aim of research is to produce directly applicable but only locally relevant knowledge – for example aimed at addressing societal issues through policy intervention. On the other hand, 'foundational research' seems to address overarching theoretical or methodological questions and to adopt a generalizing perspective. It works on problems that are somewhat detached from specific empirical cases and at the same time virtually relevant to all. In this case, the specificities of context are seen as not fundamental and, if anything, as a sort of noise – a hindrance to the full comprehension of the system under study. In fact, the focus here is on underlying properties, i.e. all those properties that do not depend on any specific realization of the category of systems that share them. The, sometime implicit, contraposition between 'applied' and 'foundational' research was felt quite strongly by my interviewees, who often also translated it into and connected it with their backgrounds, practice, and interests. In a nutshell, it can be recast as the tension between 'caring about context' and 'caring about methods'.

"Context is very important. To the point where I get sick of context, because I do think you need to generalize somehow... but I wouldn't know how to generalize this to a unifying theory that I myself would buy. That's not how I was trained. It's not my skill set. It's not my way of thinking about what research is trying to achieve."

(Interview 6)

"I think there are people who are interested in the systems because they inform them on the methods and there are people that are interested in the methods because they inform them of the system. I am in the second category. I care about the system. I'm interested in how the system works. I'm interested in economics. That's the thing that I'm interested in. And I am interested in learning how to model it better. For me then it really is important why you make that assumption, what is telling you about the system, what you are you ignoring. That's important to me."

(Interview 7)

If the scope and focus of 'applied' and 'foundational' research are different, the same is true for their methodologies. While both quantitative, they can consist of very different tools. Overall, 'foundational research' seems to rely on more abstract quantitative tools, for example belonging to physical and mathematical frameworks, which indeed tend to discard specificity in favour of generality. On the other hand, 'applied research' is the type that more closely resembles disciplinary studies in social and life sciences and often deploys techniques borrowed, for example, from statistics or data science.

"I think people that do more foundational work here at the CSH do that. They do more the mathematics of this kind – with all the information theory or things that you need. There is also maybe another set of work, which I would describe more as data science, where the objective is to infer something about the property of the system from the observables."

(Interview 7)

‘Applied’ and ‘foundational research’ are of course only ideal types, which are hardly ever practiced at the hub in the way I have described them, in isolation. In other words, the contraposition between ‘applied’ and ‘foundational research’ is better understood as that between the two poles of a spectrum – rather than as a dichotomy – and the research carried out at the hub as characterized by a combination, to different degrees according to the case, of both ideal types. To convey a sense of how diverse within the ‘applied’ – ‘foundational’ spectrum the research carried out at the hub can be, I make use of three CSH talks I attended (Complexity Science Hub, n.d.o; n.d.p; n.d.q). The projects that were accounted for in the first two talks ‘Quantifying the Stability of Refugee Populations: A Case Study in Austria’ and ‘What is Hindering the Economic Integration of Migrants Across the EU?’ (Complexity Science Hub, n.d.o; n.d.p) both dealt with the topic of migration and were concerned with trying to measure the levels of (in)stability and integration of migrants moving to a new country. The first focused on Austria, while the second broadened the perspective to include all the countries of EU28. The first project aimed to produce finer-grained results – directly applicable by policymakers to the Austrian case -, whereas the second aimed to draw more sweeping conclusions about migration to new countries in general. The different levels of generality that characterized these two talks stemmed from the enlargement and the diversification of the data sets considered, something that still kept both grounded in a very specific context and thereby closer to the pole of ‘applied’ research. The research reported on in the third talk ‘Intersectional Inequalities in Social Networks’ (Complexity Science Hub, n.d.q), on the other hand, aimed to answer the question of how “information from our multidimensional identities” is integrated in social networks “to form connections with each other”, and, in turn, how preferences in creating social connections (e.g. preferring to bond with people similar to us – ‘homophily’) impact “intersectional inequalities of social capital”. In this work, therefore, social networks were studied at a rather abstract level, with mathematical structures (e.g. “multidimensional attributes vectors”) enabling the bracketing away of any specific context of application. In this case, the generality did not stem directly from an enlargement and diversification of the data sets (even if the data sets used, mostly for validation, were actually quite large and diverse), but from a mechanism of abstraction aimed at understanding the underlying properties of the system – valid for any network of social relations independently from the specific context of application.

Box 3 *The ‘applied’ – ‘foundational research’ spectrum: three examples*

Importantly, the two poles of the ‘applied’ - ‘foundational’ research spectrum do not play the same role at the CSH, nor is the ‘vertical axis’ completely flat in terms of epistemic authority. To be precise, my empirical work showed that, on one hand, the majority of research projects at the hub lean towards the ‘applied’ side and that yet it is ‘foundational research’ and its abstract tools that are accorded a more prestigious role – rising to a quasi-religious status. Between ‘caring about context’ and ‘caring about methods’, furthermore, the latter seems to be the more ‘successful’ mode within complexity science and at the CSH – with methods sometimes not even being “for problems, [but] only for making oneself better” (interview 3).

“I think the holy grail [at the CSH] is still finding some unifying theory, which is a physics mentality. Outside the Catholic church, who does this? I really admire the levels of abstraction they will develop to figure out the similarities between completely different systems and to find out what is the mathematical framework that would unify them. I think it's pretty cool. It's not what I do, unifying theories is not what a social scientist necessarily does.”

(Interview 6)

“I find that oftentimes the people that have been more successful in this field are precisely the people that don't truly care about the systems they're studying, but they use the systems as pragmatic examples for the kind of interaction they're trying to study. So, let's say that you're interested in cascades on networks. You might not care about supply chains per se, as a topic, but you think they are an interesting example of cascades on network.”

(Interview 7)

Taking stock of the last two sections, what emerged from my empirical work is that the ‘horizontal’ and ‘vertical’ axes of the CSH’s epistemology are in fact enmeshed and entwined, with the same asymmetries I have encountered in the ‘horizontal’ axis – where, again, more abstract approaches are, at least in some cases, given priority and authority over more contextual ones – carrying over and being reflected on the ‘vertical axis’. Incidentally, this also drastically complicates the notion of ‘quantitative methods’, by showing that it contains a wide range of practices, each with different affordances, motivations and status. Importantly, all my interviewees with a background that was not in physics or mathematics expressed at least some ambivalence with respect to this state of affairs, maybe supporting or even admiring the approaches based on mathematical and physical frameworks, but always clearly distancing themselves from their practice and often from their outcomes.

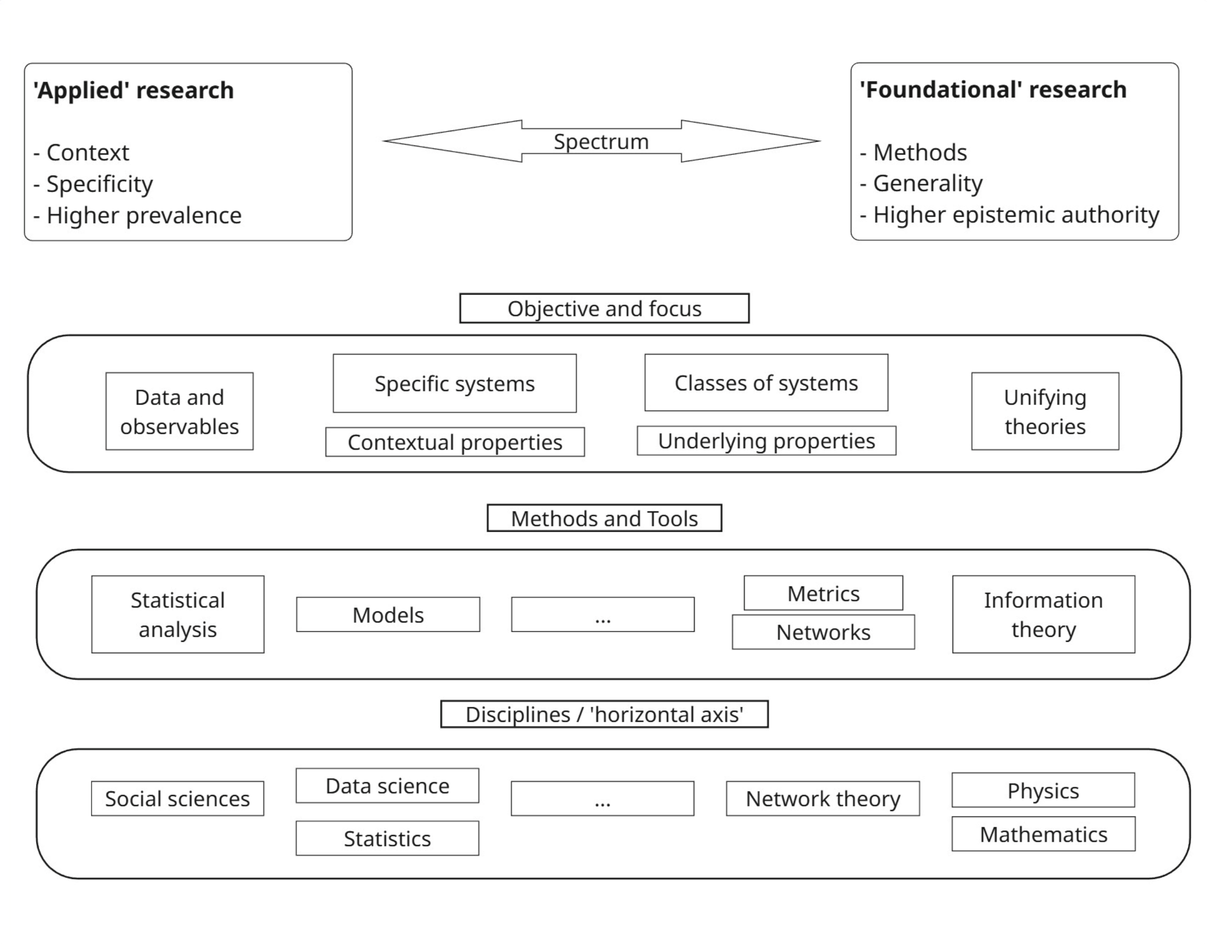

Figure 2 *Schematic representation of the 'applied' – 'foundational' research spectrum. While 'applied' research 'cares about context' and aims to understand specific systems through the analysis of data and observables, 'foundational' research strives for generality, 'cares about methods' and aims to develop unifying theories about classes of systems; while the former is more often practiced at the hub, the latter seems to be granted a higher epistemic authority. The last two boxes in this figure serve to convey that, as discussed above, the 'applied' – 'foundational' research spectrum is mirrored into a sort of ordering of practices, methods and disciplines and thereby enmeshed with the 'horizontal axis' of the CSH's epistemology. Of course, the examples provided in the last two boxes should be taken with a grain of salt – only as part of a schematic representation.*

6.3.1.4 Moving Upwards and Sideways: Quantifications, Simplifications, and Generalizations

"I try to always translate this more vague or ambiguous concepts and phenomena to numbers and do it properly."

