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Ontological Commitments and the Role of the Experiment in the Sheldrake vs. Rose Controversy

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Abbreviation	**Written out**
ANT	Actor-Network-Theory
EP	Evolutionary psychology
LED	Light-emitting diode
pnna	Page number not available
SSK	sociology of scientific knowledge
STS	Science and Technology Studies
UK	United Kingdom

1 INTRODUCTION

Book burning is an act that was used historically by different (often quite radical) groups to suppress knowledge and censor thought. It has a long and negative history. A history that means that a call for book burning would not be ushered lightly. Nevertheless, there was a hypothesis, a theory, that was so controversial and outrageous, that an editor of Nature questioned whether the book containing that hypothesis shouldn't be burned. In an editorial in 1981 the editor wrote concerning this hypothesis:

> And even bad books should not be burned; works such as *Mein Kampf* have become historical documents for those concerned with the pathology of politics. But what is to be made of Dr Rupert Sheldrake's book *A New Science of Life*, published in the summer (Blond and Briggs, London, 1981)? This infuriating tract has been widely hailed by the newspapers and popular science magazines as the "answer" to materialistic science, and is now well on the way to being a point of reference for the motley crew of creationists, anti-reductionists, neo-Lamarckians and the rest. The author, by training a biochemist and by demonstration a knowledgeable man, is, however, misguided. His book is the best candidate for burning there has been for many years. (ANON, 1981, p. 245).

The editor who wrote these lines in the article "A book for burning?" answers the question he posed in the last lines: "[Dr Rupert Sheldrake's] book should not be burned (nor even confined to closed shelves in libraries) but, rather, put firmly in its place among the literature of intellectual aberrations." (ANON, 1981, p. 246). While the editor is not openly calling to the action of book burning, the comparison still is quite dominantly in view.

What is this hypothesis that created such an emotional uproar and who is its nefarious author? The hypothesis concerns 'formative causation', a concept regarding memory transfer in the field of biology and the author is Rupert Sheldrake. His ideas and theories were, as can be seen above, heavily attacked and criticized (ANON, 1981; Sheldrake, 2019, "Rupert Sheldrake"). He nevertheless continued publishing after the statement above was written in 1981 and further formulated his hypothesis. The debate around his books went on until 1992 when Steven Rose publicly announced that he would like to test Sheldrake's hypothesis in Rose's lab. Steven Rose is a neurobiologist by training and a prominent figure in the UK research landscape in neurobiology. Sheldrake and Rose agreed to plan an experiment jointly and made a contract to publish the results together. But as it turned out, the experiment delivered data that was inconclusive to both, and they disagreed over the interpretation. This

led to a broken contract and three articles published by Sheldrake and Rose (two by Sheldrake and one by Rose), publicly explaining the experiment, the data, the interpretation and why the interpretation of the other was misguided.

This controversy is not only a telling example of what has come to be known as 'Controversy Studies' (Pinch & Leuenberger, 2016; Jasanoff, 2019) in Science and Technology Studies (STS) but also encompasses some interesting peculiarities. First, it involves an experiment planned and executed by opposing sides of a controversy. While often in controversies the experiments of the opposing side are subject to questions of credibility, this point seemingly falls flat in an experiment made by both parties. Second, both researchers debated the hypothesis at stake drawing on literature from the field of STS and the history and philosophy of science. This is as well unusual for a controversy, to pick up the literature that studies a field socially to incorporate it in discussions about the field. And third, the way this controversy unfolded, from pop-science books to newspaper articles, to articles in journals to go back again to pop-science books. The involvement of the public is an important part of this controversy and it unfolded is peculiar in its circularity.

Apart from these peculiarities, the case which this thesis studies is a theoretically interesting site where instances of knowledge production, knowledge contestation and knowledge communication intersect. To properly address these issues this thesis will take a closer look at the field of 'controversy studies' and the research that has already been done in it. The literature concerned with knowledge production, from Laboratory Studies to historical descriptions will be part of the grounding literature as well. Special attention will be paid to the factors involved in the closure of scientific controversies and the role of the experiment in them. The research question that will guide this project is: **How does the controversy between Sheldrake and Rose about the formative causation experiment unfold?** To answer it, three concepts will be used. The first will be 'boundary work', to analyze the boundaries both sides want to draw in this controversy. The second is the 'experimenter's regress', dealing with the human factors involved in the use of experiments as deciding criteria over theoretical assumptions. The third is the concept of 'theater of proof', to analyze how different instances of the experiment and its coverage are created to sway an audience. The method used to accompany it will be a document analysis with a focus on the produced documents from both actors but also including additional data from a small bibliometric analysis. The empirical analysis will use the concept of situational analysis to answer the research questions.

2 BACKGROUND

To understand this controversy, its actors and the uproar it caused one has to take a deeper look into the background of it. It started in 1981 with the publication of one of Sheldrake's books. Rupert Sheldrake is a biologist by training. He studied natural sciences at Cambridge University, philosophy and history of science at Harvard University and took a PhD in biochemistry also at Cambridge University. He also held the position of Director of Studies in biochemistry and cell biology at Cambridge University. He spent a few years researching and working in Malaysia and India, where he wrote “A New Science of Life”, the book that caused such an uproar (Sheldrake, 2019, “Rupert Sheldrake”).

“A New Science of Life”, written in 1981, introduces what Sheldrake calls ‘morphic resonance’: “the past forms and behaviours of organisms influence organisms in the present through direct connections across time and space“ (Sheldrake, 2019, “A New Science of Life/ Morphic Resonance”, pnna[1]). This happens through morphic fields, where information is stored, and which can be accessed by all ‘self-organizing systems’. These systems include humans, animals, plants, and even crystals. His hypothesis proposes solutions for the problem of how organisms get their physical form, which is still unanswered in biology. He describes his book as “[c]alling into question many of our fundamental concepts about life and consciousness, this reinterprets the regularities of nature as being more like habits than laws” (Sheldrake, 2019, “A New Science of Life/ Morphic Resonance”, pnna). He describes these fields as something outside of the known four forces in the natural sciences (gravitation, electromagnetic, weak and strong interaction), thereby questioning the order of physical laws. He argues that when accepting this fifth force and morphic resonance, phenomena like telepathy and collective memory might be scientifically possible (Sheldrake, 2019, “Rupert Sheldrake”).

Rose has a degree in biochemistry and a PhD connected to neurobiology and is an emeritus professor of Biology and Neurobiology at Open University and Gresham College in London. His research revolved around the neurobiological aspects of memory formation and possible treatments for Alzheimer’s disease. Most of his research was done with chicks and studying the molecular changes in the nerve cell patterns during the learning process. He concerned himself with the broader aspects of the field of neurobiology and communicating science to the public. He has won several awards for his work influencing the founding of the field of neuroscience. Together with his wife, he worked on the study of the “ethical, legal and social

[1] Pnna: Page number not available. Some of the documents cited are unnumbered webpages. Direct citations using the page numbers are not possible, since formatting and browser variations can cause text to move. Whenever direct quotations are used from webpages, pnna will signify that the specific location cannot be more closely detailed.

implications of developments in science, especially in the fields of genetics and neuroscience" (Rose, 2019, "Biography", pnna). From this work stems also a book on the topic of the influence of society on research. He was the founder of a movement that criticized heavily sociobiology and the notion that social behaviour could be explained by genes (Rose, 2019, "Biography").

The experiment planned by Rose and Sheldrake was an attempt to experimentally disprove or prove Sheldrake's hypothesis of 'formative causation', which includes the concept of 'morphic resonance'. The experiment was planned and carried out in 1992. It took place in Rose's laboratory and was primarily done by one of Rose's employees. The experiment revolved around chicks and their tendency to avoid specific stimuli. There were two patches of young chicks, one of them was trained to avoid a stimulus (a LED light) through the injection of specific chemicals (Lithium-Chloride) which made them sick. They associated the sickness with the LED light and started avoiding it. Sheldrake's hypothesis stated that the other patch of chicks, even though they were not directly in contact with the first batch of chicks and not trained to avoid the stimulus, would show a tendency to avoid the same stimulus. Rose argued against it. Both noted down their predictions about the experiment and agreed to publish the results, no matter the outcome. As already stated, the outcome was not conclusive to either of the predictions and the results were published separately.

3 STATE OF THE ART

Before the case is presented in its entirety and we delve into its dramaturgy, I will present the knowledge this thesis draws upon and the knowledge it embeds itself in.

The first body of knowledge that I will discuss revolves around *knowledge production*. It will deal with how knowledge is produced, by whom and who is excluded in its production. Between the first and the second body, there will be a bridge topic concerned with the importance of *experimentation.* This includes the role of the experiment in knowledge production, its metaphorical and rhetorical significance, as well as its role in the contestation of produced knowledge. This will lead right into the second body of knowledge which is concerned with the *contestation of knowledge.* This body takes a closer look into the big field of controversy studies, and the subfields that will be relevant for this thesis. The third and last body of knowledge I discuss revolves around the *dissemination of knowledge.* This will include science communication, its forms and modes and different manifestations.

3.1 DYNAMICS OF KNOWLEDGE PRODUCTION

The subject of the production of knowledge has a longstanding tradition in the field of Science and Technology Studies (STS). This subject spans many years and instances of published work, and to draw an all would become too voluminous for this thesis. I will look at some of the works that will be most important for the case I will present later on, while also including works that are too important to be left out. The subject of knowledge production is always intimately tied to the social aspects of the group that produces it. This was also the starting point for early studies concerning this topic and the fundamentals of the field which came to be known as STS. Fleck and Kuhn are names that come to mind when thinking about communities and their practices regarding knowledge.

Fleck (1975 [1935]) worked on what he called ‘thought collectives’, groups of people who share the same ‘thought style’. He stated that ‘extra-scientific’ ideas and contexts are important in the shaping of new scientific knowledge. He argued that new knowledge can only be generated in a ‘thought collective’, at a specific point in history. This makes the genesis of knowledge an inherently social process. A ‘thought style’ always limits and enables new knowledge. It is built on already accepted ways of enacting with other members of the collective and the world. Each ‘thought collective’ has a specific set of truth, which may be limited to only one collective. A person can be a member of more than one collective, and through this may bring new ideas into one collective, by borrowing them from another. This, in turn, changes then the prevalent ‘thought style’ of the collective.

Kuhn's (1962) work takes a similar stance in analyzing the social factors involved in science. He theorized about what he labelled 'paradigms', connected to specific scientific communities. A paradigm guides the perception of individuals through institutionalized patterns of assumptions and practices. It is the sum of accepted theories, techniques and questions to produce knowledge. To create new knowledge, the rules of the paradigm must be considered, because it is the baseline that makes it possible for scientists to communicate. Every new hypothesis is compared to the knowledge generated in a paradigm and discarded if it does not fit in it. If observations cannot be described by the existing paradigm, they are either ignored, categorized as exceptions or the paradigm falls apart. New paradigms are in Kuhn's view always connected to one specific person, who starts the social process of a paradigm shift. The new paradigm makes more explanations of observations possible. Different paradigms are 'incommensurable', which means that there is no communication between paradigms possible since they involve different languages, methods and perceptions when talking about an observation. Kuhn will also get a specific significance in this project, as one of the discourses brought by the actors into play.

Focusing more on the practical aspects of knowledge production, Latour (1987) has written about science-in-the-making and ready-made science. Latour compares science with a double-faced Janus, who others two different advice for science while in the making and science when it is made. While science is being made, uncertainty has to be combated at every step. These uncertainties and struggles while making science are then moving to the background when science has been made. When science has been made, the facts are presented as straight, as holding up without a doubt, and everything that contributed to the emergence of facts is black-boxed.

Together with Woolgar (Latour & Woolgar, 1979) Latour picked up this notion again and has shown how in everyday laboratory practices knowledge is inscribed. In the course of this inscription (turning material matter into written text) all the necessary steps to reach it are pushed into the background. The written document (in the laboratory often diagrams and such) is then the focus of discussion and the process of achieving it are seen as mundane and unimportant. This went hand in hand with the stripping of modalities of the produced knowledge. More and more modalities (such as the machines involved or the people) are left behind to achieve a fact, something that is widely known.

In the same vein, Knorr-Cetina (1983) has shown that knowledge production in the lab is heavily dependent on the material surroundings. The scientific products produced in the lab are occasioned, meaning: "the circumstances of production are an integral part of the products which emerge." (Knorr-Cetina, 1983, p. 124). These circumstances can include but are not

limited to, the availability of certain machines, personnel, substances, books, etc. The often-held notion that science is done with first a goal and then the choice of materials, does not hold up under Knorr-Cetina's research. Researchers take what is available and modify their goals accordingly, creating a back and forth of adjustment, until the experiment 'works'. These adjustments as well as the influences on the decisions made in the planning phase are then most of the time omitted when the finished products are presented.

Taking a historical example, Latour shows how knowledge is made and acted out in social structures with his analysis of Pasteur's work. In his book "The Pasteurization of France" (Latour, 1988) Latour follows the movements of Pasteur through the scientific community in France and through this traces the involvement of different groups in the establishment of scientific facts. As with ANT (Actor-Network-Theory), Latour here builds on a foundation of 'agnosticism' towards the actors involved, so as they will not be reduced to broad terms such as "science" or "society". This also means to forego any pre-made conclusions about hierarchies of actors, of the existence of specific social groups and boundaries. Of great importance in this analysis is the historical context of the knowledge produced by Pasteur.

Around the 1850s in France, there was a perceived crisis of unhealthy young men. Men were needed for wars and industry and a diverse range of sicknesses and ailments hindered this 'human capita' to be used. This crisis led to the rise of the hygienist movement. The hygienists thought that illness could be caused by anything. There were no defined causes for illness at that stage and therefore a targeted approach to illness was not possible. This meant that since everything could be the cause of illness, and the hygienists wanted to act on it, nothing could be ignored and everywhere had to be acted upon. This included treatment of water, alcoholism in specific groups of people, work conditions and housing of the working class amongst many more. Nevertheless, despite enforcing many precautions, illness always returned. It was difficult to convince either the public or the authorities involved for the programs of the hygienists since illness could always spontaneously spring up again of the unknown. To convince the social movement of the hygienists, a doctrine needed to explain the problems they had as well as give possible points of action to hinder the further spread of illness.

The main contribution of Pasteur was "redefining the social link as being made up everywhere of microbes"(p.39). His insights into the way microbes cause sickness was the missing link the hygienists needed. It connected the seemingly random circumstances to the illness. Through this, it was possible to have a focal point of attention, to grapple with the microbes and therefore with sickness itself. The hygienists were now able to still be all at once and to spread their movement, while also getting solutions to their problems.

Another social group that benefitted greatly from microbes were the surgeons. Through the disinfecting of their tools, fewer patients died on their tables and they were enabled to move further in their practice and their patients' bodies than before. Since the microbe gave them more power in their practice, they also readily accepted its existence and aligned themselves with Pasteur and his knowledge. Other social groups were more sceptical and did not so easily accept Pasteur's research. The physicians did not gain much by the prospect of accepting the new knowledge but rather had a lot to lose. They were unwilling to give over what they thought of as an essential patient relationship to the doctrine of the Pasteurians. If they did, they would have lost something that was to them one of the main points of their trade. They did not critique Pasteur openly but were rather slow and sceptical to use his knowledge. Once an arrangement had been made that benefitted the physicians they were quick to accept the knowledge and to spread it.

The work of Pasteur and its insights gave the hygienists the tools to achieve what they wanted. By incorporating Pasteur's knowledge the hygienist movement gave Pasteur the credibility and the importance often attributed solely to him. Pasteur became more than one scientist adding his knowledge to the pool of others, he became a figurehead and spokesperson of the hygienist movement. Pasteurians and hygienists strengthened each other. Through this the actors that were before 'standing in the way' of the hygienists, the unwilling masses and authorities, now were swayed by the combined credibility of both movements. This meant that the hygienists had to trust Pasteur completely and also make a show of it so that each of their credibilities would not be questioned.

Whether a theory is rejected or accepted depends on what it offers or endangers to the social groups who encounter it. This means that for different social actors and groups the theory in question might be completely differently understood and acted upon. Once knowledge is taken up and used, and most often used to further the goal of the user, it becomes more and more routine and less and less controversial.

3.2 The Role of the Crucial Experiment

In the scientific process of the natural sciences, experiments have long played an important role in the production of knowledge. The history of experimentation goes back a long way. Shapin (1999) has followed this way back to the seventeenth century in England. In his historical account, Shapin points to the importance of the social setting of the experiment. In England at that time, experiments were most of the time only seen as legitimate means of knowledge production if they were witnessed by the already existing class of gentlemen. It was believed that the individual might make mistakes, the collective would not. The gentlemen

witnessing fulfilled the roles of active participants. They did not merely look at the experiments that were performed especially for them, but they also discussed what they had seen. These gentlemen were seen as possessing such moral integrity that they constituted the reliability of experimental knowledge witnessed.

Another historical account of the role of experimentation in knowledge production is again Latour's (1988) description of Pasteur's work. I have already discussed the importance of Pasteur's knowledge production, but the role of experimentation was also key in stabilizing the knowledge produced.

The place where nearly all of the knowledge of the Pasteurians was produced was the laboratory. They proclaimed it as the vital place to study, understand and act on microbes. Pasteur and his followers often switched disciplines, but what remained is that they tried to solve the problems of every discipline through the tool of the laboratory. Since they were well versed in the use of the laboratory, this re-definition of problems gave them a good chance of giving the solution. In the laboratories, the phenomena became controllable and dominatable.

> For convenience, I divide this movement, which I call the "spring" of the Pasteurians, into three stages: in the first stage move the laboratory to the place where the phenomena to be retranslated are found; in the second stage move the phenomena thus transformed into a safe place, that is, where certainty is increased because they are dominated; and in the third stage transform the initial conditions in such a way that the work carried out during the second stage will be applicable there. (Latour, 1988, p.75)

Even outside the laboratory, the cultural authority of the Pasteurians held. The mechanism at work was what Latour called the 'theater of proof'. Pasteur's experiments were not limited to the laboratory but were also carried out in the field. These experiments, injecting microbes into cows with the intent to make them sick or immune, were invented to convince. They were designed for showing, visually, to others what Pasteur had achieved in his laboratory. In this theater of proof, others have to be interested in and invested in the phenomenon at stake and then shown clearly visible results. Latour writes about Pasteur's experiments: "Without this double movement of interest and dramatization, the tests might have been indisputable but would either have remained undiscussed because they interested nobody or […] have been discussed but remained disputable." (Latour, 1988, p.87). But a theater of proof is not something that can be created out of thin air. Phenomena and the techniques that deal with them need an apparatus that makes it possible for them to exist. In Pasteur's case, this was

the laboratory with its techniques, knowledge and practices. Through time though, all these apparati were becoming more translucent, until the knowledge produced seemed universal.

Collins and Pinch (1998) have shown with an example of the detection of gravitational waves the role experiments play in the production of knowledge. The followed the physicist Weber's claims in 1969 of having detected gravitational waves. Weber's claim was so unexpected that it needed much more than the results and description of his experiment to persuade others of the correctness of his experiment. The first step in convincing the scientific community of the competence of his experiments was to get them to try and disprove it. The wording of his elaboration was crucial in convincing others to try to reproduce it. When laboratories began to design experiments to try to achieve the same results as Weber they were posed with a difficult problem. Since it was unclear at the time if gravitational waves do exist, the outcome of any given experiment concerned with the detection of gravitational waves could not be known beforehand. Following this, it was as well not possible to know what constitutes a good experimental setup. Since most technical artefacts in an experiment are judged by whether they can show what they are supposed to show (in this case detect gravitational waves) and that is not known, it is impossible to know if the material used is good enough for what the researchers want to achieve. This circular movement is called 'experimenter's regress' and was introduced by Collins in 1985 (Collins, 1985). This circle can only be broken when there can be found a criterion that is independent of the output of the experiment. This criterion is almost always a social one. When this criterion is found, it becomes decisive also about the existence of the phenomenon at hand. When one knows what a good detector is, one knows if gravity waves exist. And a good detector is defined by whether the scientific community believes that the experiments were done properly. In the case of Weber, the scientific community reached after years of trying and failed attempts to reproduce the findings the conclusion that Weber's experiments were not done properly. The notion of the experimenter's regress will become important later on as well in the case under scrutiny and will therefore be explained in more detail in the theory section.

Experiments have time and time again been claimed by those who perform them to be a 'crucial experiment'. This implies that one experiment can be so powerfully designed, that it will decide over competing theories. While a range of scholars in STS have already criticized this notion, that it leaves out a plethora of additional factors in the closure of controversies, the notion of a crucial experiment is still strong. Christie (2000) analyzed an experiment that was hailed as a 'crucial experiment' concerning the ozone layer. She stated that experiments might be presented as crucial, to add further weight to their results. This might happen in retrospect

or even before the experiment is done. It is a rhetorical strategy to give more credibility to the experiment done and the results achieved.

What unites all these strands of literature is the fact that experimentation is never without a social context. The social context is crucial for the experiment, whether historically or even today. The peer-review system might have superseded the role of gentlemen in the production of knowledge, but experiments are only seen as properly done if they are documented and given to the wider scientific community for scrutiny.

3.3 THE CONTESTATION OF KNOWLEDGE

Knowledge production does not always go smoothly. Not all knowledge is produced uncontested. And not all produced knowledge remains uncontested. Time and time again, following different reasons, actors call into question the knowledge produced. This might happen at different stages during or after the production. This contestation happens most often in controversy.

Building on these early works on social factors involved in controversies, other work in this direction has flourished. At the EASTS Conference in 2016, Trevor Pinch gave a conclusive overview of the work that has been already done in the field of STS on controversies (Pinch & Leuenberger, 2016). He identified four different approaches on how to study and analyze controversies. The first is the school of research connected to Merton, which focuses on priority disputes (who was the first to pose a specific theory). The second is the study of citizen protests concerning the negative effects of science and technology. The third revolved around the sociology of scientific knowledge (SSK) and deals with research frontiers and experimental and theoretical disputes. The fourth is what Pinch defined as the modern STS approach which sees controversies as integral to the scientific and technological practice and dissemination.

The range of literature falling topically into these approaches is broad, starting with works from early scholars on shifts of prevailing knowledge and the conflicts revolving around them which has been described by Fleck (1975 [1935]) and Kuhn (1962), already detailed above. Following a more recent tradition of analyzing controversies and using empirical methods for contemporary cases, the “Bath School” and with it, Collins as well as Pinch (including, but not limited to: Collins, 1985; Collins & Pinch, 1979; Collins & Pinch, 1998) were more interested in the boundaries that were crossed in controversies and how controversies were closed. This is situated in the third approach above (SSK). In their work, they studied different disciplines of scientific conduct, and others more on the fringes of science. Collins and Pinch took a closer look into the human factors involved in controversies, and how factors such as credibility or

competence influence the outcome of controversies. The role the experiment takes in the view of researchers has been the focus of much of Collins work, which led to the concept of the experimenter's regress, which will be part of the sensitizing concepts later.

When controversies are seemingly decided and the uproar quietens, there still might be researchers left fighting. Simon (1999) argues that it is impossible to pinpoint the closure of controversies in some cases because scientists are still researching the topics. Even though the majority of scientists, and seemingly the scientific community, has long accepted some theories as disproven, researchers struggle on to revive the theories in doubt. Some controversies even appear to be unsolvable. Following the proclamation of the theory of relativity in the 1920s, an international network of opponents emerged that heavily criticized it. Wazeck (2013) writes about this controversy that Einstein's opponents felt that his theories threatened their view of what constitutes science and their social standing. Wazeck follows in her article the opposition that sprang up around Einstein's theory of relativity in the 1920s. She argues that while most controversies can be classified as *in* (controversies about specific knowledge claims, involving mostly scientists as actors) and *about* science (a focus on moral, political and economical concerns and involve a diverse range of actors), the controversy around the theory of relativity is neither. Wazeck classifies it as a controversy "driven by ontological commitments and deep disagreements about the nature of science." (ibid., p. 165) When following the actors of this controversy she paid attention to the marginalization processes that happened. Marginalization is composed of two dimensions: epistemical and social. Epistemical marginalization revolves around the rejection of specific knowledge claims. Social marginalization revolves around the communal aspects of science and how deviant knowledge can be clocked from circulation. Marginalization processes can also be characterized by four stages:

1) The first stage is the deviation of the scientific consensus by either leaving it behind actively or passively not following new developments.
2) The second stage is the argumentation that springs from the conflict between the different bodies of knowledge (the consensus and the deviation) and the perceived rejection of those following the deviated body of knowledge.
3) The third stage is the development of a defensive attitude by the marginalized party, which perceives the rejection as unjustified and a threat.
4) The fourth stage is the strategic opposition that transgresses the scientific realm and seeks different audiences for support.

Not every marginalization follows these steps and not in that order, but it can be a helpful tool to analyze marginalization processes. These processes are “inherent to science because science represents a nonstatic, developing system of knowledge” (ibid., p. 168)

Wazeck shows in her article that most of the critique regarding Einstein was centred around ontological commitments that were threatened by it. Concepts such as the ether were integral parts of the way Einstein’s opponents viewed the world through science. They were not willing to give up these commitments in order to accept the theory of relativity.

In the course of the controversy, Einstein’s opponents saw themselves ever more victimized. This served them in the way that they did not need to accept responsibility for their situation and had the right to counter-actions. The opponents were comprised of people ranging from different backgrounds and levels of education, but were united first by their perceived victimization and second by their belief that the theory of relativity was undermining how science should look and be done.

Wazeck closes her article with the insight that the controversy about relativity was not solvable. The reasons for that lay in the gap between the two parties:

> The case presented here provides evidence that the willingness to communicate and learn and the capability to discuss the theoretical, metaphysical, and ontological foundations of knowledge claims can be sincerely restricted precisely because of ontological commitments; thus, there is neither a common ground for debate nor the interest to create one. (ibid., p.185)

Nevertheless, controversies are not only bound to the scientific community. Citizen protest against the use of specific technologies is one part, but in the last years, the role of “nonexpert knowledge” (Lengwiler, 2008) and its participation in scientific controversies has also come to attention. Lengwiler analyses four different stages of participation in a historical context. The latest stage, starting in the 1960s, is characterized by the growing participation of groups ‘outside’ of science. With roots in social movements that criticized technology and its uses, the participation of nonexpert groups is now seen more and more as valuable. The influence of this participation can not only be seen in the context of policy making but also influences the closure of scientific controversies.

Knowledge is not only produced in institutionalized scientific settings. More and more research was and is being done with ever more importance given to the role of lay knowledge in the production of knowledge. These two types of knowledge production, the institutionalized scientific one and the lay one, often do not go together without friction. Wynne (1996) has

shown with the case of British sheep farmers affected by the aftermath of Chernobyl, how cultural factors hindered the relationship between lay and expert knowledge. The expert advice carried within it certain beliefs regarding itself, such as the power to control and predict, and the non-need to incorporate other forms of knowledge. The lay knowledge of farmers had its own cultural style, which contained individuality and flexibility, and was critical concerning the universal claims of the experts. These differences in the cultural stances to knowledge production of both sides made it impossible to reach a common ground in dealing with the problem.