(Interview 4)

In the last three sections, I have tried clarifying the coordinates of the CSH's and complexity science's epistemology – mapping it as if it was a plane. In this section, I instead focus on movement on this plane. For what I could gather in my empirical work, starting from data and 'observables' relevant to applications, it is always and crucially a process of quantification, mathematization and abstraction that allows to 'decontextualize' and move up along the 'applied' – 'foundational' spectrum, but also horizontally between areas of expertise. In this process, concepts and ideas from the target disciplines are transformed into mathematical objects, e.g. models and metrics, while context is progressively separated from the supposedly 'underlying properties' of the system. In modelling, this step is best exemplified by 'operationalization', the identification of quantitative proxies for concepts or ideas that are themselves not quantitative nor easily measurable (note that this issue is tightly bounded with issues concerning data, see **Box 4** below for a few examples).

"Many of us actually tend to do relatively complex research from the technical point of view... as in, we basically come up with our own methods all the time [...] and try to translate sociological concepts into metrics and theories, into models that are quantitative."

(Interview 4)

"Because you are talking about probabilistic statements, you want to test them with some kind of statistical framework. So, you end up trying to quantify stuff... which will be through proxies of the concepts that you have in mind, that organize how you think about the world."

(Interview 6)

Importantly, while this process of translation can happen at different scales and via different mechanisms, it is always affected by the tension I talked about in Section 6.2.2.3: that between constructing a faithful representation, which preserves at least some of the original complexity of the system, and a representation that is simple enough to make sense of it. In other words, operationalization always comes with simplifications.

"To understand the brain, we should not just reproduce it, but we should simplify it so that we, as humans, can understand it. [...] So, ideally as simple as possible to be able to understand something."

(Interview 5)

"The question becomes: we know our [modelling] assumptions are wrong because no two actors will behave the same, how can we simplify their behaviour so that we can actually do modelling? If we have to go to each person in the world and ask them how they choose what to buy, then modelling would be impossible. It would be a complete waste of time."

(Interview 7)

Most of my interviewees described the translation of phenomena and problems into mathematical language as a delicate process, entailing many choices, tradeoffs and challenges. In fact, not only did my interviewees agree that it is hard to 'measure' in general, but they also showed awareness that it is even harder to 'measure responsibly'. For example, one of them, talking about the struggle to attach a simple number to the very slippery notion of 'research output'[30], expressively commented "try to measure that!" (interview 3). Another interviewee, instead, argued that in complex systems "only one parcel or one aspect of the behavior" (interview 4) can be captured and invited to modesty with respect of the conclusions that can be drawn from quantitative approaches. In the face of these acknowledgements, my participation in several CSH talks revealed that in the practice of complexity science relatively little space is dedicated to discussing modelling choices and whether they are warranted or not. When a discussion did arise about a certain parameter or a certain proxy in the model, it was in almost all cases only a technical discussion. Therefore, a tension emerged between the fact that individual scientists seem to generally be aware of the limitations and risks of 'moves' along the axes of the CSH's epistemology and the fact that collectively and structurally (see 6.3.2.1) these issues seem to be given little space and priority.

"I'm going to be a bit maybe critical of myself and my peers, we often don't really know and/or have the tools to be sufficiently critical of the way we approach this. We are not trained in having that kind of sensitivity to thinking, 'Hey, is it really, is it really okay to model it this way? Like what are the political implications? What am I leaving out? [...] I'm not sure that we engage even with the question oftentimes. Because sometimes it's kind of very mechanical modelling, we approach this in a very mechanical way. And so, we don't have that kind of disciplinary part of like knowing why you should, why you shouldn't even ask the question."

(Interview 7)

As one of my interviewees commented, excessive simplifications and generalizations and excessive priority for methods over context, per se, are not problematic "until you start claiming that you can say something about the system. Then you really should think more" (interview 7). It is in light of this point that the claims of 'societal relevance' and applicability of complexity science should be analyzed with particular care (see 6.3.3).

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[30] It is worth noting that, in this case, my interviewee actually tried, with their research, to improve on more narrow indicators of 'research output', including in the measurement also unpublished works and other activities not directly related to publications.

6.3.2 Tensions and Struggles on the Way to 'Good Science'

6.3.2.1 Systemic Struggles: Academia as a Structural Constraint

"I think expertise is something you either have or you don't. Some people just don't have it. You can't ask a mathematician to explain to you why GDP can be a problematic measure. He doesn't know. He would need to learn this… and of course that means a lot of work for very little benefit in terms of publications. So, if you can avoid it, you avoid it."

(Interview 7)

Overall, my interviewees expressed several harsh critiques of academia and contemporary science – with one of them even describing the system as "completely broken" (interview 2). A common point of complaint was the fact that "we are producing too much" and then "everything goes forgotten" (interview 2). All the interviewees with whom I talked about academia, indeed, told me that scientists are under too much pressure to find new results, and that, therefore, sometimes published works are "correct, but not useful" (interview 2), "not that interesting" (interview 7) or even straight-up "false" (interview 6). With respect to the community of complexity science, moreover, pressure to publish sometimes results in research that is not "particularly complex system-y" (interview 7). Another common critique concerns the narrowness of accepted registers in publications. In particular, my interview partners found fault with the exclusion from publication of negative results, which "might be very useful" (interview 2) and, more specifically, of works that are based on scenario building (see 6.2.2.2) and are therefore considered "too opinionated" (interview 7).

"Of course, there's even the worst ones, which is just the papers you write because you have to publish. And those are just papers where you try to find a new data set to apply the thing you already know."

(Interview 7)

The metrics of academic evaluation were also the object of reproval: on one hand, the over-emphasis on citations, on the other, the priority given to linear academic trajectories over more complicated paths. The latter point is particularly relevant for scientists working in complexity science, as their backgrounds tend to always be tortuous and hard to put into boxes (see 6.2.3; 6.2.4.1).

“If you want to make a career you need to find a hot topic and follow this hot topic because if you do always the same things you become an expert and you get professorship.”

(Interview 2)

Importantly, especially for the discussion of the last section (6.3.1), my interviewees described all these issues as a constraint to the proper and careful practice of inter- and transdisciplinarity. For example, one of them stressed that “in many academics' lives there is not enough time to really study literature from other fields” (interview 5), while another shared that they were forced to stop reading into philosophy and sociology of science because they “needed to finish [their] PhD in computer science and needed to put things on the computers” (interview 3). A similar point, but in reference to the interaction between scientists and policymakers, was made by one of the senior researchers with whom I spoke.

“You learn a lot from that interaction [with policy makers]. It's not super appreciated in science if you make it and it's very time consuming. It doesn't give you papers, but it gives you a lot of insights. You learn from it… mostly modesty, I think.”

(Interview 6)

Finally, it is also important to note that the scientists I interviewed mostly felt they lack agency in relation to the academic system. While more than one stated they believed the system needs to be reformed, no one seemed to be able to imagine entry points from which to do it. Incidentally, one junior scientist said that he didn’t believe that “junior scientists have a lot of power within this system” (interview 7) and that maybe only professors could do something about it. Overall, the conversations about academia with my interview partners were haunted by a sense of frustration and desertion.

“I didn't apply anymore [for a professorship]. I don't care now about this fucking system. Everyone is complaining about the system, but no one does anything. Then I prefer to go on with it in this way. It's fine.”

(Interview 2)

6.3.2.2 Data Hunting and Data Struggles

As already mentioned in Section 6.2.2.1, complexity science has a pretty idiosyncratic relationship with experiments – which are pretty hard to run on complex systems. In light of this, data become a prime asset in research, and ‘hunting’ for data a big part of the practice of the scientists at the CSH. While data is, understandably, also considered as an essential basis for the practice of ‘good science’, the issue is to understand precisely what is meant by data, and especially by ‘good data’.

“I gathered a lot of data during my PhD, like hunting for data in about 19 different fields. It's like 200 million papers. Huge. And I was telling [my future PI at the CSH]: I love your methods, but they're really applied to small samples. I have data... I could, you know... I want to work on applying your methods to this big data. So, I started to collaborate with them.”

(Interview 3)

Proxies: as already discussed in 6.3.1.4, scientists at the hub often deal with concepts, notions or variables that are not easily – or not at all – quantifiable. In these situations, scientists need to resort to using proxies. Of course, different proxies will afford different data collection, and it is thereby inevitable that, when in doubt, quantities for which it is easier to collect data will be chosen as proxy. A somewhat archetypical example is Gross Domestic Product (GDP), for which a whole global data infrastructure exists – making it a very ‘comfortable’ proxy for the status of a country’s economy. Here, though, I discuss a more specific example from a CSH talk I attended: “Quantifying the Stability of Refugee Populations: A Case Study in Austria” (Complexity Science Hub, n.d.o). In this talk, the multidimensional and hard-to-pin-down concept of (in)stability of refugees is made into a quantified and unidimensional variable by taking ‘residential mobility’ as a proxy, i.e. how many times within a given timeframe refugees are changing their registered residency. Through this choice, official data becomes available and allows to provide precise quantitative estimates on the correlation between ‘(in)stability’ of refugees and some other of their attributes (e.g., gender or country of origin). Importantly, at no point during the talk or the subsequent discussion was the choice of this specific proxy questioned, or its implications discussed.