Taking this notion further, Jasanoff (2011) used the term civic epistemologies:

> […] I suggest that modern technoscientific cultures have developed tacit knowledge-ways through which they assess the rationality and robustness of claims that seek to order their lives; demonstrations or arguments that fail to meet these tests may be dismissed as illegitimate or irrational. These collective knowledge-ways constitute a culture's civic epistemology; they are distinctive, systematic, often institutionalized, and articulated through practice rather than formal rules. (Jasanoff, 2011, p. 255)

These civic epistemologies look different in different nations, but also in different smaller collectives. Non-experts can become involved in controversies if how authorities or other actors want to order their lives goes against their implicit rules of knowing. Involvement does not always guarantee being heard. As Suryanarayanan and Kleinman (2012) have shown with their analysis of the controversy concerning the colony collapse disorder, lay people can achieve expertise in specific topics to rival formal experts, but that does not mean that their opinions are valued. Here again, as with other examples already mentioned, the knowledge produced by beekeepers is not seen as fitting with the epistemology of the institutions and industries which were actively trying to impose a specific kind of order.

A critical moment in all controversies is the closure of the debate. While Collins focused on credibility, Shapin and Schaffer's book (Shapin & Schaffer, 1989) on the historic conflict between Hobbes and Boyle and the political climate surrounding it, brought another novel dimension to the field of controversy studies. In their analysis, the role of the experiment has also been given increased attention. Hobbes and Boyle had differing views on how experiments should be treated. Boyle wanted to treat experiments as objective accounts of nature, outside of the realm of human and social influences. Hobbes argued against it, wanting to transfer authority over experiments to a central representative. Latour (1993) has picked this historical controversy up again and shown that Hobbes and Boyle agreed on nearly everything, except on the way to deal with experiments. Latour argues that through this controversy about

the role of experiments and how they should be handled, one can see that both Hobbes and Boyle had contrasting views about the importance of science and politics. Each had a theory about science and politics, and the controversy could only end if each gained one of these realms to be 'right' in. For Latour, this led to the divide between nature and society that has long been the tradition in the minds of researchers. Also picking up this historical conflict, Jasanoff (2004) argues that at stake was not only what counted as reliable knowledge, but also who got a say in it. Jasanoff connects this to co-production and points out that both actors in this controversy had to engage with problems of already existing systems of social order and social authority. Often in controversies "it may not be possible to address questions of the facticity and credibility of knowledge claims without, in effect, redrafting the rules of social order pertaining to the trustworthiness and authority of individuals and institutions." (Jasanoff, 2004, p. 29). These new orderings of social authority have "to be sustained through elaborate and carefully designed social practices (Jasanoff, 2004, p. 30).

This then brings us to the concept of boundary work formulated by Gieryn (1995) and built upon by others. Boundary work has evolved as a critique of different philosophies of science that tried to pinpoint what constitutes the special cultural space given to science. The concept of boundary work argues against any fundamental characteristics of science but rather that all that makes science science is decided when it is demarcated from other ways of knowing and other social activities. Only through drawing boundaries, what counts as science is being established. The definition of science is often seen through its practices. Gieryn (1995) took up the historical episodes of Hobbes vs. Boyle as well. Gieryn here uses the metaphor of drawing cultural maps. A cultural map is a way of ordering and making sense of science and other social worlds. These maps include attributes connected to science and attributes connected to all other areas that might be on the map. Gieryn shows that Hobbes and Boyle both drew cultural maps. These maps included spaces which through their claims and practices were authoritative and authentic, and which included themselves but excluded the other. At stake was which way of producing knowledge would be seen as authoritative, and where on these maps both actors lay. While there was some overlap of the maps in specific points (religion having nothing to do with science), they were too different to be able to merge. It was a controversy over which map would be accepted by those in power. In the end, Boyle could make his map stick because it was more closely tied with the interests of Restoration England.

As Bijker and Pinch (1987) have shown in their study concerning the manifestation of technical artefacts, there are many ways to close scientific/technical debates. One option is a rhetorical closure, where problems that were the focus of contestation are said to be solved, whether

that is true or not, and the relevant social groups are convinced by that claim. Another option is the redefinition of the problem, where the problem at hand is shifted into the background because it was transformed into a problem of another kind.

Another possibility of closing controversies has already been discussed above, which is concerned with credibility. As Collins (1985) has shown, the credibility of the actors involved in the controversy plays a big role in the closure of it. The more credible the actor, the more credible their actions and therefore their opinions.

Credibility comes not only to play between researchers as single points in a controversy but also in the fate of whole disciplines. This is visible in the example of the controversy and discussion about the ideas of the geneticist Lysenko. This debate was held in different parts of the world. Iida (2015) analyzed the historical shift in the controversy in Japan. Starting in the years after the second world war Iida describes a scientific community that expressed general sympathy towards Lysenko's ideas. Lysenko argued against Mendelian genetics, which was the scientific western consensus at the time, and proposed theories of inheritance not connected to genes. Lysenko's ideas were in the Soviet Union also tied very closely to the political ideas of communism. During the Cold war years, Japan's discipline of genetics tried to reestablish itself globally. An important step in this process was the gaining of authority, which was seen as contradictory to being sympathetic towards such critically discussed ideas as those of Lysenko. Lysenko's ideas were deemed unfitting to be held when competing globally.

Controversies take many forms, and closure is not always easily reached if it is even possible. While the mentioned research deals with a lot of possible ways a controversy might close, there is a possibility that does not get as much attention. Following the research above, controversies might even be closed through the rhetorical presentation and demonstration of experiments and the researchers that do them. It is a possibility to enrol allies for the controversy through public presentation of oneself and one's research. The communication of science and its influences on the researchers and their work is the topic of the next chapter.

3.4 The Communication of Knowledge

Lewenstein (1995) gives in his article an overview of the canonical model of science communication at the time and shows with a case study concerning Cold Fusion how this model is lacking. The canonical model (also called diffusionist) is one of source to receiver, uni-directional and linear, without any feedback. The sender in this model is science, the receiver public, with media being the messenger. This view sees the scientific enterprise as

too complex to be understood by the general public, and therefore a mediation is needed to make it more accessible. This mediation is done by science journalists, which translate the finished scientific facts to the public. This model is by most seen as outdated, but still in use in some places today. The problem that this diffusionist model sees with science communication is how ‘distortion’ could be avoided while delivering the information. This was often seen as the media distorting the information from science on its way to the public.

Opposed to this model is the view that scientific information is shaped by the sender regarding whom it addresses. Every audience and every context transform the information science gives. This view makes no distinction between ‘pure’ scientific information and ‘popularization’. It also sees communication as a bilateral approach. This view is also held by Lewenstein who nevertheless argues for a further step. Arguing with a case study from Cold Fusion, Lewenstein shows that the researchers (mainly physicists) who were involved with the topic of Cold Fusion took up vast amounts of information from different sources and judged each information on contingent factors. The researchers were grappling in reaching an understanding of the topic with which they worked and based theories and experiments on (or did not, regarding their belief in the phenomenon). The presence of multiple forms of media produced an information instability. Many sources were documenting, reporting and arguing about Cold Fusion, all embedded in different media. The researchers who wanted to get a hold of specific information had to piece together several ‘incomplete’ sources to answer the questions they held. The more, sometimes contradictory, information was taken up, the less stable the scientific judgements that were made. Each of the media present influenced each other, sometimes taking up information, sometimes distributing it, sometimes transforming it. Mass media played a key role in the way that “[t]he mix of all communication media depended on the degree to which *mass* media were involved.” (p.427). This is represented in Figure 1 below. Once the mass media left the stage, more and more stable judgements were made since the surplus of information went down. Lewenstein argues that science communication is convoluted and irregular and only once the complexity is reduced, the more stable judgments can arise.

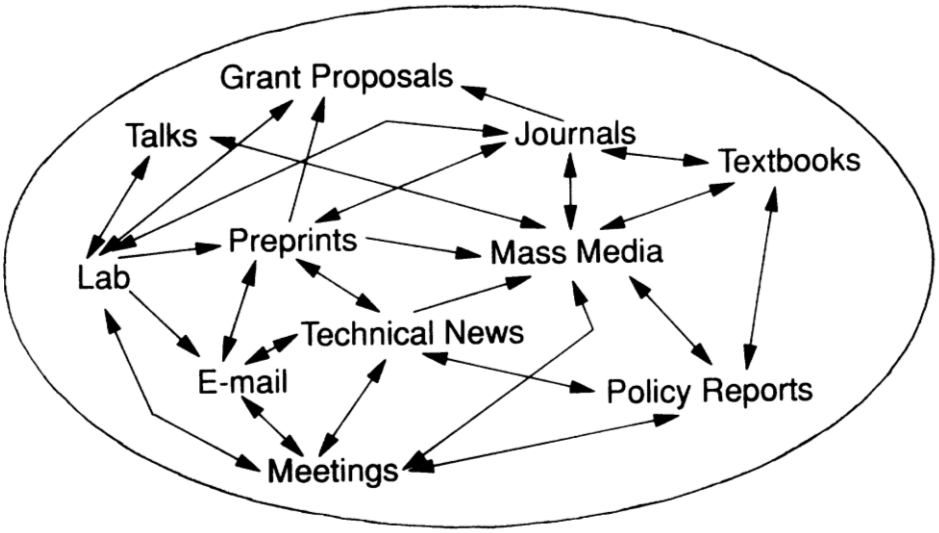

Figure 1: A graphical representation of different sources of information for the researchers involved with Cold Fusion. This exemplifies the complexity of science communication. Taken from Lewenstein, 1995, p.426.

Bucchi (1996) describes in his article the different routes that can be taken by scientists in communicating as well as the models that are a good fit for describing their actions. This approach is different from the one presented in Lewenstein (1995) and is called the continuity model of science communication. This model emphasises that no clear boundaries can be drawn between scientific discourse and popular discourse, even though these boundaries are sometimes drawn by scientists to preserve authority over knowledge. The model defines four main stages of scientific communication: Intraspecialistic (in the speciality, e.g. scientific article in a journal of a speciality), Interspecialistic (between specialities, e.g. articles in Nature or Science), Pedagogical (textbook science) and Popular (press, TV, magazines, etc.). This model enables a trajectory of knowledge, passing through the four stages step by step. That does not mean that the researcher only publishes at one level, knowledge can be published at all stages. It can also happen that the produced knowledge is not equally accepted or taken up at all stages. It is also possible that each stage influences the others, knowledge can travel in each of the directions in this model. A graphical representation is shown below, in Figure 2.

It can also happen that ideas sidestep some stages, which Bucchi termed ‘deviation’. This deviation happens most often in crisis situations. When a conflict cannot be resolved in the scientific community, it might require the intervention of the public to bring it to a close. While it may be that the Popular stage is only used to address the public, it can also happen that researchers send messages to colleagues through publications that they could not do in other stages because of the limitations imposed at that stage. When scientific controversies are taken to the Popular stage, it is not possible to determine beforehand the outcome. At stake is

often the “definition and negotiation of scientific boundaries.” (Bucchi, 1996, p. 382). The turn to the popular stage can help in establishing a paradigm shift within a discipline. The thinner a boundary that is at stake is, the less easily visible it is for the public. Therefore, scientific tensions brought to the public are often transformed “[…] into matters of demarcation between orthodoxy (science) and deviance (non-science), which can be resolved by the public degradation and expulsion of ‘heretical scientists’ from the scientific arena.” (ibid., p. 383)

The public response to the issues presented to them is influenced by the resonance with certain issues, the visibility of the scientific actors and institutions or the relations between disciplines and the public. Often, the involvement of the public is not able to close a controversy forever, but rather to close it for the time being, with competing views drifting around, ready to be picked up again.

Appealing to the different stages can include or exclude certain actors. This can then also be used as a tool for drawing boundaries. There are boundaries drawn around the ‘right’ and ‘wrong’ way of addressing the public.

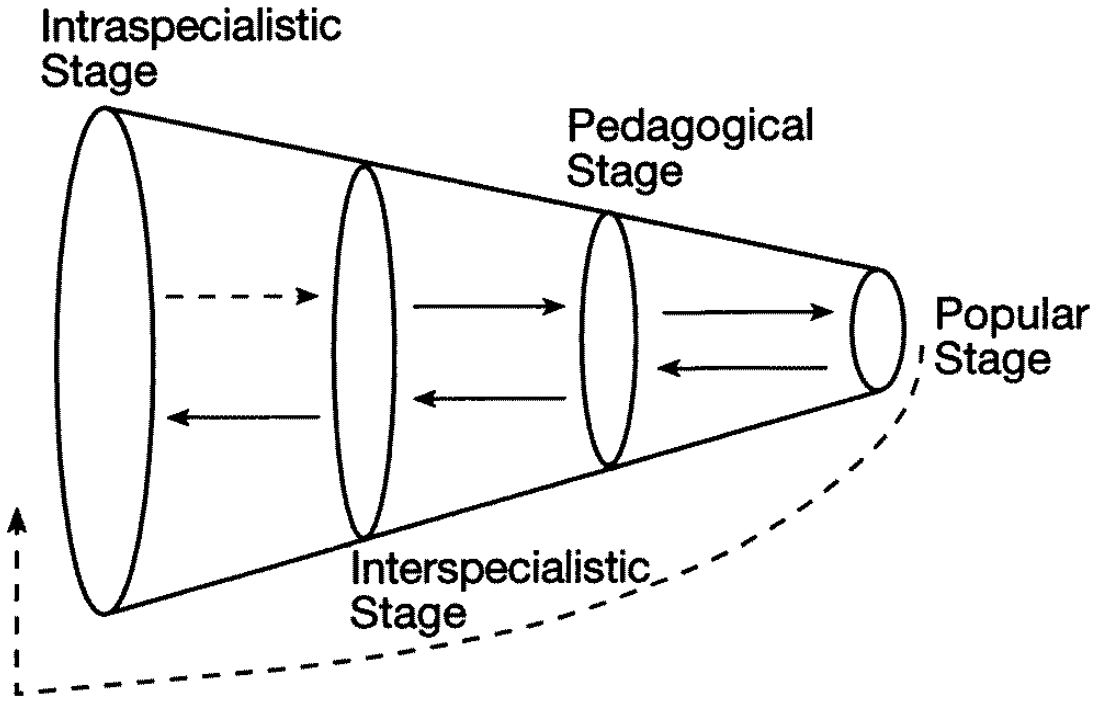

Figure 2: A graphical representation of the four stages introduced by Bucchi (1996). Taken from the same source.

Coming back to the ‘deficit model’, Cassidy (2005) argues that the model which has been described as ‘canonical’, ‘dissemination’ or ‘deficit’ is problematic on “empirical, theoretical and

political grounds." (Cassidy, 2005, p.115). Using the case study of the media coverage of evolutionary psychology (EP) in the 1990s in the UK, Cassidy shows how the deficit model is lacking in explanatory power. Evolutionary psychology is a theoretical framework that sees the human psyche as having adapted evolutionary through natural and sexual selection. EP is an unusual case in the way that it was discussed along the lines of its social implications rather than its scientific foundations. Cassidy traces the appearance of EP in the media and shows that the discussion of the topic in the academic literature started after a discussion in the popular media. This turns the deficit model on its head, going exactly in the other direction, but following in line with the continuity model. Cassidy argues that EP is also a deviation process of science communication and that the continuity model still holds for the majority of science communication. In the case of EP, the discussion in the media started with pop-science books published by two authors in the first half of the 90s. The connection between book publishing and newspapers is a routine one. Newspapers profit from the possibility of coverage over new books as well as interviews with the authors and the authors of the books profit from the publicity thus gained. Nevertheless, this coverage of EP in newspapers led to a rise in the publishing of academic literature on that topic, which also did not drop when the news coverage about it dropped. The amount of published academic literature continued to rise to the end of the 90s. What was also different from other scientific coverage by the press was the percentage of contributions by science journalists compared to scientists and general journalists. In the case of EP, the percentage of articles written by science journalists was lower compared to other topics, while the percentage of scientists and general journalists was higher. This is due to the way that popular books and their media coverage give scientists the opportunity of intervening more directly in the media, by giving interviews or writing comments. The other factor is the implications of EP on the daily life of people, which led to the higher coverage of it by general journalists. This means that while in the deficit model others (the media, science journalists, etc.) would argue over the scientific findings, in the case of EP the scientists themselves were involved.

In her follow-up article, Cassidy (2006) talks about the relationship between scientists and the public domain. This relationship is seen as ambivalent: on one hand, scientists are admired and valued for their engagement with the public, on the other public engagement can in some cases reflect badly on science, with the scientists involved seen as egoistic and brutal. Cassidy also writes:

> Public debates of this kind draw attention to the sciences, but they also violate many of the implicit rules of scientific conduct, most notably the peer review process, but also

> ideas about scientific discourse being polite, subsuming personal opinions and being above commercial influences. (Cassidy, 2006, p.176)

Cassidy draws on Gieryns work to argue that discussions that revolve around the basics of science are mostly fought out in the public domain, where less restriction apply than in the domains of science. Through this, boundaries of what counts as science or non-science are constructed and defended, as well as support gained for specific approaches or disciplines. Given the broader audience and low-threshold language, mass media can be used for communicating across disciplinary boundaries and reaching wider audiences of professionals.

Nevertheless, there are also standards attached to the communication of science: “Overenthusiastic, excessive or premature (in terms of the science or of the career of the scientist) popular discussions of science can still be seen as illegitimate, especially from within the academic arena.” (ibid., p. 197)

Mellor (2003) gives in his article an overview of the importance of the PUS movement in the UK in a historical context. Science was to be brought to the public, with the aim to explain science. This led to the creation of the genre of popular science books. The genre leads to expectations towards the content and the stance of the books. In this genre there are three main modes of addressing the reader: narratival (with the author telling a story about the history or the figures of science), expository (centred around the exposition of a discipline and/or a specific topic, giving a detailed view of a discipline or topic), investigative (journalistic investigations into controversial topics), with most of the popular science books using all three modes together. Even though there exist these three modes, most of the popularizations adopt a distinct pedagogical tone. The assumption is that readers approach these texts to learn something.

There is always the rhetoric of accessibility in play with pop-science books. There is an assumed boundary between science and non-scientific publics. This boundary is seen as something that is needed to keep science working intact, on the other side it is seen as something that has to be bridged so that the publics can understand and support science.

Mellor argues that pop-science books form ‘nodal points in an intertextual web’, which stabilizes which constructions of science are popularized. In contrast or addition to Lewenstein (1995) this means that in non-controversial times or spaces, popularization helps in stabilizing the construction of science. This stabilization can then be drawn on as a cultural resource in negotiations and controversies about science. Popularization, Mellor argues, can be seen as ‘routine boundary work’. It is an

> […] ongoing day-to-day maintenance work that reinforces and sustains the social and epistemic status of science. This routine boundary work is significant because it maintains a cultural resource of normative images and understandings of science which acquire a wide public circulation and can be invoked whenever challenges are made to the position of science in society. (Mellor, 2003, p.521)

This day to day boundary work is seldom seen as something controversial and is seldomly contested. It is not only a boundary work in the now, but also establishes authority in the future. This is mostly done by invoking the history of science and its successes. The anchoring of the past in the present gives rise to the view that in the future science will be as important or even more than it is today.

Science communication is not always about the communication of established facts. Amelung et al. (2020) show with their case study of forensic genetics an instance where the uncertainty of specific scientific methods is highlighted in the communication with the public. The imagined public is too confident in science's powers to deal with genetic forensic evidence and has to be educated about the possible shortcomings and uncertainties of this technology. In a different vein, Böschen et al. (2010) have shown that science does not only produce knowledge but also non-knowledge. Uncertainties are also made and handled differently in different epistemic cultures of disciplines. This includes also the communication or non-communication of non-knowledge, which can then also be used in different political settings for different goals.

3.5 Excurse: Mechanism, Organism/Holism, Vitalism

I want to give an excursion into three important concepts in biology: Mechanism, Vitalism, and Organism/Holism (these terms are capitalized to separate them from other words written the same, with different meanings). They will become more important later on and I wanted to devote some space to a better explanation of them. All these concepts come into consideration when looking at organisms, their nature, and the best ways to investigate them (Allen, 2005). At the core, these concepts are more than theoretical considerations, they can be seen as ontological standpoints about biology, life itself (and whatever that might mean to the different concepts), and the ways researchers should interact with it. Without wanting to classify them, each can be understood as a different biological/philosophical Weltanschauung. These ontological standpoints are often not held openly, and only in rare instances are they mentioned and discussed in the open. Nevertheless, they influence how scientists did and do interact with biological research. They can furthermore be seen as cultural maps in Gieryn's sense, since they have been established historically, at different times. These concepts are important to consider for the case at hand. Sheldrake and Rose are trying to answer questions

that lie close to the kind of questions that each of the Weltanschauung has tried to answer. The concepts run through the case invisible and at times, as we will see later on, quite visible.

3.5.1 Mechanism

Mechanism can be classified, following Allen (2005), as a form of materialism. The word stands short for mechanistic materialism, a distinction that will be important later on. Mechanism sees the world as "[…] a mosaic of separate, independent parts. A detailed description of each of these parts and their interactions would produce a complete description of the system" (Allen, 2005, p.265). Not only that but, "Embedded in this Mechanistic view was the assumption, as in physics, of a strictly deterministic universe where, if one knew all the inputs, it would be theoretically possible to predict the precise outcome of any process" (Allen, 2005, p. 269). Following this, the analytical method of Mechanism is to take apart a system and try to understand all its parts. This way of reducing a system to its part can be described as a reductionist approach: higher levels of organisation can be completely understood by their lower levels of organisation. This leads to a top-down view, where the search for understanding goes lower and lower in the levels of organisation to reach the most basic processes and explanations. This research approach relies also heavily on separating parts of the system to understand their specific characteristics, which could not be achieved when looking at the whole system altogether. In the historical process that formed this view on biology, mostly in the late 19th and early 20th century, experimentation became the methodology of the highest importance to distinguish between often conflicting hypotheses. If a hypothesis was not capable of being tested by experimentation it was deemed as meaningless. Mere speculation and theories not based on experimentation (which was for a long time the modus operandi of biology) were seen as not worthy of a place in biology (Allen, 1981).

In the early 20th century, Mechanism was still something that had vocal proponents, while today the mentions of Mechanism become very rare. It is still used rhetorically by its opponents, but Mechanism has become a big part of the development of biology as a field and is today deeply embedded in it.

One of the most notable historic proponents of Mechanism was Jacques Loeb (1889 -1924), who will also become more important later. Loeb came to be known through his experimentation on mimicking fertilization in sea urchins through physical stimuli. Loeb had strong views on how the progress of science could be achieved and believed that no barrier should stand between scientists and their complete control of life phenomena (Landecker, 2007). Control and experimentation were closely connected in Loeb's views. Loeb thought of reductionism being the crucial analytical process of understanding biological systems. He was

also committed to using Mechanism to bring the at his time relatively new field of Life Sciences from being a ‘soft’ science to a ‘hard’ science (Allen, 2005).

3.5.2 Organicism/Holism

Organicism and Holism (both terms can be used to mean the same thing) can be seen as standing in opposition to Mechanism. Holistic biologists were more concerned with how biological complex systems functioned as a whole. Holism sprang from a critique of Mechanism, with it being seen as too simplifying and using the wrong level of analysis to look at biological systems. Holism shares nevertheless the materialistic viewpoint of mechanistic thinking. This means that holistic thinking is “[…] seeking to account for living processes as functioning wholes within the framework of known physical laws.” (Allen, 2005, p. 266). This is an important distinction criterion to Vitalism (see below). Holism does not think that new forces are needed to describe the universe. A main tenet of Holism is that biological systems cannot be seen as machines, they are capable of activities that cannot be understood through a machine metaphor. This led to the conclusion that systems should not be taken apart to be analysed, but rather that the whole system should be looked at as different levels of organisation, with each level being investigated on its own terms. While a reductionist approach is not seen as forbidden, Holism strives to move further, and analyse all the levels in a system. Holism views the levels as having properties that could not be explained by just looking at the parts of the level:

> For holistic biologists, complex systems (even very simple ones) show emergent properties that are the product of the individual parts plus their interactions […] But emergent properties could not be predicted from knowing only the individual components making up a given system. Explanations relevant to a given level had to be derived from studying that level itself and not merely by isolating its lower-level components. (Allen, 2005, p.268)

3.5.3 Vitalism

Vitalism emerged from Holism but does not share the materialistic understanding with Holism. That means that Vitalism “[…] claimed that living organisms defy description in purely physico-chemical terms, because organisms possess some non-material, non-measurable forces or directive agents that account for their complexity.” (Allen, 2005, p.267)

Vitalism was from all the currents in biological thinking mentioned here always the one with the worst position. While Mechanism and Holism have been widely accepted by major parts of

biology throughout history (even though in changing proportions), Vitalism always held a marginal position in scientific debates.

One of the most notable historical figures regarding Vitalism is Hans Driesch (1867-1941). He will also be of importance later. Driesch came also to be known for his experiments on sea urchins. When working with stages of the embryonic process in sea urchins, Driesch took the results as a sign of opposition to the mechanistic models that many researchers before him had postulated about the same topic. He nevertheless spent the next seven years of his academic career searching for a mechanistic explanation of the processes that he encountered. After these years “He claimed that embryonic development was guided by an ‘entelechy’, an organizing, directive force that consumed no energy, was immaterial, but was the factor that distinguished living from non-living matter. Eventually, Driesch abandoned experimental biology altogether for philosophy.” (Allen, 2005, p. 271). This interpretation of the results of his experimental research was an evident vitalistic interpretation, which showed no connection to any of the known physical forces and left all materialistic grounds behind. Through this, Driesch moved further and further away from mainstream biology of the time.

4 RESEARCH QUESTIONS

The case study at the basis of this master thesis is the controversy between Sheldrake and Rose. The case revolves around their divergences of opinion, their attempt at a joint experiment, and their attempts at embedding the failed experiment into their own views. The basis of the case will be different textual documents that have been written by the two actors.

This case study of a failed biology experiment gives rise to an interesting site for studying the connection between produced knowledge, its boundaries and communication. The mash-up of popular science communication, as well as specialized communication connected to an experiment created and executed jointly by opposing sites of the controversy, makes this case a unique way to study boundary work in action. This project aims to analyze both sides of the controversy and the presentations of themselves and their opponent in the material. The main research questions will therefore be:

How does the controversy between Sheldrake and Rose about the formative causation experiment unfold?

The focus here lies on the views of the actors, the controversy is a situation arising between them and they are the most important players in it. It makes sense to view the controversy from their positions, to see what issues paved the way for the controversy and what problems both wished to resolve.

This main research question will be divided into smaller sub-questions

Question 1: **What is the controversy between Sheldrake and Rose revolving around in the actor's view?**

This question aims at understanding the actors' view of themselves in the controversy. It is closely related to issues of boundary work. This sub-question allows uncovering the relation of the controversy at hand with previous controversies and other cultural resources that get reused and integrated into the controversy. This sub-question tries to identify the rhetorical instruments and strategies that both employ to position themselves and each other.