‘Fake’ Data: in some cases, extensive data might be available but only be ‘fake’ or ‘synthetic’, i.e. obtained through models and simulations that are only based on much smaller datasets. ‘Fake’ datasets have mainly two issues: first, the obvious risk of being much less representative of actual phenomena than they can appear; second, and relatedly, the complete impossibility of using them to study exceptional cases and deviations from the norm, like crises or shocks. Moreover, whether a dataset is ‘fake’ or not is sometimes opaque for users. The term ‘fake data’ was explicitly used in another CSH talk I attended: “Modelling Remittance Decisions and the Impact of Disasters” (Complexity Science Hub, n.d.r). Here, the speaker was attempting to study the correlation between international remittance flows, that is money sent to their home countries by migrants, and environmental disasters. The speaker explained that the dataset they had used at the beginning of their work was only later found out to be consisting of ‘fake data’. Therefore, with a substantial loss of time, in the end the study had to be carried out using other datasets.

Data Ownership and Institutional Reliance: who owns data? Are they willing to share it and how did they collect it? In many cases, these questions can be very thorny. The easiest answers can be found when data is owned by an institution, like a government. Yet, relying on institutions also implies that in some situations data, more or less by definition, is going to be almost impossible to collect. This case is exemplified in a particular clear fashion by the words of one of my interviewees.

"Of course, you know, we don't model the informal economy... but this is just because there's no data."

(Interview 7)

Box 4 *Three Examples of Data Struggles*

In practice, all sorts of challenges can emerge when trying to find the right data to answer a research question: for example, some things might not be translatable into data, data might not yet have been collected or, if collected, be hard or costly to access for a researcher. The quality of data can also be a major issue, as data can be incomplete, biased (hiding some very relevant features of the system under study) or 'fake' (in the sense of being derived from simulations or models and not directly from phenomena – see **Box 4**). My participation in many CSH talks confirmed the fact that 'data struggles' are part and parcel of research at the CSH, since a consistent share of almost each presentation and discussion was devoted to issues relating to the origin and quality of the data used.

"We needed two years of data collection. So, we had [one dataset], for which we had to download the data at midnight, every night, for two years. Whereas the [second dataset], they had a repository, so it was easier."

(Interview 2)

This situation is complicated by the tension between scientific questions and data, which partly resonates with the discussion above concerning the navigation of the horizontal and vertical axes of the CSH's epistemology (see 6.3.1). Indeed, another way to think about the spectrum 'applied' – 'foundational' research is to transpose it into the duality between data and scientific questions. In this metaphorical correspondence, data would be on the side of 'applied research', as it bounds research to context and warrants its applicability, and 'questions' on the 'foundational research' side, as they can be a driver of abstraction and allow to extrapolate general proprieties from specific case studies. Despite the fact that most of my interviewees agreed that questions should, at least ideally, have the priority over data and despite the existence of some voices that claim that it should be free-floating scientific questions to guide the research process as data does not anymore represent a bottleneck to research (see 6.2.2.1) – the reality I have encountered is much more complicated and nuanced, and encompasses a whole universe of practices, difficulties, and compromises. What data is available, of what kind and owned by whom are all issues that end up influencing both research process and research questions much more than some grand narratives of data success – in some cases subscribed by people at the CSH – would admit.

"Of course, we are happy if we get data of any kind and then we formulate the questions once we have a look at the data. But the ideal thing is I want to demonstrate or confute something and then I try to find the data that help me, and then I do the study. This is the logical thing that everyone should have in this field. And this you learn here"

(Interview 2)

"For bigger systems, of course, you don't have the same luxury. You cannot every time collect all the data about a country or organization. So, you need to rely more on found data – data that's already there and then shapes the questions you're asking."

(Interview 5)

Significantly, this predicament opens the door to a sort of symbiotic exchange between actors that own data and need to make sense of it – most commonly institutions, governments and organizations – and the CSH. On the one hand, this relation could be considered a form of fruitful transdisciplinary collaboration. On the other, it does ask some questions concerning the extent to which it can jeopardize the independence of research at the hub – especially because, in many cases, the actors that own the data are also the ones that fund the hub (see 6.1.3; **Table 7**).

"I think at the hub it's a great way that they're using this data because, even if it's not a theoretical question, it's some applied question that the Ministry of Health is asking: how can we improve healthcare to everyone in Austria? How does it depend on where they are located in supply chains? How can we prevent the collapse of the whole system? Which companies should we protect? Those are legitimate and great questions and analyzing this data is guided by them. Whatever ministry, whatever bank gave the data, it's not just blind exploration."

(Interview 5)

6.3.3 The Third Dimension: Societal Relevance

"How are we going to move our society from some catastrophe? How are we going to motivate people to be better? That is the question. I guess at least having that in mind is very important. Eventually your model should not only resemble something in the real world but actually try to explain something and help us as a society to do better."

(Interview 5)

As already discussed in Section 6.1, the CSH outward presentation (see **Box 1**) as well as its research (see 6.1.6) are both strongly focused on the production of knowledge that is relevant for society. In fact, basically all the CSH talks I attended discussed topics that can have a direct application in society. In a few cases, the talks even recounted research that had directly been commissioned by another institution – for example the Austrian Ministry of Interior in the case of a study on how migration affects demographic shifts and thereby public service demands. Furthermore, the speakers often emphasized the applicability of their research, making it a point to explain how it could turn out to be relevant for broader societal issues. These themes commonly occupied a considerable part of the discussion after the talk as well, as the audience sometimes questioned whether and to what extent the simplified models presented would be relevant to actual policy implementation, or asked clarifications about the scope of application the author had in mind. Overall, also considering the mapping of the CSH research topics (see 6.1.6) and my interviews, what emerged is that the scientists at the CSH almost invariably do research having societal relevance in mind, at least in one form or another, and that, moreover, they are often in close contact with societal actors, stakeholders and decision-makers – who often commission research at the hub and in some cases have regular meetings with scientists. Finally, research at the hub *is* 'societally relevant', at least insofar as it is actually used by policymakers, organizations and companies. This was, for example, very visible during the Covid pandemic, when some of the CSH scientists became famous in Austria for their opinions and interventions in the political debate.

"We have a project that is trying to help the Albanian government figure out what development plans they could have and what development trajectories might exist for their different regions. We have a project about Ukraine and how Ukraine could diversify away from Russian value chains, mobilizing the human capital they have abroad right now."

(Interview 6)

"World Bank is using this algorithm [on which my interviewee previously worked] to make forecasts for nations. And I know also that Sweden asked to use this algorithm to understand where to invest. Of course, with the algorithm you can have forecasts. And it seems that the forecasts this algorithm provides are the best ones with respect to other algorithms."

(Interview 2)

Staying within the metaphor I used above according to which the epistemology of the CSH can be represented as a two-dimensional plane, I imagine societal interventions as a third dimension of the life at the CSH: potentially connected to everything has been said before but also moving on a broader set of coordinates. After already having discussed how the possibility of working on societal relevance is a key motivating factor for scientists to work in complexity science (see 6.2.4.2) – a source of purpose for scientists at the CSH –, in this section I attempt to clarify how researchers make sense of this 'third dimension', how they relate to it and include it in their everyday research, what kind of societal intervention they strive for and what they see as a challenge or an impairment to it.

6.3.3.1 Three Ideal Types of Societal Interventions

"Sometimes policy makers have questions about how to implement or whether to implement this or that policy. [...] Between going completely in the dark by pure intuition and approaching it through data and through some results that we could have obtained through our science... I would say that it's of course always better to do it with data and systematic research."

(Interview 4)

All my interviewees agreed that science should in some way or another intervene in society. In many cases, complexity science and the CSH were framed to me as platforms that afford this possibility much more than traditional disciplines. Some of the researchers at the hub focus on topics that they directly care about, with the aim of improving the lived experiences of certain groups or in general contributing to the creation of a better world. In other cases, scientists imagine themselves as producing 'fundamental' knowledge and thereby feel less attached to a direct and linear application of their work. Yet, even in these latter cases, societal relevance is always present in researchers' discourse. To further clarify the nuances of how scientists imagine the relation between their research and its application, I introduce here three different ideal types of societal intervention that I have encountered in my empirical work. As ideal, these types are not rigid and stable but should rather be interpreted as somewhat caricatural descriptions that I use to make sense of my empirical material. In fact, researchers do not necessarily believe that one type is the correct one and often switch between different ideal types, sometimes even within the same conversation.

The first type is about 'providing evidence' on the existence of certain phenomena, "showing that the problem exists, because a lot of people don't know" (interview 3). In most cases, this type is associated with the epistemic authority of numbers, as quantitative studies are imagined as, and often are in practice, a way to settle an issue and provide 'objective' knowledge on how things really are. Note that researchers that subscribe to this type are perfectly aware that there might be a discrepancy between what they try to prove with their work and how people will perceive it, that "even when we format the message in a way that everybody can understand [...], people still don't change their mind." (interview 5). According to type one, societally relevant knowledge is precise and robust, quantitative and objective – and it is produced with an activist aim in mind.

"We have been invited to give talks about our work. [...] And it's really nice because we present this big data analysis of gender inequality, and then they are surprised... they were telling us: 'it's so cool for us to see that this is systemic. Instead of individual'. Because then you don't go crazy, you feel represented."