Question 2: **What is the status of the experiment in the controversy?**

This will help to show the possible ways in which both researchers try to navigate the experimenter's regress and what methods they use to close the controversy. Here the aspect of what could be at stake by the different possible outcomes of the controversy will also be

shown. Since the status of the experiment is rather unique in this controversy, with opposing sides creating a shared experiment, special attention to how this influenced the controversy will be taken.

Question 3**: Through what arenas does the controversy move and how is it affected by them?**

This question focuses on the different arenas the controversy plays out in. Each of the arenas is influencing the controversy and the possibilities of action of the actors. This question aims to flush out how the actors react to the possibilities and limits of the arenas. Through this light is shed on the changes in the course of the controversy as well as the reasons for specific actions of both actors.

Question 4: **Why is no closure of the controversy possible?**

This last sub-question is exploring the possible reasons for the non-closure of the controversy. It is a more exploratory question with no claim to giving a definite answer. Nevertheless, it helps to understand different possible outcomes of the controversy and what the actors could have done differently.

5 SENSITIZING CONCEPTS

To answer the research question and the connected three sub-questions, three sensitizing concepts will be used. The main sensitizing concepts used will be the experimenter's regress connected to the analysis of its role in the controversy and the concept of boundary work.

5.1 EXPERIMENTER'S REGRESS

The first theorizing concept will rely mainly on the work done by Pinch and Collins, who looked at the human factors involved in controversies. Here the focus lies on the concept of experimenter's regress, as described in the work of Collins (1985).

He introduced the 'experimenter's regress' stating that to argue over the quality of a method used to test something is to argue over the quality of the phenomena under scrutiny. Collins focus here lies on the often-overlooked social criteria that influence decisions regarding the trustworthiness of experiments. In his empirical work, Collins found that if an experiment and the phenomenon it tests are not seen as firmly set in the practices of a scientific community, the uncertainty regarding either the phenomenon or the experiment will always include the other since it is not possible to fix one without the other. This view is opposed to the view often held in different scientific communities that the facts or nature would speak for themselves and that specifically designed experiments could elicit these answers. Collins, therefore, argues also against a crucial experiment, since not nature decides what counts as the best representation, but the scientists involved in the debate around a phenomenon. From his work Collins (1985) voiced propositions about experiments used in science. Some of these propositions are especially relevant to the case at hand: The proper outcome of experiments defines the ability of experimenters and the functionality of the equipment. Scientists believe that nature can be manipulated by specific actions within a set of rules. This makes experiments possible. If no successful outcome of an experiment is present, scientists argue about the competence of experimenters and apparatus.

> Where there is disagreement about what counts as a competently performed experiment, the ensuing debate is coextensive with the debate about what the proper outcome of the experiment is. The closure of debate about the meaning of competence is the 'discovery' or 'nondiscovery' of a new phenomenon. (Collins, 1985, p. 129)

"Decisions about the existence of phenomena are coextensive with the 'discovery' of their properties." (Collins, 1985, p.129). Phenomena with radical properties (phenomena whose existence would question the basics of scientific communities) can only exist in institutions and

niches, which overlap only marginally with science as a whole. If not, either the phenomena or science as a whole must change. The farther away in social time and space the creation of knowledge was, the more certain it appears to the onlooker.

These propositions can help untangle the role of the experiment in this controversy. Especially important are the propositions that revolve around the competence of the actors and the existence of the phenomenon. Both sides of the controversy in the case tried to have the experiment as the deciding factor in their argument. This relates to the propositions of Collins since both actors in the controversy believe that the experiment can be a deciding factor. The predictions both wrote down at the start tried to determine what would count as a successful outcome of the experiment. But the results, rather than to lend weight to one of the two predictions, opened the way for an experimenter's regress. Since there was no decision on the existence of the phenomenon, the experiment came back into the focus, coming back to the proposition that the discussion over competently done experiments defines the existence of phenomena. Both sides in their documents work to close the experimenter's regress. To see what factors were deemed important to close the regress and how they were presented will be one of the foci of the project.

Collins (1985) also describes the possible effects of different strategies of scientists to declare results in favour of a specific hypothesis. The more generally applicable results are, the more interesting they become to specific scientific communities. Scientists will most likely try to make their claims as broad as possible, to focus attention on them. But if the hypothesis and results do not preserve the social basic assumptions about the natural world of specific groups, they will cause an uproar. This can be easily seen in Parapsychology. Because it most of the time redefines basic assumptions of the natural sciences, it threatens too much of too many social realities to be accepted as scientific. Each subgroup of science has its own beliefs about reality and therefore will be involved in the controversies that threaten its specific reality. Thus, the arguments of scientists will be affected by their perceived place in the community and their ambitions. Because the resources of experiments alone cannot solve the experimenter's regress, non-scientific tactics must be employed to solve it. These tactics are rendered invisible once the controversy is solved.

The views of scientists on their colleagues or adversaries and on the experiment itself shape the outcome of scientific controversies. These two factors can now be combined to encompass two possible outcomes: the experiment and the experimenter are competent, or they are incompetent. This can be seen in Figure 3.

Scientists' Views Regarding the Competence of Experimenters and the Integrity of Their Experiments

		Scientist believes in phenomenon under investigation	
		Yes	No
Experiment finds results consonant with phenomenon	Yes	1. Competent	2. Not competent
	No	3. Not competent	4. Competent

Figure 3: Competence of experimenters and their experiment. Taken from (Collins, 1985, p.146)

To strive for competence, researchers can get help from inside and outside the 'core set'. The 'core set' are all the researchers involved in the scientific controversy. Allies outside of the core set can help through financial, publicity or publishing support, among others. Allies within the core set can help support through taking sides, using the discussed theories themselves and acting as though the theories are reasonable.

In the controversy in this project, the two researchers involved in the experiment have opposing beliefs about the phenomenon under investigation. Comparing that to the figure above, the answer to the question if the actors believe in the phenomenon are different. That means that depending on the actors' beliefs in whether the experiment finds results consonant with the phenomenon, they will either view the integrity of the experiment as intact or not. The integrity of the experiment now relies on the re-interpretation of the results as either consonant or not with the experiment. This leaves open room for navigation. The belief of the actors in the phenomenon might be rather fixed, but their trust in the experiment might not. The documents of the case study can be seen as ways of engaging allies within and outside of the core set, which could help close the controversy. But different strategies and tools might be usable by the different sides of the controversy, which depends on their position in the grid of Figure 3.

As can be seen, the breaking of the experimenter's regress is not possible solely with ways of experimental planning or theoretical deductions. Other factors, such as the credibility of the researchers involved or other social components are needed for resolving the experimenter's regress. These social factors are often in everyday scientific settings defined as not being part of science. In the context of controversies or the experimenter's regress, these factors become part of the scientific process. These 'non-scientific' methods become for a short time scientific to help solve problems. What kinds of factors, methods or other things are included in scientific argumentations in the case of controversies and the experimenter's regress, is something that is closely connected to another guiding concept, boundary work. The next section will deal with boundary work and also give more detail to this connection.

5.2 BOUNDARY WORK

The second sensitizing concept will, therefore, be Gieryn's "boundary work" (Gieryn, 1983, 1995, 1999). The concept of boundary work deals with the boundaries of science, what is classified as science and what as non-science and analyses the societal actors that do boundary work.

Boundary work as a concept evolved through the critique of essentialist attempts to define criteria that apply exclusively to science in every circumstance and every temporal dimension (Gieryn, 1995). Such essentialist attempts at demarcation were amongst others Popper (with his demarcation criterium of falsifiability), Merton (with his four norms of science) and Kuhn (with the shared paradigm). All these demarcation criteria crumble under closer inspection, exposing the contingent character of them which is routed in social situations. Nevertheless, science holds a strong cultural authority. If this authority cannot be explained through criteria inherent to science, boundary work tries to explain this authority by how the boundaries of science are drawn. "Constructivists argue that no demarcation principles work universally and that the separation of science from other knowledge-producing activities is instead a contextually contingent and interests-driven pragmatic accomplishment drawing selectively on inconsistent and ambiguous attributes." (Gieryn, 1995, p. 393).

The concept of boundary work then sees science as a kind of empty vessel, with no clear demarcations that could be measured, named or standardized. Only when scientists are involved in discussions about the demarcations of science, the vessel becomes filled.

> "But what *is* "science"? Nothing but a *space*, one that acquires its authority precisely from and through episodic negotiations of its flexible and contextually contingent borders and territories. Science is a kind of spatial "marker" for cognitive authority, empty *until* its insides get filled and its borders drawn amidst context-bound negotiations over who and what is "scientific" (ibid., p.405, italics in the original)

As the word 'boundary' implies, boundaries are drawn between science and non-science every time discussions arise over what exactly constitutes science. "Boundary-work occurs as people contend for, legitimate, or challenge the cognitive authority of science—and the credibility, prestige, power, and material resources that attend such a privileged position." (ibid., p.405). Furthermore: "the authority of science is reproduced as agnostic parties fill in the initially empty space with variously selected and attributed characteristics, creating a cultural map that, if accepted as legitimate, advances their interests." (ibid., p.405f). Gieryn uses here the metaphor of a cultural map, something that he often refers to, as the pivotal point in boundary

work. The acceptance of these cultural maps by specific groups is the essential ground for the shift of boundaries. The more convincing these maps are, the better they are in shifting boundaries. Here the connection to the theoretical lens of the experimenter's regress can be drawn. While the experimenter's regress is not concerned with the broader boundaries of science as such, one can see the connection to the cultural maps Gieryn mentions. For novel experiments to be accepted as credible, along with their makers, a cultural map has to be drawn that establishes the researchers and their experiments as credible. If this cultural map is accepted by others, then the experiment and the researcher as seen as credible. Others can and maybe will also submit their cultural maps in the question of new phenomena. Then it is the question of which of the cultural maps will be accepted by whom. Reasons for this acceptance can be many but are sometimes also contingent on earlier episodes of boundary work as we will see below.

Episodes of boundary work and their definition of science are always "local and episodic accomplishments" (ibid., p.406), but that does not mean that they cannot draw on maps that have already been drawn before: "Those contesting the borders of science select from and creatively reconstruct past episodes of boundary-work, often using old maps to legitimate the validity of their own" (ibid., p. 406). These older maps can be seen as a 'repertoire' to choose from. This brings us back to the experimenter's regress. As already stated, the cultural maps that are designed in these situations can draw on older maps, other instances of boundary work. Not only in the case of the decisions about the credibility of experiments is this important but also in the case presented about what counts as science and what not. The existence of older maps and instances of boundary work is something that will influence the analyses of the case at hand. It is necessary to see the controversies that have already played out in the field of biology over its historical existence as crucial to the understanding of boundary work at every moment in time. Without the knowledge of the repertoire at hand for the actors in the controversy in biology in the 1990s, one would miss insights into the already established and accepted authority that some maps might hold with certain audiences. Therefore in the state of the art, a small excurse has already been given into the history of different philosophical standpoints in biology concerning the theoretical foundations of biological thought, to help better understand what kind of repertoires and maps can be drawn on and how they can be creatively reused.

Boundary works as well with the concept of a "social world" (Gieryn, 1995, p. 412). Social worlds are groups of people "with shared commitments to the pursuit of a common task, who develop ideologies to define their work and who accumulate diverse resources needed to get the job done." (ibid., p.412). 'Getting the job done' here focuses on how work is done, which

also includes the representation of said work. This representation can happen in many instances, one of them being science communication in all its forms. While I already covered the broader literature on science communication in the State of the Art, I want to point out here the connection of boundary work to the work of representing science. As seen above, each social world is actively engaging in the representation of themselves. Scientists are participating in this, and each social world of scientists is as well. As we have already seen, this communication can take many forms and can go unchallenged or not, resulting in controversies or not. “Very important activities within all social worlds are establishing and maintaining *boundaries* between worlds and gaining social *legitimation* for the world itself. […] These processes involve the social construction of the particular world and a variety of claims-making activities” (Clarke, 1990a, p.20, italics in original). This legitimation is often achieved through science communication: “Therefore, scientists need to convince others that the work they do is legitimate, that their knowledge claims are valid, that scientists should have jurisdiction over scientific knowledge, and that they should be supported by the rest of society.” (Cassidy, 2006, p.178). This legitimation can happen in instances of ‘purely’ scientific presentation, through published books or articles in journals, or through the more ‘popular’ route of engaging with the non-scientific public: “[…] exposure in the public domain may, at times, help individuals or groups make arguments or achieve visibility in a way otherwise not possible in more specialized communication forms.” (Cassidy, 2005, p. 135). The route of the ‘popular’ science communication is heterogeneous and the route, as well as the content of the communication, can become the object of boundary work: Mellor (2003) draws on Hilgartner to make the point that there is a demarcation “between ‘appropriate simplification’ and ‘distortion’” (Mellor, 2003, p. 516). Not only is it important where something is said, but also how it is said.

The concept of a social world is also taken up by the methodology of situational analysis. It serves as one of the basic instances of situational analysis. While situational analysis will be discussed in more detail in the Methods section of this thesis, I wanted to show here the close connection of boundary work to this methodology, since they both incorporate the same approaches towards social concepts.

Boundary work also plays an essential role in the safeguarding of science’s cultural authority by expulsing members who threaten this authority.

> Sanctioning deviants is also an opportunity for corrective public relations campaigns, restoring among powerful constituencies elsewhere in society a belief that science on its own is capable of weeding out impostors (so hands off) and restoring confidence

> that science is really only what genuine insiders say it is (nothing dirty going on). (Gieryn, 1995, p. 432)

This weeding out is never without the interest of different parties attached. At stake with this kind of boundary work is what practices, thoughts and theories constitute science, and which people are allowed to represent that.