(Interview 3)

The second ideal type of intervention is about 'understanding'. In this case, the ultimate goal of changing things is perceived as too far and hard to reach. Therefore, an intermediate step that researchers settle for is arriving at a better understanding of the system under study, in order to eventually enable a more direct intervention at a later stage. In the second type, societally relevant knowledge is deep knowledge about a phenomenon or a system, which potentially might provide insights into how to change it further down the line. In line with tropes from the field of complexity science more broadly, this type can entail a sort of double-thinking when referring to complex systems: on one hand, complexity is very hard to tame; on the other, there is always the possibility that the correct form of investigation might eventually accumulate to find the right lever to move the system in the right direction. In other words, even if it is imagined as an incredibly hard task, the ultimate goal is still that of 'cracking the system'.

"It's so difficult to change or disrupt the system with your analysis, at least our analysis... so mainly we are trying to understand it, more than changing it... because it's so big that it's difficult to change it. You need a lot of resources to change it and from different directions."

(Interview 3)

"The intention of this book was exactly [to] help prepare the way for new progress in a number of directions where we are currently stuck. [...] We believe that pushing these directions will finally help to develop a more coherent theory of complex systems than we currently have. And perhaps more importantly, it will help to finally establish a coherent framework for quantifying, monitoring, and managing systemic risk, resilience, robustness, and efficiency."

(Thurner et al., 2018; p. 399)

The first two ideal types of societal intervention link directly to the third one, which is somewhat overarching as it describes the general attitude that some researchers believe science should have towards society. The third type is about 'informing'. In this ideal type, researchers imagine science as providing useful knowledge to society, which, in turn is "not forced to use anything you find" (interview 2). According to interviewees that were referring to this type, science should not "recommend what should be done but should be able to inform people on how we function" (interview 5), even if the risk exists that people "don't care", in which case they will at least have taken their "decision consciously" (interview 3). According to type three, societally relevant knowledge is neutral but provides important information about how things, people or societies work. It is not produced with a specific activist goal in mind, but it is still geared towards the general good.

"Physicists and medical doctors are informing the society in various ways through schools, through educational programs, [through] knowledge that these different sciences have acquired and give to people so that they can build better bridges and not kill themselves with electrocution and not spread diseases. And that's essential for us to function as a society. But social science is a bit behind. There are maybe some self-help books. There is some political theory… of course there is something there. But really, we are not yet good in telling people how to nurture a healthy society. […] And my goal, my hope, would be that we progress to some level of certainty about how complex social systems operate so that we can teach it in schools."

(Interview 5)

Ideal types of societal intervention	**Characteristics**	**Imagination of societally relevant knowledge**
Providing evidence	Concerned with providing evidence of phenomena through numbers and quantitative descriptions	Activist knowledge that can be used to convince people that issues (e.g. 'intersectionality' or 'gender discrimination') are real
Understanding	Since intervening on complex systems is extremely hard, the focus is on understanding them better	Deep knowledge about society, which might or might not directly help to find the right 'levers' to intervene in it
Informing	Rooted in a linear model of the science – society relation, this type imagines knowledge production as finalized to discovering objective facts and then sharing them with society	Basic and neutral knowledge that can inform people about their functioning or the functioning of social systems and could thus contribute to the common good

Table 9 *The three ideal types of societal intervention I encountered in my empirical work.*

6.3.3.2 Challenges and Risks in Doing Societally Relevant Science

"I kind of reconciled myself as an individual: making huge changes, like systemic changes, takes a lot of time."

(Interview 3)

The ideal types mentioned above clash with a reality of challenges and risks in the application of societal relevance – something which researchers are generally very aware of. First of all, more than one interviewee mentioned the gap between their work, "very theoretical or for scientists" and "real world interventions" (interview 3). Following a relatively simple 'translation model', some emphasized that the connection with stakeholders and policy makers is extremely important for scientific work to have a positive effect on society – insofar as societal actors will otherwise ignore or not understand scientific results. Even in the best cases, though, "you never connect to the big bosses, the ones who decide. If you are lucky, it's a two-step process" (interview 2).

"As physicists, we studied the spreading of diseases, and we have many models on that. We have studied it for decades. Then it was Covid, and politicians could just have read the abstracts of the papers and understand immediately what to do. They ignored it, and there was chaos. The point is you can produce whatever you want if the politicians are not reading or they don't understand what you are doing. It's not useful. [...] So, the connection with stakeholders it's very important."

(Interview 2)

Another researcher had a more nuanced imagination of how the interactions with societal actors play out but still emphasized its complicated nature. Indeed, they pointed to all sorts of mismatches between the expectations of researchers and those of societal actors: from the over interpretation of the scientific results to the 'crumbling' of academic ideas once put to the test in the real world. In this case, fruitful collaborations with societal actors were described as requiring something more than translation and proximity and entailing an iterative process through which the very results of research would be under scrutiny.

"I know that all of these academic ideas typically crumble once you have to look at the implementation of them. The categories don't fit... you haven't considered that the policy levers that are available are very different from the ones that you want to move. So, there's a lot of iterative work in that. [...] You cannot just drop [academic knowledge to the policy world]. You need to hand it off and stay with the people who are using it and figure out that you actually gave

something useless to them or prevent that they start drawing conclusions that were never warranted from what you did."

(Interview 6)

Another point that was insistently mentioned as a challenge in producing applicable knowledge was the fact that societal interventions can have unintended and unexpected consequences. For one of my junior interviewees, for example, the "biggest fear [was] convincing some policymakers that [they] should implement X policy and then it completely backfiring" (interview 4). Since more than one of my interviewees expressed the idea that knowledge is in some sense neutral, i.e. that the same research can have both negative and positive consequences, they also expressed concern about not knowing how the results of their work would be used. In some cases, researchers reacted to this by inviting to precaution and modesty in the dissemination and application of scientific results. By the same token, in some cases they also made an argument for having knowledge 'out in the open' at least.

"O*ne of the sides of my project wants to decrease the bias towards minorities for face recognition. But the ethical problem that I see is that there is a lot of work in face recognition used in weapons, in drone attacks. Then you ask yourself if you want to improve it… For that as well? It is so problematic… Because it could solve some problems but also make other worse."*

(interview 3)

"Somebody can say, especially my physicist colleagues who are still feeling guilty about the nuclear bomb, 'this knowledge can be used to manipulate people if you publish this'. I'm just a scientist, my budget is small, there are multibillion dollar companies that spend all their time to learn how to manipulate people… the least I can do is to put this out in the open so that people can learn. […] So, my answer to the concern that this is potentially dangerous is that it is better that knowledge is out in the open than it is hidden and just used by some."

(Interview 5)

Finally, a potential obstacle between the imagination and the practice of societally relevant research was identified in the performativity of quantitative interventions – with explicit reference to Godhart's law from economics (see 2.3.1.3). In this case, the problem was identified with the strong focus on proving "that the policy was effective" and on measuring "how effective it was" (interview 7). This point of view questioned the idea that the effect of research on society can or should be quantified and thereby implicitly complicated the very notion of 'societal intervention', as well as its assessment and evaluation.

“If a measure is used, then the measure will be corrupted. So, when a measure is used to make decisions, it's definitely very dangerous. When the measure is just used to measure, it's less clear to me that it's necessarily dangerous.”

(Interview 7)

7 Discussion

In this last section before the conclusion, I build on the literature review and empirical chapter to try and answer my research questions (see 4). While inevitably partial and provisional, these answers are, to the best of my possibilities, faithful to what I witnessed at the CSH. By the same token, they tend to still share quite a high degree of openness. Section 7.1, which speaks to SQ1, is a review of all my material about complexity science and the CSH that showcase them as specific and situated institutions of knowledge production. In Section 7.2, I first discuss some general elements of the epistemology of the CSH and then move on to discuss SQ2 and how different knowledge domains and disciplines are integrated at the hub. Section 7.3 is about societal relevance and thereby connects directly with SQ3. Therein, I discuss the significance of the articulation of societal relevance for complexity science vis-à-vis the broader shifting relations between science and society. In Section 7.4, finally, I try to tie together all the loose ends of this discussion and attempt to formulate an answer to my main research question.

7.1 The CSH as an Idiosyncratic Institutional Space: Affordances and Challenges of Flexibility and Outsiderness

Looking back at my discussion of the CSH, a few institutional and organizational elements immediately stick out. First of all, throughout my empirical work I identified an impressive continuity between the CSH and the SFI. This was clearly manifest in a concrete connection between the two institutes, for example in terms of direct collaborations and of the inclusion of many SFI scholars among the CSH external faculty (see 6.1.4). Moreover, a strong ideological correspondence also emerged, with the SFI being omnipresent, almost invariably in very positive terms, in the discourse of the scientists working at the CSH. It is worth mentioning that for more than one of my interviewees the SFI and its most famous scientists (e.g., Gell-Mann, Anderson, West) represented nothing short than ‘childhood heroes’. This corroborates Williams’ (2012) claims that complexity science brought about a new way of communicating science, best represented by the publication of popular science books by famous personalities active at the SFI (see footnote 14). Interestingly, the CSH seems to have followed in the SFI’s tradition also in respect to the attention devoted to dissemination and outreach, as showcased by the prominence and vast activity of its communication team (see 6.1.5). Another key element that emerged from my analysis is the CSH's embeddedness in the Austrian academic and institutional context. This is testified by the close collaboration with many Austrian universities and the mix of private and public funding that the CSH benefits from (see 6.1.2; 6.1.3). Again, this resonates with the findings of Williams (2012), who identified 'quasi-stable funding', "pure science with market dynamics" (Williams, 2012; p. 153), as one of the main features of complexity science (see 2.4.2.1). All these elements, in particular the partial penetration of private actors and market logics within an academic setting and the increased relevance given to the mediatization of science, configure the CSH as a good example of the process of 'commercialization' that characterizes contemporary academia (see 2.2). At the same time, the CSH seems to have appropriated and reinterpreted this overarching tendency, adding to it its unique touch comprising of both organizational and epistemological elements.