> As is probably the case in most allegations of pseudoscience, amateur science, or deviant science, decisions about inside-outside forced debate over where that border shall be drawn—debate that becomes contentious because of diverse interests attached to the map that eventually wins out as (provisionally) accurate. (ibid., p. 433)

Drawing boundaries in this instance will always leave some individuals outside of science, with very real personal consequences for them. A successful expulsion strips the expulsed of all cultural authority attached to them. These boundaries are hard to redraw since the cultural map that led to expulsion will still be quite fresh in people's minds. This aspect of boundary work is important in the controversy at hand because expulsion is always at stake for one of the involved parties. This point will be elaborated more in the empirical chapter.

The social interpretations of the word and concept of science are therefore important to consider. Close attention to the boundary drawing in controversies can show what factors may be acceptable to influence the outcome of it. These boundaries rely heavily on power relations, rhetorical devices and the enrolment of allies. Boundary work will be essential, as a sensitizing concept, since it can show the importance of different rhetoric styles and arguments regarding the declaration of what constitutes science. Only through language can boundaries be drawn, and language is connecting the boundaries to be drawn with the call for action. Boundary work will be used in this thesis to pay close attention to what is defined by both actors as 'inside' and 'outside' of science, which cultural resources and repertoires they draw on, and how they navigate the boundaries set by the other. Furthermore, boundary work might shed light on what may be at risk for the involved parties in the controversy.

5.3 LATOUR'S THEATER OF PROOF

Another concept I will be using in guiding my analytical work is Latour's concept of 'theater of proof' that I have already in part elaborated in the State of the Art. A theater of proof is constructed around an experiment, for the sake of onlookers or an audience, to convince them that the shown proof is 'real'. As already stated, this theater needs a few things. Before it can happen, the audience has to become interested in it. The other necessity is the 'clearness' of proof. The proof has to be so dramatic and undisputable as to make it absolutely clear that no

other interpretation than the proof could work. I will use the quote again, because I think it highlights perfectly what is necessary, for such a theater of proof to work: “Without this double movement of interest and dramatization, the tests might have been indisputable but would either have remained undiscussed because they interested nobody or […] have been discussed but remained disputable.” (Latour, 1988, p.87). As has also already been mentioned, a theater of proof needs utensils, it cannot exist on its own. These utensils can be the laboratory, can be techniques of work, but also literary resources. Even a scientific article can be construed as a theater of proof. Articles and experiments are deeply intertwined:

> The so-called technical article does not float over laboratory experiments like some empyrean. It is part of the action (Callon et al.: 1986). It is the action itself, the action that constructs credibility and makes the "scientific facts" disputable or indisputable. (Latour, 1988, p.101)

The dramatization or ‘rightness’ of the proof in scientific articles is achieved through the language used and the literary resource woven to paint a specific picture:

> An article, especially a scientific one, is a little machine for displacing interests, beliefs, and aligning them in such a way as to point the reader, almost inevitably, in a particular direction. Scientific rhetoric often channels the reader's attention in a single central direction […] (ibid., p.19)

The interest of the audience is often given when it revolves around scientific articles since they are most often read with the idea of gaining information from them. Nevertheless, the readers of articles have to also be interested in the specific problem discussed, to then accept the proof:

> A scientific article is not of course a description or a distraction. It is a means of pressure on readers, convincing them to change what they believe, what they want to do, or what they want to be. In order to construct those paths that attract, move, force, or do violence to the reader, the author associates himself with everything that may tend to support his point of view and to make his conclusions as indisputable as the course of a river through a V-shaped valley. (ibid., p.94)

What for the experiment in the theater of proof are the machines and the technical skills, are in the articles in the theater of proof the already existing ‘cultural maps’ and rhetorical skills. Of high importance here is, as in the other concepts before, that others have to be convinced for a specific description of a phenomenon to stick. To just introduce a solution, an actor, a phenomenon is not enough:

> To convince someone that an experiment has succeeded, that a technique is effective, that a proof is truly decisive, there must be more than one actor. An idea or a practice cannot move from A to B solely by the force that A gives it; B must seize it and move it. […] An idea, even an idea of genius, even an idea that is to save millions of people, never moves of its own accord. It requires a force to fetch it, seize upon it for its own motives, move it, and often transform it. (ibid., p.15f)

Other actors have to believe in the 'rightness' of the new actor and its connections to other actors, and in its power. Here the distinction between belief and knowledge becomes non-existent. Both are tied to the personal investment the actors have in the new actor: "Indeed, the exactness of a science does not come from within. It, too, comes from the strength of the agents with whose fate it has managed to become linked."(ibid., p.52)

What this means for the case at hand is close attention to how the experiment, as well as the documents dealing with it, are constructed. The concept of the theater of proof helps to think through how these instances have been designed to influence those 'watching' it. As with the other concepts, the theater of proof shows that what gets accepted becomes 'real', which puts great importance on the understanding of the mechanisms used to get things accepted. This means close attention to the construction of documents through their language, as well as all the resources they draw upon as well as the circumstances of their production. How exactly this was done, is the topic of the next chapter.

6 MATERIAL AND METHODS

To better understand what material was chosen and for what purposes I want to dive in more detail into the case and its temporal dimension before reflecting on the considerations made in the choice of materials and methods.

6.1 TIMELINE

As already mentioned, I find it helpful to get a better look at the whole controversy before zooming in into the different steps. Below is a graphical representation of the timeline.

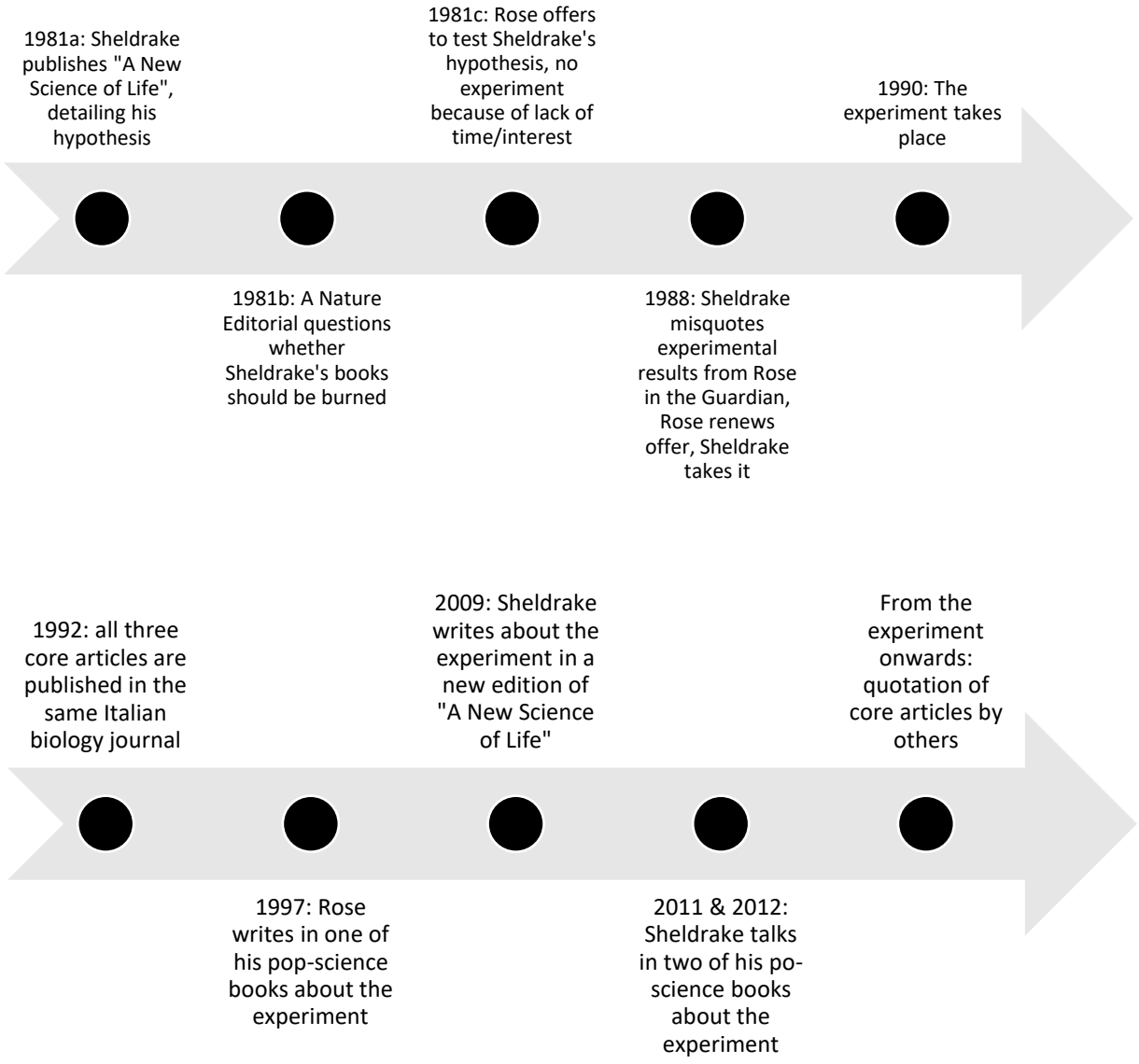

Figure 4: Timeline of the controversy

The controversy around Sheldrake is also the starting point of the controversy between Rose and Sheldrake. 1981 Sheldrake publishes his book “A New Science of Life”, which contained the first description of his hypothesis of ‘morphic resonance’ (Sheldrake, 2009 [1981]). This book and its content were in the same year also discussed in the journal *Nature,* where its editor wrote an editorial raising the question if Sheldrake’s books shouldn’t be burned (ANON, 1981). This struck a nerve with Rose, who offered his laboratory and other resources to Sheldrake to test the hypothesis together with himself. Sheldrake declined because of lack of time (Rose, 1988a). Sheldrake then went on to further work on his hypothesis and was also employed by The Guardian to write columns about a diverse range of topics. In 1988, Sheldrake wrote a piece about the storage of memory in the brain, raising doubts about the view at the time that memory must be stored physically in a way in the brain. He also advertised his new book, “The Presence of the Past: Morphic Resonance and the Habits of Nature” (Sheldrake, 1988a). Rose, who has worked most of his scientific life on questions regarding the storage of memories in the brain, wrote a reply in The Guardian, pointing out the faults in Sheldrake's reasoning and research (Rose, 1988a). This led to a reply by Sheldrake and another reply by Rose until they took their correspondence to more private channels (Rose, 1988b; Sheldrake, 1988b). Rose renewed his offer for a joint experiment and this time Sheldrake accepted, and the planning of it commenced. In the summer of 1990, the experiment was carried out. It took place in Rose’s laboratory, financed by Sheldrake, but carried out by an assistant of Rose (Sheldrake, 2009). Both researchers agreed on an experimental design, made predictions of the outcome beforehand and decided to publish the outcome no matter what it would look like. Nevertheless, the data produced by the experiment was interpreted quite differently. Sheldrake took the data as a sign of his hypothesis being correct, while Rose took it as a sign of it being incorrect. No shared understanding and agreement was reached between the two (A1, A2[2]). Sheldrake then published the experiment and his interpretation of the data in a small Italian journal called “Rivista di Biologia - Biology Forum” in 1992. Rose published his comment and interpretation of the data also in the same journal, in the same year. This led to Sheldrake commenting again on Rose’s article, which was also published in the same journal, in the same year (A1, A2). It was clear that there would be through this also no shared understanding achieved. Rose and Sheldrake went again their different ways, while they both still wrote for the Guardian. In 1997 Rose recapitulated the experiment and his view of it in one of his pop-science books called “Lifelines: Biology Beyond Determinism”. In 2009 Sheldrake published his view of the experiment in the new edition of his book “A New Science of Life”. He also included his view in the new edition of “The Presence of the Past: Morphic Resonance and the Habits of Nature” in 2011 and another book he wrote, “The Science

[2] This notation will be explained in a section further below

Delusion", which was published in 2012. While these were the last mentions made publicly by the actors themselves, other researchers picked up the articles published by both and integrated them into their respective works.

6.2 MATERIAL

6.2.1 Considerations in the Choice of Material and Method

The material for this case and thesis were chosen following a few guiding principles. Controversies can be hard to pin down and to delimit their beginnings as well as end points (see the chapter in SotA). The same is true for the controversy between Sheldrake and Rose. It is impossible to say where exactly this controversy started, and where it ended. This is in particular because there are only a handful of documented instances of the two interacting. The emphasis lies here on the word documented. Rose and Sheldrake are both working scientists with many connections to other scientists, other professions and so on. To limit the controversy between the two actors in question and to get a complete view, one would have to know where they met and what they spoke. To tackle the question of the limits of this case different methodological and material approaches were considered. The choice of material and methodology went hand in hand. From these considerations, the choice fell on analyzing documents with a situational analysis approach. The reasons for this choice were the following: there are documented instances of Rose and Sheldrake interacting (more on that below) with quite rich substance. Situational analysis was the method of choice since it enabled a close working with the documents without too much influence of my person on the material. With this, I do not mean that I am an objective and unopinionated observer. This can never be the case, but it is important to reflect upon that (Traweek, 1988). As Cassidy (2006) put it nicely: "Like Gieryn's cartographers of science, I have an inescapable, albeit complex location in these debates, which will in turn affect my analysis." (p.180). Through my engagement with this controversy, I am influencing it (even though it happened 30 years ago). This thesis is through its engagement with the controversy becoming potentially part of it. While this is a known possibility, the influence I have on the controversy should be for analytical reasons minimal. I am without a doubt situated in the mesh of the controversy. The way I became aware of this case was through personal interaction with Sheldrake's theories in my previous academic career. While this was from a medical and biological perspective, this interaction with the theory led nevertheless to me reading some of Sheldrake's work and even meeting with him in London for an interview. The choice of not doing interviews with both of the actors was in part influenced by this. I cannot undo the interview with Sheldrake I did years ago, but to keep the influence of my personal experiences on the analysis as minimal as possible I chose to analyse solely documents, without interviews with the actors. Carrying out interviews would have meant

to be posed with the question of the importance of this one interview on the analytical process. Through analysing documents I hoped to gain a distance to the topics and to the persons involved. Through this, I hope to be able to make the analysis as uninfluenced by personal ties as possible.