For what concerns institutional flexibility, my research shows that the CSH again shares much with the field of complexity science writ large. To begin with, the 'structural essential tension' between institutional flexibility and institutional stability (Turner et al., 2015; see 2.2.1.1), which Williams' (2012) described as unfolding in complexity science between innovativeness and credibility (see 2.4.2.1), was a recurring theme throughout my empirical work. On one hand, at an institutional level, this tension is once more dealt at the CSH in continuity with the SFI. For example, the CSH directly borrows several of the 'legitimization strategies' identified by Li Vigni (2021c) – most notably the organization of many schools, workshops and talks exactly in the spirit of the New Mexican institute (see 6.1.4). In the language of Molyneux-Hodgson and Meyer (2009), these can be parsed as 'community making devices' that contribute to the 'stickiness' of 'emerging disciplines' (see 2.2.1.2). In the face of these tendencies, though, my research confirmed that complexity science is by no means an institutionalized discipline, as already shown by Li Vigni (2021c). On the other hand, at an individual level, the tension between institutional flexibility and institutional stability transpired clearly in the accounts of my interviewees – who alternatively highlighted its affordances or the challenges it entails (see 6.2.3; 6.2.4.1). Deeply aware of the lack of organizational consistency of complexity science, researchers at the CSH do recognize the existence of a rather concrete complexity science community – to which both individually and collectively they are connected and attached.

In a similar fashion, my interview partners also expressed ambiguous feelings of disciplinary belonging, struggling to feel at home in complexity science but also in their 'home disciplines'. In order to navigate this 'affective essential tension' (Turner et al., 2015; see 2.2.1.1), my interviewees were in most cases ‘juggling multiple identities’ and engaging in the ‘identity work’ that Molyneux-Hodgson and Meyer (2009; see 2.2.1.2) described as characterizing ‘emerging disciplines’ – something which also confirms Li Vigni’s (2021a; see 2.4.2.2) results about complexity science. Yet, even if at times challenging and possibly jeopardizing of a scientific career, the disciplinary porosity of complexity science (see Williams, 2012) was often described by my interviewees as an opportunity to ‘get around boundaries’ (Williams, 2012). Importantly, my research showed that for many researchers this characteristic of complexity science is a motivation and driver of purpose – either because it allows natural scientists to carry out what they perceive as societally relevant research, or, more generally, because it allows to pursue research interests with less constraints than it is usually allowed in academia (see 6.2.4).

In fact, one of the most important themes in my interviews was the appreciation of complexity science, in general, and of the CSH, specifically, as places imagined and experienced as alternatives to traditional academia. In some ways, this confirms Li Vigni’s (2021a; see 2.4.2.2) description of complexity science as a ‘scientific platform’: “a meeting point between different specialties, which, on the basis of a flexible common ground, pursue together shared or parallel socio-epistemic objectives”. At the same time, I believe my research to also complicate this picture. Indeed, my empirical material shows researchers as lively actors moved by often contrasting motivations who, if they sometimes might deploy complexity science strategically, most of the times do not do so for a straightforward benefit in academic competition. In other words, if the choice of working in complexity science and at the CSH is made out of opportunity, this appears to me to be much more the result of contingency and *bona fide* interest rather than a strictly agonistic attempt to optimize one own’s research career. Vis-à-vis the systemic shortcomings of academia, of which my interviewees were generally deeply aware of, the flexibility and porosity of complexity science can be parsed as an interesting attempt to address some of researchers’ most important needs: those for curiosity and relevance. This reading resonates with Fochler’s (2016a; p. 259) and Fochler and Sigl’s (2018) results concerning scientists working in biotechnology companies, whose professional paths are also “deeply influenced by their perception of deficiencies in the conditions for epistemic work in contemporary academia”. Of course, this does not mean that complexity science and the CSH can *tout court* be understood as positive examples of scientific practice. In fact, the institutional and organizational characteristics of the CSH also have, in combination with the practical circumstances in which they unfold, far reaching epistemological and political consequences. In these regards, my analysis tends to be a bit more dire.

7.2 Epistemology and Interdisciplinarity at the CSH: From Integration to Imperialism

Concerning the CSH's epistemology, my research largely confirms the results from the literature (see 2.4.3). For example, complexity is defined at the CSH in an open and broad fashion, mostly in quantitative terms and with a jargon directly borrowed from, or at least inspired by, mathematics and physics (see 6.2.1). Similarly, my research confirms the relative narrowness of the methodological toolbox of complexity science, with a strong predominance of quantitative methods over qualitative ones (see 6.3.1.1). Yet, within the realm of quantitative methods, I found scientists at the CSH to perceive their practice as innovative, as a clear break – even a paradigmatic one – with other disciplines (see 6.2.2). This perception mostly hinges upon the claim that complexity science acknowledges complexity and uncertainty more than traditional reductionist approaches do – with far-reaching consequences for the role of predictions and, to a lesser extent, data and experiments. This is perfectly in line with Williams' (2012) findings, which identify 'difference' in modelling as one of the defining traits of complexity science (see 2.4.3.2). Within the realm of quantitative methods, scientific practice at the CSH also emerged as granular and plural: encompassing a great variety of techniques and levels of abstraction (see 6.1.6; 6.3.1.3; 6.3.1.4). Given the small size of my research, I could only follow this multiplicity through the opposition between 'applied' and 'foundational' research, thereby only beginning to confirm Li Vigni's (2020; 2021b; see 2.4.3.2) much more fine-grained results. Finally, my empirical work also bears witness of the recent transformations of quantitative and data-centred methods (see 2.3), as I found network theory and data science to be two of the main areas of research at the CSH – a clear testimony of what Barabási (2012) terms the 'network takeover' and what Li Vigni (2021c) labels as the second phase in the history of complexity science (see 2.4.1).

Superficially, the relevance of data and quantitative methods at the CSH is hardly surprising. Clearly, it can be connected with the high epistemic authority of quantitative methods (see 2.3.1.2), to notions of 'objectivity', 'rigor' and unambiguity, and to the recent shifts towards data-driven research (see 2.3.3). Indeed, many of these reasons were explicitly mentioned by my interviewees (see 6.3.1.1). At the same time, an STS perspective can add much to this discussion. For example, bringing attention to the effects of 'retro-performativity' (Appadurai, 2016; Islam, 2022; see 2.3.1.3), the literature from sociology and ethics of quantification allows to problematize the choice of defining complexity in a strictly mathematico-physicalist language. Similarly, the concept of 'telic-performativity' (Islam, 2022; see 2.3.1.3), sociological counterpart of Goodhart's law from economics (Goodhart, 1984), can help to critically analyse the practice of using purely quantitative research to inform policy interventions – something that seems to be practiced quite consistently at the hub and that was pointed out as problematic also by one of my interviewees (see 6.3.3.2). Furthermore, my research also testifies to the “expertise, discipline, [and] coordination” (Espeland & Stevens, 2008; p. 411) entailed by quantification ‘work’ (see 2.3.1.1), which I was able to trace in 1) the tension between simplification and complexity that runs through the modelling practices of the CSH researchers (see 6.2.2.3), 2) the process of ‘operationalization’ that attaches quantitative proxies to otherwise ‘ambiguous’ sociological concepts (see 6.3.1.4), and 3) more generally in all the steps of the process of abstraction and decontextualization that allows scientists at the CSH to navigate what I have termed ‘all-encompassing epistemology’ (see 6.3.1).

The move of comparing my empirical material and the State of the Art is enlightening concerning data as well. Indeed, while heavily promissory narratives around ‘big data’ are definitely present at the CSH (see 6.2.2.1; 6.3.2.2), on the ground I found the situation to be much more complicated – resembling closely the one described by Leonelli (2014) for biology (see 2.3.3.2). To begin with, during my research I have encountered many ‘data struggles’ (see 6.3.2.2), which serve to debunk the notion of ‘raw data’ and rather showcase how data is ‘constructed’. Indeed, data is often ‘hunted’ for by researchers at the CSH – who also have to select between different sources, assess their respective qualities, and ultimately decide what data is ‘good’, or ‘good enough’, for their research. It is also worth noting that most of these ‘data struggles’ can be parsed as ‘data frictions’ (Edwards, 2013; see 2.3.3.1), as they often arise at the junction of different knowledge domains – for example at the interface between complexity science and the institutions that collect and own data. Overall, my results show that data is not ‘that which is given’ at the CSH and they support a ‘relational understanding’ of data (Leonelli, 2015; 2020; see2.3.3.1). As a byproduct, this perspective also showcases the fact that ‘data struggles’ are all but inconsequential and that they can have substantial implications on research questions and research practices (see 6.3.2.2).

Finally, what can be said about interdisciplinarity? On one hand, the CSH’s positioning is clear: interdisciplinarity is an extremely important feature of fruitful scientific work. This point of view emerged basically in all my interviews and is advertised extensively on the CSH’s website (see **Box 1Error! Reference source not found.**). On paper, the aspirations of interdisciplinarity are embedded into the scientific activity of the hub through an extremely broad scope of research – the ‘horizontal axis’ of the CSH’s ‘all-encompassing’ epistemology (see 6.3.1). Yet, my work showed that the practice of interdisciplinarity at the CSH is all but smooth, and it is affected by the tensions between ‘caring for methods’ and ‘caring for context’ (see 6.3.1.2) and that between a careful engagement with literature from other disciplines and its disregard (see 6.3.1.3).

Crucially, I found quantitative methods to be a fundamental compass in the resolution of such tensions, functioning as 'integration machines' (Igelsböck, 2016; Schikowitz, 2020; see 2.2.2) that allow the translation and, in some cases, the appropriation of topics and concepts across disciplinary boundaries (see 6.3.1.2; 6.3.1.4). This also resonates with the literature on mathematical models (see 2.3.2.1), which often interprets them as inhabiting a space in-between different knowledge domains and levels of analysis. Complexity science, in fact, seems to live in a similar interstitial space, since it is the tension between being grounded in context and striving for generalization that sets apart the researchers at the CSH from both purely applied researchers and from purely theoretical ones. Importantly, I found quantitative methods to also induce a sort of hierarchy of disciplines (see 6.3.1.2; 6.3.1.3), with abstract quantitative methods and the disciplines they are borrowed from being valued more than those that are perceived as more 'applied'. Incidentally, this seems to also reflect on an individual sense on not-exactly-belonging for non-mathematicians and non-physicists within complexity science (see 6.2.3).