Another important point is that I want to treat both actors in the controversy symmetrically. Although my previous experiences cannot be undone, I tried to approach the documents from an analytical perspective that treated both actors as equal and which gave high importance to the documental representations of the actors, held apart from the personal connections to them.

While my situatedness imposes certain limitations, it nevertheless opens up other possibilities. Situatedness is sometimes crucial for the analytical process (Clarke, 2005). Through my knowledge of biology, I am in a position to also engage with the content of the documents differently, than if I had not had my previous experiences.

6.2.2 Choice of Documents

The choice of documents was the next consideration that had to be done. What exactly constitutes a document is place of much discussion (Prior, 2007) and can be different from case to case. In the case analysed in this master thesis documents are defined as textual artefacts that are published by a media source. The media source part of the definition has the reason behind it that there very well might be textual artefacts produced by the others that might not be accessible by the broader public (diaries, personal communications, etc.). Since this master thesis has a strong focus on the way both actors engage with the public, how they present themselves and how they try to sway the public, only documents that are in a way accessible to the public are considered. This access may nevertheless exclude certain parts of the public since subscriptions to newspapers or scientific journals would be needed, but there is nevertheless the possibility of access, which is important.

The material was collected following the identification of the three “core articles” as I call them. These three articles, as already mentioned above produced by Sheldrake and Rose, are the focal point of the analysis. I have known about them for quite a while and they were the starting point of how this case was assembled. Through the references they make, to the Guardian, and the quotations which included them, the pop-science books by the authors and other scientific articles, the rest of the documents were gathered. More on this gathering is below. When searching the internet for the sources, it became quite clear that there was a distinction possible and sensible when analysing the documents. Different documents were published by different kinds of media, had different audiences in mind and had different styles. The form

and content of the documents and their way of being published influenced each other. The core articles are different in their mode of circulation and their way of engaging with a public to the Guardian columns to the Pop-Science books. This is what I will call “arenas”. I draw here on Clarke and her definition of arenas: “Controversies happen in an arena of interaction, a conceptual location where all of the groups that care about a given phenomena meet” (Clarke, 1990b, p.19)

I see as an arena a somewhat closed medial space which both of the actors can navigate. When collecting the documents, it became clear that there would be three rather distinct arenas.

6.2.2.1 First arena and document collection

The first is the newspaper “The Guardian” (an English newspaper with a circulation of 110 000 copies and a political leaning towards the left). Both Sheldrake and Rose were employed by the Guardian for some time to write amongst other things columns on different subjects, amongst them writing about science and biology-related topics. Columns are commentary pieces (Cassidy, 2005) on different subjects, and are quite common in the newspaper practices of the UK. Columns are mostly short documents (a few paragraphs on the side of a page) and therefore do not allow in-depth discussion of topics.

I have gotten access to the archive of The Guardian and searched for “Sheldrake” as well as “Rose” in the timespan of 1980-2000. This includes the time of the joint experiment (1991), as well as the time the proposal has been made to test the hypothesis (around 1988), as well as an extended range of time to account for any other important data. The search came back with over 200 matches in that time period. Not all are relevant, a lot of newspaper articles were written about Sheldrake. Nevertheless, four columns are especially important for the research done and can be clustered together. These columns are all written in 1988 over the course of four weeks. It started with a column written by Sheldrake and the following columns are replies and replies to replies. Since they all are direct responses to each other and revolve around the same topic, they were deemed as a group and analysed together.

The Guardian classifies as an arena because Sheldrake and Rose had access to it, navigated it with their written columns and parts of the public had access to it.

6.2.2.2 Second arena and document collection

The second arena is the peer-reviewed Italian biology journal “Rivista di Biologia-Biology Forum”. The second arena now comprises the three core articles, all published in 1992. Two

of these articles (the first and the third in the timeline) are written by Rupert Sheldrake and one by Steven Rose. For the simplicity of reading and quoting from these sources, the articles will be quoted as A1, A2, and A3. This choice was made to make it easier to follow the temporal aspects since the quotation with name and year was becoming cumbersome and potentially confusing to the reader. The three articles together compromise 25 journal pages, containing text as well as some figures. These are the aforementioned core articles. I call them core articles because they deal with the experiment done by both actors, which will be analytically important. They are also the main place of action, in the sense that the other two arenas are not as centred on the interaction of Sheldrake and Rose.

Biology Forum describes itself on its website as:

> "Founded in Italy in 1919, «Theoretical Biology Forum» (formerly «Rivista di Biologia / Biology Forum») is one of the oldest biological journals in the world. It is devoted to *theoretical biology*, in its broadest sense. The journal aims at going beyond specializations, discussing, before a multidisciplinary audience, biological topics of general theoretical interest. Areas of interest include (but are not limited to): Evolutionary Biology, Developmental Biology, Molecular Biology, Neurosciences and Biology of Mind, Complexity of Biological Systems, Biomathematics, Biophysics, History and Philosophy of Biology. All topics are dealt with from a theoretical standpoint, the focus being on concepts rather than on technical details. Mathematical formalism is reduced to a minimum, so that articles are also accessible to non-specialists. Experimental papers are not accepted. «Theoretical Biology Forum» publishes research articles, reviews, 'perspective' articles, book reviews, letters to the editors." (Fabrizio Serra editore, n.d.)

This arena was chosen by both actors. While the first arena was more coincidentally the place of controversy (as we see later on), this journal was chosen by the actors. The reasons behind it were not discussed in the material, but there is some room for speculation. The first obvious reason is that the topic of the experiment fits into the description of the journal. Since it deals with theoretical biology it seems logical that an experiment concerning new theories should be a good fit. There is on the other hand another reason that might have played a role in the choice of journal. At the time this specific part of the controversy played out Sheldrake was already on the margins of canonical science (ANON, 1981). It is not easy for researchers not following the consensus of mainstream science to find places for their articles, in part because of the peer-review process (Fyfe et al., 2017). A rather small Italian biology journal might have lesser gatekeeping structures in place than for example journals in the UK, the country where both actors work and live.

This arena has certain limitations to it concerning accessibility for analysis. While it was relatively easy to find the articles and to connect them to each other, the access to the articles was problematic. They were published before journals had a strong online presence, therefore they were published in print. They were not uploaded digitally since as some other articles have been. Biology Forum has a website, but they have not uploaded old editions, only started in 2011 to upload their journals. The articles were accessed through the website of Rupert Sheldrake. Rupert Sheldrake has a strong online presence as an author and researcher and has on his website given public access to a range of his written research (Sheldrake, 2019, "Morphic Resonance"). On his website, Sheldrake has also uploaded the three articles. The articles are in text form, not in PDF or scanned pages from their original publication, which makes the allocation of quotes to specific pages of the original publication nigh impossible. While the text is presented as it would have been printed, an interference on Sheldrake's behalf can not be ruled out. The texts were used in the trust that they were not tempered with. Attempts to get physical copies, as well as digital copies of the original printed articles, were up to date fruitless.

6.2.2.3 Third arena and document collection

The third arena is the popular science books written by Sheldrake and Rose. Both authors have written several popular science books on a diverse number of subjects, but not all were relevant for this case. Only those books were chosen for analysis that dealt with the experiment and the interaction of Rose and Sheldrake. This was checked through quotation data of the core articles as well as a check of the indices and references of the books. Four books fulfilled these criteria:

- Lifelines, 1997, by Steven Rose
- A New Science of Life, 2009 [1981], by Rupert Sheldrake
- The Presence of the Past: Morphic Resonance and the Habits of Nature, 2011 [1988], by Rupert Sheldrake
- The Science Delusion, 2012, by Rupert Sheldrake

"A New Science of Life" and "The Presence of the Past" were originally published much earlier than the controversy that is the case in this thesis, with A New Science of Life also sparking the controversy about Sheldrake in general, and The Presence of the Past in part starting the controversy between Sheldrake and Rose. Since these books were written too early to incorporate any details regarding the events happening between Sheldrake and Rose, the updated versions were the ones with the most analytical value.

These pop-science books fulfil the criteria set out to be considered an arena. Both actors have a means of engaging in this space, since they have published many pop-science books, and the general public has access to it, after the barrier of paying for them. As already shown in the state of the art, pop-science books have become more and more interesting to researchers interested in science communication. A specificity of the pop-science books analysed here is that they tie in with a historical movement of science communication that is particular to the UK. Cassidy (2006) analysed the tradition of popular writing in the UK. This tradition begins with Darwin's Origin of Species, which was aimed at an audience of learned men and the broader educated public. While in the twentieth-century scientific communication became more embedded in institutions, communication happened mainly through journals. The only exception were senior members of the scientific community who sometimes published books aimed at the wider public. In the 1970s this tradition was enlivened again by E.O. Wilson and Richard Dawkins who both published books on sociobiology. These books were scientific but aimed at the broader public and led to a rise in published popular science books in the UK. This led also to "an increased popularity of events where scientists give talks or lectures, or debate with one another in front of public audiences." (Cassidy, 2006, p.182). The audience envisioned by the people communicating in the Cassidy case was an "elite, professional one" (Cassidy, 2006, p.198), which can reasonably be also argued for this case. The authors of popular books described that they could also write in ways in the public domain, which they could not in the domains of academic journals.

The popular science books written by both authors fall temporally in this strong trend of increased public science communication. The audience for these books was becoming increasingly bigger. This means that this arena is also characterised by attributes missing in the others. The form in which pop-science books are written is drastically different from the columns and peer-reviewed journal articles. Lacking the space constraint of a newspaper column and the rather fixed form of journal articles, the actors can take up space and use it rather freely within the pop-science books. This opens up many possibilities to the actors, which the other arenas did not. Another facet of pop-science books that makes them different from other arenas is the involvement of the author in it:

> Thus, for the purposes of understanding the public construction of science, the author of a popular science book is significant as a textual presence, not as a living and breathing human being interacting with other living, breathing human beings. In other words, in so far as we discuss authors, we need to look at the 'implied author' of a book rather than the actual author. One implication of this is that authors who are not

> practising research scientists can be constructed as ‘scientists’ within their texts or in other associated texts. (Mellor, 2003, p.519)

These differences are important parts of the analysis contributing to what is possible in which arena.

6.2.2.4 The non-arena and document collection

There are additional documents that are of importance to the analysis, but which are not produced in an arena. These documents are the citations of the core articles. The three core articles were picked up by the broader scientific community. The term scientific is here a matter of degree because many of the articles which quote the core articles are published in journals that lie on the margins of science or would be classified as pseudoscience. They are nevertheless important since the uptake of the artefacts of the controversy is producing analytical findings as well. These documents are not part of an arena since the actors of the controversy do not actively partake in it. Nevertheless, it is accessible to parts of the public. The uptake of the controversy in this non-arena will not play a big part in the analysis, rather it will inform supplementary findings which try to see what was left behind after our actors.

These documents were found through a search through four databases: Google Scholar, Web of Science, Scopus and PubMed. The search terms were the names of the core articles. When they were found in the database, the citation data was analysed. The detailed information on the findings and all resources can be found in Appendix A.

6.3 METHODS

The methods used are strongly influenced by Adele Clarke’s situational analysis (2005), as well as some smaller contributions by other authors concerning the ‘how’ of document analysis.

Situational analysis sees the situation as the main point of analysis. The goal is to understand the elements and relations in the situation: “the goal is to understand, make known, and represent the heterogeneity of positions taken in the situation under study and/or within given (historical and/or visual and/or narrative) discourses in the situation” (Clarke, 2005, p. 25). Situational analysis builds theoretically on grounded theory, which sees the data as the starting point for the analysis, not any questions beforehand. From the data, through work, the theories emerge. This work is the close reading and coding of the data at hand with different methods. Situational analysis mostly works with maps: 1) situational maps, which lay out the elements involved and entangled with a focus on relations between elements. 2) social worlds/arena

maps, which lay out collective actors, involving organizational, institutional and discursive dimensions. 3) positional maps, which show the range of discursive positions taken or not.

With grounded theory, no 'reading' of data is the same, the researchers working on them have their own personal experiences and interests that they hold, which influence the outcome. But as already stated in the material section, these personal experiences do not per se harm the process, they are inevitable. Nevertheless and furthermore, because researching also always means "intervening" (Clarke, 2005, p.14), the researcher has to be highly reflexive of their personal standing in/towards the situation analysed. This includes questions of why it was chosen, who benefits from it and who are potentially harmed.

What Clarke sees happening in the arenas that are important for the understanding of the situation is the following:

> Participants in an arena can be conceived as social worlds- groups with shared commitments to certain activities, building shared ideologies about what should happen in their arena of concern, and gathering resources to achieve their goal. Thus social worlds are collective actors in an arena; they typically have diverse moral and political commitments; and they promote conflicting agendas for action. Controversy derives from these worlds having different perspectives on what should happen, when and how. Controversies occur where different social worlds meet-in arenas. (Clarke, 1990b, p.19)

The actors in these arenas produce discourse, which in turn can be analysed to get a better view of the situation in the arena. These social worlds exist multilayered and mosaic, one actor can be a member of more than one social world. These social worlds organize social life, and each of the worlds has subworlds, segments concerned with specific commitments. One can understand a social world if one can understand all the arenas it acts in and all the discourse It produces.

In these arenas, actors might also be implicated. This means that they may or may not be physically there in the arena, but they have been silenced by the other actors, even though they are discoursively present. An understanding of these actors gives an understanding of the power relations at hand in the arena.