The central issue here seems to be about how to approach what Turner et al. (2015; cfr 2.2.1.1) denote 'epistemic essential tension' between unity and plurality, or, referring to the framework of 'trading zones' (Galison, 1996; 1997; see 2.2.1.1), about how to navigate the two axes of coercion-collaboration and of homo-heterogeneity (Collins et al., 2007). To what extent are the different disciplines and approaches that formally coexist within complexity science accorded an equal level of epistemic authority? How much is plurality actually valued within this field, vis-à-vis the need of homogeneity and unity or even a sort of 'integration imperative' (Klenk & Meehan, 2015; see 2.2.2)? In some sense, the situation mirrors what I already discussed in relation to the roles of models as 'mediators' (see 2.3.2.1). They, too, like complexity science, depending on *how* they are deployed can either represent productive 'carriers of interdisciplinarity' (Mattila, 2005) or 'equalizing but exclusive integration machines' (Schikowitz, 2020).My research showed that the disciplinary porosity and institutional flexibility of complexity science, which my work confirmed as institutional characteristics of the CSH, do not seem to be matched by a corresponding plurality in epistemological terms (see 6.3.1). On the contrary, more than one of my interviewees described as careless or unreflective the engagement of complexity scientists with other disciplines and their literature (6.3.1.3). Overall, my empirical work showed that interdisciplinary collaborations at the CSH are affected by heavy asymmetries and that quantitative approaches are rather used to suppress diversity rather than to provide a full account of complexity (see 6.3.1.26.3.1). This partly confirms the accusation of 'imperialism' moved by Li Vigni (2023; see 2.4.3.2) towards complexity science, which the author has described as forcing a narrow methodological repertoire on the disciplines it aims to 'formalize' (most eminently the 'soft sciences') and enroll. For all these reasons, both complexity science and the CSH can largely be described as appropriating, rather than integrating, multiple knowledge perspectives. In the face of this general critique, my research also identified a whole range of different points of views and sensibilities that coexist at the hub concerning the relation between quantitative and qualitative methods (see 6.3.1.1) and between different disciplines (see 6.3.1.2). What is more, most scientists at the CSH show a genuine motivation to broaden their expertise and collaborate with researchers from other disciplines, as well as a rather deep awareness of their own positionality.

Informed by my research and by a valuation studies perspective (3.2), I believe this tension between rhetorical and practical level and between individual aspirations and collective behaviour, to mainly hinge upon two factors. The first factor is the dominant 'valuation regime' within academia (see 3.2.2; 6.3.2.1). Expressing this point in a valuation studies language (3.2), one could say that as long as the overall system in which valuations and choices take place is hierarchical (as opposed to heterarchical, see 3.2.1), it matters little whether scientists personally relate to or believe in multiple 'evaluative principles' (for example, their interest in expanding their expertise, or their appreciation for the introduction of qualitative elements within their research process). Indeed, if multiplicity is not valued at a systemic level, the imperatives to publish and optimize that shape academic valuation (see 2.2) end up representing an insurmountable bottleneck in researchers' lives and practices – irremediably impinging on their ability to practice interdisciplinarity carefully and caringly. This is in line with the findings of scholars (Felt et al. 2013; Felt et al., 2016; Fochler et al., 2016; see 2.2.2) who have criticized the narrowness of the valuation repertoires in academia and have pointed to the current state of academic assessment as profoundly damaging tor inter- and transdisciplinary research. That the dynamics of the overall academic 'valuation regime' even affect the CSH – an institutional space expressly built as an alternative to academia and as independent from its tradition – is only testimony to their overpowering omnipresence. The second factor is the persistence of the uncontested epistemic authority of quantitative methods, which skews valuations towards what is measurable and quantifiable and thereby exerts a subtle but persistent ordering force onto all epistemic practices (2.3.1.2). The compounded effect of these two factors, I believe, can explain the bypassing of interdisciplinary qualms in epistemic valuations performed by scientists at the CSH, in favour of what can superficially be described as reckless, and 'imperialistic', appropriation. In words that might leave a stronger impression on complexity scientists: caught within structures that do not understand nor value multiplicity, the easiest if not only option for researchers at the CSH seems to be, quite paradoxically, to simplify complexity and resort to reductionism: to "subordinate every aspect of reality to the same interpretative key" (Israel, 2005; p. 555; see 2.4.3.2).

7.3 Societal Relevance at the CSH: Bringing Complexity Science to Society

My empirical work shows that societal relevance is a central element of the CSH's identity (see 6.3.3). While they might imagine or practice different ideal types of societal intervention (see **Table 9**), scientists at the hub are overall very concerned about the applicability and usefulness of their work. Importantly, the possibility afforded by complexity science and the CSH to produce knowledge about societally relevant issues emerged as a key motivating factor to work in complexity science – especially for scientists coming from disciplines like computer science and physics (see 6.2.4.2). Nevertheless, this relationship is all but straightforward and it is affected by more than one challenge (see 6.3.3.2). Besides systemic elements again impairing careful engagements with society (see 6.3.2.1), my interviewees generally expressed a dual positioning with respect to societal intervention. On one hand, it was described as something that motivates them and provides purpose, on the other hand the possibility of actually intervening in society still felt very distant and hard to achieve – riddled with risks (e.g., unintended consequences) and mismatches (e.g., between researchers' proposals and the actual "policy levers"). This might partly be explained by the fact that my research found the CSH to mostly practice its interaction with society through a 'linear translation model' (see Felt et al., 2016; 2.2.2.1). In this model, the scientific and societal arenas are clearly separated and, while societal actors might initially formulate the research problem, ultimately knowledge flows one-directionally (after translation) from science to society. A good example of this model is commissioned work, a great deal of which is indeed practiced at the hub (see 6.1.3). Of course, there are some exceptions. For example, one of my interviewees did highlight the fact working with societal actors is an iterative process and that, oftentimes, knowledge flows the other way around (see 6.3.3.2). Moreover, not too surprisingly given the discussion in Section 6.3.2.2, data is surely one aspect with respect to which the interaction between the CSH and society does consistently resemble a 'delimited neutral arena model' (see Felt et al., 2016; 2.2.2.1) – according to which societal actors possess key knowledge or information and are thereby let into the research process.

Crucially, compared to what is discussed in existing literature about complexity science (see 2.4), the focus on societal relevance seems to be much more marked at the CSH – and possibly in recent-days complexity science more generally. Arguably, this is a reflection of the broader transformations in the relation between science and society (see 2.1; 2.2.2). In fact, the situation closely resembles the one discussed by Bieszczad et al. (2025; see 2.2.2; 2.2.2.2) in relation to the field of deep sea research. In a similar fashion, indeed, complexity science seems to also be undergoing a transformation finalized to the better articulation of the societal relevance of the knowledge it produces. Reading my results through the work of Brunet et al. (2025; see 2.2.2.2), it is possible to appreciate the different levels at which this transformation plays out, as societal relevance is 'done' at the CSH through all four modes identified by these authors. Indeed, it affects 1) the disciplinary organization and epistemic practices of the hub; 2) how much and how societal actors are engaged in the scientific process; 3) the relevance given to the demands and needs of political and funding institutions; and 4) the positioning of the CSH with respect to other universities and academia writ large. Another side of the multifaceted articulation of societal relevance at the CSH is that both 'fundamental knowledge' and 'directed knowledge' (Bieszczad et al., 2025) play a role in it. Indeed, among the ideal types of societal intervention that I identified in my research (see **Table 9**), 'informing' strictly concerns 'fundamental knowledge', while 'providing evidence' strictly concerns 'directed knowledge'. The third type, 'understanding', is instead ambivalent – as it was alternatively used to refer to 'fundamental knowledge' (more commonly in my interviews) or 'directed knowledge' (more commonly in the 'CSH Talks' I attended; see **Table 5**). The association between the 'understanding' ideal type and 'directed knowledge', interestingly, was always present in the case of commissioned research. In this case, the hub was framed as potentially possessing the ability to 'see through' the complexity of the issues that commissioners needed help to solve (e.g. migration in the case of the Interior Ministry).

In analyzing the relation between the CSH and society, finally, it is important to emphasize the economic (see 6.1.3) and infrastructural dependence (see 6.1.2; 6.3.2.2) of the hub on the actors that finance it and with whom it is partnered with. This is in line with the results of Williams (2012) and the discussion in Section 7.1, which show that the institutional configuration of complexity science, generally, and the CSH, specifically, creates the need of establishing business-like relations with funding institutions and research partners – be them private or public. As highlighted in Section 6.3.2.2 and as showcased by some of the CSH talks I attended (**Table 5**), such entanglements have an effect on the CSH at least in regard to the formulation and framing of scientific questions. The limited scope of my research did not allow me to investigate whether this should be simply parsed as a case of virtuous transdisciplinary collaborations or might, in some cases, run the risk of jeopardizing the independence of the institute. At any rate, the CSH provides an extremely interesting example of how the relations between academia and society are evolving and of how the former is being restructured in order to be, at least formally, more responsive to the needs of the latter. In particular, the CSH shows that these ideas can be mobilized by academic actors in order to create and shape their identity – often with a direct return in terms of visibility and funding and, at the same time, enabling researchers to find meaning in their work.

7.4 Living and Pursuing 'Good Science' at the CSH: Settling for 'Good Enough Science'

How do scientists working at the CSH narratively reconstruct and valuate 'good science'? This was my main research question (see 4). Through the answers to its three sub questions, which I have tried to formulate in the last three sections, I have begun to put together the pieces of how 'good science' is perceived, strived for, and generally valuated at the CSH. Even if there cannot be a simple answer to this question, I would like to engage with it by emphasizing a few important points.

First of all, I want to circle back to the concepts of epistemic living spaces (Felt, 2009; Felt & Fochler, 2012; see 3.1) and 'good science' (see 3.2) and try to analyse the CSH from this theoretical perspectives. In my work, I have tried to trace the multiple dimensions that compose the CSH as a specific epistemic living space: the epistemic, the material and spatial, the temporal, the symbolic, and the social. Some were easier to access, some were harder. Yet I still believe my description to at least partially touch on all of them. Through my conversations with scientists working at the hub, a multifaceted picture emerged. The CSH is generally perceived by the scientists working there as place full of possibility – a feeling that is mostly constructed in opposition to academia and spans all five dimensions of epistemic living spaces. Despite some confusions and some struggles in the 'orientation work' needed to understand the boundaries of their epistemic living space – and the presence, in some cases, of some slight undertones of a sense of not-exactly-belonging (6.2.3)–, my interview partners generally expressed positive feelings about the CSH and complexity science. As for 'boundary work', confirming Williams' (2012; see 2.4.2.1) and Li Vigni's (2021a; see 2.4.2.2) results, the researchers I interviewed seem to mostly engage in negative boundary work, as they again construct their identities and their sense of belonging in opposition to academia. In other words, they do not necessarily attempt to reshape the epistemic living space in which they currently live but rather narrated their engagement with complexity science as a break – a sort of extreme 'boundary work' – with their previous epistemic living spaces, their 'home disciplines'.

For what concerns 'good science', my results are in line with previous works in valuation studies that have emphasized the messiness and often contested nature of practices of valuations (e.g., Heuts & Mol, 2013; see 3.2.3). Indeed, even though the limited scope of my research did not provide direct access to any valuation situation, the narrative reconstructions of my interviewees already provide an idea of the quantity and variety of ‘evaluative principles’ that can play a role in their attempt to practice ‘good science’. Just to name a few examples: my interviewees spoke about the importance of producing knowledge that is ‘societally relevant’ (even though the very meaning of relevance was not univocal, see 6.3.3), voiced appreciation for the innovativeness of complexity science (6.2.2), discussed the tension between ‘caring for context’ and ‘caring for methods’ (6.3.1.3), expressed interest in expanding expertise beyond one own’s original background, acknowledged the importance of qualitative approaches while still deeming quantitative methods as a key to rigour and exactness (6.3.1.1), and lamented the pressure to publish and to ‘build a career’ in science (6.3.2.1). In light of this multiplicity, it is hardly surprising that the valuation of ‘good science’ emerges from my research as all but trivial, as not guaranteeing success nor control (Heuts and Mol, 2013; 3.2.3), and rather as an open ended engagement best described in terms of care (Heuts and Mol, 2013; 3.2.3).

Crucially, in more than one instance I found my interviewees’ imagination of ‘good science’ to be frustrated – either consciously or surreptitiously – and less consequential in practice than liberal notions of ‘actions’ would grant. On one hand, this discrepancy is in some sense surprising, especially in light of the entanglement, which emerged clearly from my research, between the valuation of ‘good science’ and the individual sense of purpose and identity of scientists (see, for example, 6.2.3; 6.2.4). On the other hand, this discrepancy also puts the spotlight on the magnitude of the influence exerted by overarching ‘regimes of valuation’. Resonating with the results of Fochler et al. (2016; 3.2.3), in fact, my research shows that the multiplicity of ‘evaluative principles’ that my interviewees referred to during interviews is drastically collapsed and narrowed down in their everyday practice. In particular, as I already discussed in Section 7.2, one of the main issues seems to revolve around the hierarchical, as opposed to heterarchical, structure of the overall valuation landscape at the CSH – something which I suggest to interpret as the combined effect of the characteristics of the CSH as an ‘epistemic living space’ and of those of the broader academic ‘valuation regime’ of which it is part. The result of this dialectic process is what I have termed ‘good enough science’: a flattened down version of ‘good science’, shaped by necessity as much as by aspiration, by ideology as much as by material infrastructures, and by personal initiative as much as by collective inertia. And yet, it is still ‘good enough’, since it was framed as overall desirable by all my interlocutors. This seems to be particularly true when ‘good enough science’ is compared with the alternative of working in more traditional academic environments and given the nagging feeling of having to fight to ‘survive’ within academia. Therefore, then, a central finding of my research is that to define the CSH and complexity science, both as epistemic living spaces and as location of the production of 'good (enough) science', the absence-presence of academia is of central importance: absence because the identity of the CSH is clearly constructed in opposition to academia, presence because academic logics and valuation repertoires still exert an extremely strong influence on the research of the hub.

And still, it would be wrong to narrate this story one-directionally. It is not, only, a tale about the encroachment of market logics into academia. And it is not, only, a tale about societal forces and institutions shaping, for better or worse, how research is done. Looking at it historically, going back to the SFI (see 2.4.1), first, and to the early days of the CSH (see 6.1.1), later, I believe another side of the coin to appear quite clearly. This is, in fact, also the story of a field that, from its inception, not only withstood the 'commercialization of science' or the call for societal relevance (see 2.2), but also had an active role in shaping them – and ultimately managed to capitalize on them. This is evident, for example, in the ideological influence that complexity science managed to exert on society through its new ways of communicating science (see footnote 14; 6.1.5) and in the effects of its close ties to finance and private funding institutions (Thrift, 1999; Williams; 2012; Baker; 2022; 2.4.1; 2.4.2.1; 6.1.1; 6.1.3). In STS jargon, this is co-production. In complexity science jargon, this is co-evolution. In both cases, the point is that the relations between different parts of a systems are never unidirectional – that lived realities are relational, entangled, and complex. In the case of this work, this means the CSH cannot be parsed, simply, as a manifestation of the resistance to and the victim of an academic system that does not allow 'good science' to thrive and forces scientists to settle for 'good enough science'. This might be true, but only partial. The CSH, and complexity science more broadly, are also part of that system and comply with and reproduce its logics. In many regards, they even reinforce them – as my work has contributed to show and several scholars before me have argued (e.g., Baker, 2022; Törnberg & Uitermark, 2025; and, more indirectly, Williams, 2012; Li Vigni,2020; 2021b; 2023).

8 Conclusion

"No one would deny that the world is complex, that it escapes simplicities. But what is complexity, and how might it be attended to? How might complexities be handled in knowledge practices, nonreductively, but without at the same time generating ever more complexities until we submerge in chaos?

(Law & Mol, 2002; p. 1)

My research of the CSH and complexity science is admittedly limited, as it is only based on the work for a master's thesis. Indeed, I only managed to talk with a handful of scientists working at the hub, and no one from the broader community of complexity science. Thereby, my results necessarily lack generality – surely concerning the CSH itself, but even more critically concerning complexity science as a whole. While they are representative of a certain way of doing science and 'living' at the CSH, their validity beyond the specific context in which they were derived should be taken with a grain of salt. This case is for sure not helped by the fact that my research approach was purely qualitative, something that does not lend itself easily to large scale analyses and generalizations (see, Jensen & Laurie, 2016). As some of my interview partners observed, this also risks making my results more 'ambiguous' and much less 'testable' (6.3.1.1). Nevertheless, as I have explained at length in Section 5, I do believe this approach to have its value. Even if I had had more resources, indeed, I would probably have only complemented it with more and deeper qualitative engagements with the hub. For example, I would have loved to extend my research by: 1) following complexity scientists in their everyday work (e.g., modelling and scientific writing practices), in group meetings and in workshops in order to reconstruct a more fine-grained understanding of scientific practices and valuations; 2) comparing the CSH to other complexity science research institutes in order to disentangle the specificity of the CSH (and of the Austrian context) from the more general transformations of complexity science in regards to 'societal relevance' and the role of data and quantitative methods; and 3) carrying out a fully-fledged ethnography of the CSH (5.2).

All things considered, though, I believe my work managed to cover quite a bit of ground. It was a long journey and, at least for me, not an uninteresting one. With the contemporary societal and scientific crises as a starting prompt (1), complexity and complexity science came into view (1.2; 1.3; 2.4) and I decided to focus my research on the CSH. In order to better understand the positioning of the hub, I found it necessary to take a detour into the shifting relations between science and society (2.2) and into the scientific role of quantitative methods and data (2.3). After historically and institutionally situating the hub (6.1), and after having gathered my empirical material relating to scientific practices and life at the CSH (6.2; 6.3), in Section 7 I finally managed to put together partial answers to my research questions (4). Now, to conclude, I want to take a step back and reflect on how my empirical work can speak to the broader sensibilities that were the initial motivation of this thesis. In other words: is there anything that can be learned from my research concerning the societal crises and the role that science can or should play in addressing them? And what about complexity and the ways it can or should be handled in knowledge production? Given the extremely broad nature of these questions, I hope the reader will forgive the analytical liberties I will take in the next couple of paragraphs.

To begin with, it seems to me that one of the most important reflections to be made in relation to how science can help address societal issues – a reflection that, for that matter, is also relevant more generally – is about how the 'good' is constructed and valuated. Through what practices and against which constraints do we collectively undertake the fundamental activity of attributing worth? That everyone more or less strives for the 'good', I believe, is not a controversial statement. But how is that 'good' imagined and practiced, contested and negotiated? I believe that, in its small part, my research contributes to make the following broader point: in many cases, we collectively do not provide enough space, nor grant enough importance, to valuations. Rather, we relegate them to the margins and externalize them to standardized and often quantified procedures. The question of what is a 'positive direction for society' and the deliberation over the potential downsides and upsides of different alternatives should instead be at the very heart of any endeavour that proposes itself as positively transformative – be it personal, political, or scientific. In all these cases, the questions to be addressed are deeply political and qualitative and can only be navigated through collective and open-ended engagement. Quantitative knowledge can of course inform such discussions, but it will never be able to settle them. The risk of not devoting enough space to the trouble of discussing what the 'good' is – or of ever illuding oneself to have a definite and neat answer to this question – is to essentialize it, to hollow it out into an empty rhetorical category. More problematically, it is potentially a way of lending ourselves and our work to the dominant logics of power.

Finally, I believe my work to also open up the question of whether complexity science can be imagined and practiced otherwise. Is another science of complexity possible – and desirable – either beyond or within the hegemonic current associated with the SFI and the CSH? I have two answers to this question, and both are positive. First, there actually exist several interesting and quite coherent articulations of what is often denoted as 'critical complexity science' (see "Special Editors' Introduction: What Is Complexity Science?", 2001; Richardson et al., 2001; Woermann & Cilliers, 2012; Cilliers, 1998; 2005; 2016; Juarrero, 2002). In a nutshell, these currents argue that the "study of the characteristics of complex dynamic systems is showing us exactly why limited knowledge is unavoidable" (Cilliers, 2016; p. 97). In other words, while not criticizing the tendency to adopt quantitative approaches for the study of complexity per se, scholars from critical complexity argue that quantitative approaches will never be enough to fully understand complexity – which is 'incompressible' (Cilliers, 2016) and thereby does not admit an accurate representation that is simpler than itself. Paraphrasing Lao Tzu: "the complexity [Tao] that can be named is not the real complexity [Tao]" (Laozi, 1972; see also Strand & Kovacic, 2022). From this perspective, no matter how good our models or data are, or will be in the future, they will always need to be coupled with broader political and ethical discussions and the humbleness that come from a deep acknowledgement of the limitedness of knowledge. Of course, this implies having "more talk and less action" and that we should move "toward a society that collectively chooses hesitant and inefficient behaviour, following passions to say 'stop' even when there seems to be no explicit reasons for it" (Strand, 2002; p.181). This, which so much resonates with the discussion about the valuation of the 'good' I sketched in the previous paragraph, is not an easy proposal to digest – especially for anyone that grew up in the modern global north. It is not a solution nor an answer, but, in some sense, the negation of the existence of either. It is an invitation to sit with the trouble of complexity. Second, and maybe most important, I believe my empirical work to also have provided a positive answer to the question of whether another complexity science is possible. Indeed, within the CSH itself, I did identify alternatives and different imaginations, tensions and practices of resistance. Another complexity science *is* possible, and already exists, potentially. My work, which has foregrounded the lived experiences of scientists working at the CSH – and has thereby given me so much to wonder and to learn – can be framed as an attempt to give space to such multiplicity: to give space to the worlds I "want to help to make" (Law & Urry, 2004; p.391; see 5.1). Flying in the teeth of capitalist domination and in the face of technocratic simplification, these worlds sprout and blossom in the cracks of the very walls that are designed to contain them: in the corridors of the CSH, and everywhere around us. We need to look for them and work to make them sharper.

8.1 Coda: Magic and Complexity

"In the marshes the buckbean has lifted its feathery mist of flower spikes above the bed of trefoil leaves. The fimbriated flowers are a miracle of workmanship and every blossom exhibits an exquisite disorder of ragged petals finer than lace. But one needs a lens to judge of their beauty: it lies hidden from the power of our eyes, and menyanthes must have bloomed and passed a million times before there came any to perceive and salute her loveliness.

The universe is full of magical things patiently waiting for our wits to grow sharper."

(Phillpotts, 1919/2018)

There are at least two ways of interpreting the quote above. According to the most straightforward interpretation, the correct one from a strictly grammatical point of view, magical things are waiting that our wits grow sharper so that they can be found and be appreciated. The other way of interpreting the sentence, for which a slight interpretative stretch is needed, is to say instead that magical things need the help of our wits so that *they* can be become sharper – that is, *more* magical. While the first is rooted in an ontological understanding of magic – things are either magic or not in and of their own –, the second is instead rooted in a relational understanding of magic – things are magic if we see and nurture them as such. I have to thank Mariano Tomatis (2025) for making me aware of the productive duality entailed in this short poem – even if it then took me a while to find out its correct attribution, which is not Russell or Yeats as commonly believed. While I am pretty sure the author only had in mind the first interpretation of the sentence, I believe it is precisely the ambiguity of believing in two distinct possible interpretations that makes this quote so beautiful. I don't think either is better than the other, but both are much less significant considered in isolation. In a similar fashion, I feel that any stance towards complexity that only aims to explain it away is missing the point. The world is full of complex things, indeed, and while we should strive to understand them, we should also actively sustain their magic and multiplicity.

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10 Appendix

10.1 Coding Table

Theme	Occurrences	Code	Description	Occurrences
Research is influenced by the broader context	84	Academia	Individual perception and experience of academia at large: expectations, norms, struggles, characteristics (both positive and negative) of academia	53
		Science communication	Experiences and examples of science communication; more broadly: individual relation with public and non-scientific actors	13
		Funding	Individual experience and perception of funding: sources of funding, influence on research, struggles or positive experiences with funding. More broadly: project management and research strategies in relation to material constraints	35
Defining and Characterizing Complexity Science	311	Topics and questions	Case studies, research topics, and research questions of complexity science	60
		Complex systems and complexity	Definitions and properties of complex systems and complexity	39
		Scope	Objectives, applications, and (potential) impact of complexity science's approaches – including their specific limits and affordances	88
		The 'discipline' of complexity science	Disciplinary status and structure of complexity science, disciplinary composition of complexity science	21

		Jargon	Usage of terminology and concepts of complexity science	48
		Motivation to work on complexity	Interviewees explicitly expressing something that motivated or more generally brought them to work on complex systems/ in complexity science	30
		Approaches and tools	Approaches, methods and tools that are explicitly connected by my interviewees with complexity science	33
		Relation to other disciplines	Relevance of complexity science to other disciplines, differences or similarities between complexity science and other disciplines, connections between complexity science and other disciplines (e.g. intellectual filiation, collaborations)	133
What makes the CSH unique	87	Attraction to CSH	How did my interviewees come to work at the CSH? What do they find unique and motivating about the CSH?	12
		Complexity science à la CSH	Epistemological characteristics of the CSH (e.g., overall background of people working at the hub, research foci, methodologies)	19
		Networking and pedagogy	The role of conferences, workshops, collaborations, and personal networks in shaping the CSH and the trajectories of my interviewees	16
		Complexity community	Connections and exchanges between the CSH and the complexity science community at large (e.g. interviewees having worked in another complexity science research center before the CSH)	19

		SFI	Relation between the CSH and the SFI, both in terms of exchanges and collaborations but also in ideological and symbolic terms	19
		CSH organization and mission	How is the CSH organized and structured? What is its relation to the broader academic and political landscape? What is the overall mission of the CSH?	24
		History of the CSH	How and when was the CSH funded? Who were the actors involved? How has it changed since?	12
Overarching/ descriptors	241	Causal explanations	Importance of causal explanations, interest in understanding causes of phenomena, comparison between causal explanation and correlation	6
		Change	Transformations that are taking place or should take place at a systemic level, specifically in academia or in complexity science	6
		Continuity in research	Temporal coherence in individual academic trajectories, either with respect or in opposition to broader academic norms	10
		Defying expectations	Complexity as a cause of discrepancies between expectations and actual results, either within science or between science and real-world implementations	9
		Depth		5
		Growth		13
		Importance		22
		Independence		12
		Novelty/innovation		50
		Openness		9
		Time		14
		Trade-offs/ choices		59
		Usefulness		32

		Worth		21
Epistemology/ general	154	Ambiguity		13
		Applications		11
		Bias		13
		Connecting things		40
		Context/ secondary dimensions		42
		Cosmology/ontology		51
		Data		82
		Definitions		58
		Description		16
		Estimates and predictions		50
		Explanation		24
		Generality		12
		Good science		188
		Human capabilities		7
		Intentionality/ conscience		10
		Interdisciplinarity/ transdisciplinarity		82
		Jargon/ disciplinary language		7
		Knowledge management		37
		Noise		2
		Objectivity		9
		Order/ chaos		3
		Precautionary/ modesty		22
		Quantification		98
		Questions		27
		Reductionism		10
		Relation with policy/ intervention		68
		Representation		4
		Reproducibility		17
		Simplification/ simplicity		39
		Theory		25
		Uncertainty		15
		Understanding		46
		Working solutions		9
Epistemology/ empirical aspects	119	Evidence/ empirics		32
		Experiments		15
		Falsification		21
		Validation		77
Epistemology/ methodology	310	Accuracy		21
		Assumptions		21
		Causality		19
		Correlation		14
		Interactions		14
		Methodology/ methods		162
		Modelling		89

		Multilayer		6
		Networks		46
		Optimization		5
		Proxies/ operationalization		13
		Scale		27
		Simulations		20
		Statistics/ probability		19
		Translation		8
		Variables		30
Personal Dimension (Rename coherently with epistemic living spaces)	370	Academic background		110
		Beauty		5
		Challenges		129
		Curiosity		13
		Fear		15
		Fun		8
		Indecision		8
		Interests/ motivations		79
		Personal scientific practice		107
		Agency		7
		Discrimination		11
Political dimension	106	Diversity		20
		Ethics		23
		Fairness		7
		Gender		11
		Justice		2
		Power/ authority		28
		Societal Change		32

Table 10 *Overview of the themes, code labels and codes that emerged from the analysis of my interviews (including occurrence count).*

10.2 AI Usage Table

AI Tool	Work phase	Prompts/How was the AI used?	Reason for usage	Comments and reflection on the results
Microsoft Word Spellcheck	Throughout	Automatically checking for spelling, grammar and syntax mistakes	It is standard practice	It was helpful to spot mistakes
Google Search AI Tool	Throughout	Sometimes, when looking on Google for language	It is in-built in Google search	It was sometimes helpful to have a direct answer

		clarification or references, I used the built-in AI tool instead of looking through the results	since a few months	without having to look through the pages provided as results by the search
Deepl	Finalizing	I used Deepl to have a first draft of the German abstract of the thesis, which was then reviewed by a native German speaker	My German level is not good enough to translate academic writing	The translation was pretty accurate and only a few minor corrections were needed

Table 11 *This table was put together by the STS TAs based on the "Documentation Table" developed by the Writing Center of UniGraz (available here).*