As already stated, situational analysis mostly works with the tools of maps, to make a situation more easily workable with. In the process of analysis, not all three maps have to be used, since every situation might call for more detail on one map than the other. In this project, mostly the situational map has been used. This map tries to capture, who and what is in the situation, what matters and who and what makes a difference to the situation. All these things are defined

by the actors as well as the analyst. Here is the key point why the situatedness of the analyst is not always a drawback. Through their own situatedness, the analyst can spot which actors or discourses were left out. Only through a body of knowledge and experience is it possible to get a grip with what other actors or discourses could have been included but were omitted by those in the situation. The defined actors are then connected on the map with specifications to their relations.

To work with the situational map, the documents were printed out and with a highlighter, everything was marked that seemed important in the situation or to the actors, defined by them. This included discourses, objects, persons, concepts and everything else that seemed to be of importance in argumentations or the general text. This resulted in an unordered, rather long string of words. These words were then clustered into groups of words that were similar. The clustering was not done after strict rules but rather following a rough feeling of what fits where. From these clusters, the first topics of interest in the analysis emerged. The topics that were chosen informed the analysis as to where more focus should be laid. While the clustered list was used to inform which topics will be followed, some parts of the list were used for maps that are shown later on in the empirical section. These maps include a graphical representation of all the important human and non-human actors in the experimental setting, to give a more detailed insight into what was happening during the experiment to the reader. The other maps are maps that combine the discourses that were drawn upon by both Rose and Sheldrake in the documents. These maps hold analytical value and serve as graphical representations. They will also be discussed in more detail in the empirical section.

The analytical work of the social worlds map was also done, but more to inform the analysis than to draw a map. For this map the important questions are: which social worlds operate in an arena, why have they chosen to do that, what are their perspectives, what do they hope to gain, what resources are there and what do they contribute? Since this case revolves solely around two individuals, the social worlds that are present are small, and therefore the benefit of a graphical representation was minimal. Nevertheless, all the questions above are valid questions that informed the analytical process and gave clear results that will be presented later on.

The last map, the positional map, was also only done to inform analysis, not for a graphical representation. Here the goal is the laying out all the major discourses present in a debate and to analyse the positions taken. This focus on discourse and positions towards an issue was part of the analysis.

Especially the last map focuses heavily on discourses. Discourse means: "[…] communication of any kind around/about/on a particular socially or culturally recognizable theme—contemporary and/or historical. […] discourse frames debates, influences perceptions, and creates objects of knowledge." (Clarke, 2005, p. 148). Discourses therefore shape shared realities. An analysis of discourses focuses on the negotiation of them in social relationships, how they produce identities and the way they produce power and control. These discourses can take many forms, from the visual to the textual to the material. The main focus of this research is on the textual, on the documents already introduced. I see the relevant documents as "[…] act[ing] as nodal points in an intertextual web of mediations of science." (Mellor, 2003, p. 510). These documents never stand alone, they always draw on other documents, incorporate groups of discourses, while excluding others, and pave the way for other documents. These webs were an important part of the analysis, they reveal which cultural resources (as in the maps already drawn in a boundary work sense) were used to construct the situation at hand.

The complementary methods used were more related to gathering general information about the documents themselves. While situational analysis was concerned with the situation and its actors, these methods were used to grasp the importance of the documents per se. Here parts of the analytical frameworks of Prior (2007) and Silverman (2014) were used, to analyze the way the articles were produced, which audience they had in mind and for what purpose they were produced. Here attention was brought also to the style in which they are written (for example conforming to the 'standard' way of writing articles or diverting from it) and the choice of journal. This led to an analysis of the implications of the texts as published articles.

7 EMPIRICAL CHAPTER

In this empirical chapter I want to give an overview of the controversy as it unfolded and the themes that rose from it. I will start with a timeline, to show what happened when, and to give a more structured overview of the temporal aspects of the controversy. Then I will focus on the different arenas, The Guardian, The Core Articles, The Pop-Science Books and The Aftermath, to show in more detail what happened in the controversy. While looking at the arenas I will show what questions, topics and issues arose from the controversy, how the actors dealt with them and how their practices shaped the further trajectory of the controversy.

I will now enter the different arenas where the controversy between Sheldrake and Rose took place. I will look at the way both actors handled themselves in these arenas and what exactly they did do.

7.1 THE FIRST ARENA: "THE GUARDIAN"

The Guardian was mostly used as a resource to get an overview of how the joint experiment started.

7.1.1 Column Number 1: Sheldrake

While there is some written material on the start of the controversy between Sheldrake and Rose in 1981, it is dated later on and written in hindsight. The first written material about the controversy stemming from the actors is dated 1988 when Sheldrake wrote an article called "Resonance of memory" in the category "Body and soul" in The Guardian (Sheldrake, 1988a). Sheldrake starts this article with a paragraph about the belief held by many that memories are stored inside one's brain. He goes on to say that most people forget that this is only a theory and that after decades of research the details remain mysterious. He suggests that this theory might be mistaken and proposes another: that memory is stored outside of the brain, in what he calls 'morphic fields' which influences the brain by 'morphic resonance'. He describes his theory of memory as being inherent in all of nature, a kind of collective memory, transmitted across time and space. He points out that this theory breaks with the current status quo of the scientific discourse. He nevertheless points out that "it is possible that the conventional mechanistic conception of nature is itself mistaken." (Sheldrake, 1988a, p. 21). Sheldrake goes on in the next paragraphs to describe his theories more in detail, also advertising the book he had freshly written at that time, "The Presence of the Past" (Sheldrake, 1988a).

Sheldrake describes in some detail the experiments neuroscientists have done searching for memory traces in the brain of animals. He claims that though quite a lot of research has been

done, the researchers have found no traces of memories. They have in his view then changed their theories to fit better with these facts: that memories might be stored in a distributed manner, or that they might be stored somewhere as a backup. He describes how animals have still been able to carry out actions that they have learned after the regions where their memories should be stored were destroyed. Sheldrake claims that his theory could explain these phenomena. He closes his article with the sentences: “The idea that memories depend on morphic resonance is only a theory. But so is the idea that they are stored as material traces inside the brain. At present the question is open.” (Sheldrake, 1988a, p.21)

While this was not necessarily intended, this has become the opening move of the controversy. What Sheldrake does here is questioning a specific status quo of scientific research, showing the gaps and supplying his hypothesis as a possible substitute. For his hypothesis, which had already been harshly criticized in *Nature,* to hold specific merit, it has to answer questions that exist in current research better. Therefore, he needs to show what these problems are and then he needs to deliver a solution. Sheldrake does that by claiming that researchers have found no material traces of memory in brains, which creates a problem for their theories and could be solved by accepting his theory. He hopes to show that his theory is equally plausible as the others, especially in his last sentences. He accepts that his hypothesis is a theory, but so is the common ground used in neuroscience, which brings them on equal footing. This argument is the first of many that will go in the same direction, used by Sheldrake.

What should not be forgotten is that the audience of Sheldrake's arguments is the readers of The Guardian. The vast majority of them are not neuroscientists. It still might be that a certain percentage has a background in biology or other related fields who might know something about the research Sheldrake points at. For the other readers, they have to take Sheldrake’s argument at a face level. He gets certain credibility, solely from the fact that he can write in The Guardian. It is a newspaper of some standing, so the reasoning would be that not everyone can just write an article about everything. There is a certain implied standard that is derived from writing in a newspaper. Since there is not much space in the article, and it seems to come from a somewhat credible source, Sheldrake’s arguments need not go so much into depth as they would somewhere else. The circumstances of the published piece of writing give it a certain degree of credibility. Even though it is in effect also an advertisement. While Sheldrake critiques, or at least questions the common ground of neuroscience, he points to his hypothesis and the book he has written about it. While this seems not uncommon in the type of article Sheldrake is writing, it should not be forgotten that one purpose of this article is certainly to advertise his new book.

7.1.2 Column Number 2: Rose

The second article is written one week later on 13.4.1988 by Steven Rose. It is titled “Some facts that just don’t resonate” (Rose, 1988a). Rose starts his article with a reference to Nature and the mentioning of book burning. Rose then states that “anything the Editor of Nature wants to burn can’t be all bad” (Rose, 1988a, p.27) and that he extended an invitation to Sheldrake to test the hypothesis together, which Sheldrake declined (this would have been some years ago). Rose now writes about Sheldrake because he put forward another book detailing more of his hypothesis. Rose takes stance on Sheldrake's article as well as the hypothesis in general. Rose points out what a scientific hypothesis has to do, to be called as such: rooted in fact, explaining data in a new way and providing “new scope for prediction, experiment and test.” (Rose, 1988a, p. 27). Rose states that Sheldrake's hypothesis fails on all counts and that Sheldrake invents the puzzles he wants his hypothesis to solve. After that Rose goes on to state the evidence Sheldrake ignores or denies, which run counter to his hypothesis. Rose puts forward what is seen as the status quo in molecular biology regarding the brain to show what strands of thought are already existing that could explain away the problems Sheldrake quotes, without a need for a new hypothesis.

The last paragraph is dedicated to a crisis that Sheldrake mentioned in his column that he sees in modern science. While Sheldrake sees a crisis in science in the way neuroscience cannot explain specific phenomena and a radical paradigm shift would be needed to further understanding, Rose sees a different crisis emerging. He is critical of the tendency of science at that time “towards an arrogant reductionism” (Rose, 1988a, p. 27). But more so, Rose sees a crisis in the standing and believability of science:

> But Sheldrake, and the space given to his views by the media, is symptomatic of science’s crisis, not part of its solution. In recent months for example, the Guardian has offered us everything from the hard-nosed biological determinism of genes for incest and alcoholism to the wilder reaches of Kirlian photography and – on the same page as Sheldrake’s piece appeared last week - people who can make lightbulbs glow by merely holding them. Between these extremes, is it any wonder that those of us anxious for an informed public debate on the uses, meanings and control of science sometimes come close to despair? (Rose, 1988a, p. 27)

What we can see here is that Rose sees Sheldrake as the epitome of a crisis in the credibility of science. Not only is Sheldrake’s hypothesis not nestled in fact and reliably quoted sources, but it is also given space by The Guardian. This might be another reason why Rose objects so strongly to Sheldrake’s articles. While Sheldrake is not up to the standard Rose has for

scientific conduct, even worse is that Sheldrake gets the space to spread his views. This is seen as running counter to an *informed* debate. Sheldrake's views mislead the readers of the newspaper and undermine the credibility and standing of science.

7.1.3 Column Number 3: Sheldrake

The third article is once again written by Sheldrake, a week after the article by Rose. Most of the space in the article is dedicated to Sheldrake's arguments on what Rose is not getting right. Sheldrake goes here into detail, as well as Rose before him, about the biological sources, their experimental designs and outcomes. The details of these arguments are not important for the analysis here, apart from the fact that Rose and Sheldrake see different parts of the evidence supporting their views.

The last paragraph is analytically more interesting. Here Sheldrake raises the question of the personal stance and commitment to hypotheses and/or paradigms:

> But however elusive the hypothetical traces prove to be, Rose can always fall back on the well-worn argument that there must be "redundant" or "back-up" traces somewhere else in the brain. He does not regard memory as a mystery because he presupposes that memories *must* be stored as traces in the brain, even though no-one knows how. The difference between us is that I regard this as a questionable hypothesis; he regards it as an unquestionable truth. (emphasis in original, Sheldrake, 1988b, p. 23)

Sheldrake blames Rose for not taking a step back to view his conception and holding tight to a paradigm that is in Sheldrake's view outdated. This rigidity is exactly the problem Sheldrake sees with science at that time, science's rigidity to move on to new views. Rose is for Sheldrake an embodiment of this spirit.

7.1.4 Column Number 4: Rose

The fourth and last article is once again written by Rose a week later. The articles grew steadily shorter and this one is a third in length than the first two. As the weeks pass, the space in the articles was also given to others writing about it. These responses are for the controversy at hand not important and are left out. The space Rose has is used mostly up by him going once again into details about biological publications, once again correcting the things Sheldrake presented wrong. He ends the article with a paragraph stating:

> There just is no way that this straightforward and impressive body of evidence can be taken to imply that memories are not in the brain, still less that the brain is tuning in to

> some indeterminate, undefined, resonating and extracorporeal field. (Rose, 1988b, p.23)

This paragraph once again states very clearly Rose's view regarding Sheldrake's hypothesis. There is no way that the hypothesis is scientifically sound. Even though Rose holds this view, we know that an agreement had been found to test the hypothesis. The controversy leaves this arena, to start again in another. Before we look at the second arena, I want to recapitulate in the next short chapter the moves made in this arena.

7.1.5 Moves Made in the First Arena

I want to look in this chapter again at the "moves made" in the arena. I see this controversy like a struggle, a grappling match between two contestants both working on swiping their opponent off his feet. Because of the different backgrounds, standings in the scientific community and goals of both actors, different moves are more profitable for each of them. I want to highlight here the moves made by both actors, either knowingly or unknowingly, in the controversy.

In this first arena, we have seen already the first steps both actors take in the controversy. Sheldrake had the "opening move". Through his publicity in The Guardian Sheldrake had the opportunity to advertise his book as well as his hypothesis. The important thing here was the credibility of his work. Sheldrake wanted to establish his theories in public and therefore had to convince them of the use of his ideas. Here Sheldrake follows a kind of Kuhnian ideal of the way science goes through revolutions (Kuhn, 1962) and Sheldrake sees this as a way for science to move forward: he shows what kind of questions remain unanswered by the current way of thinking in biology and presents another way of thinking which could answer these unanswered questions as well as fit with the rest of the already answered questions. He has to keep ties with what has already been established before, otherwise, his ideas cannot find a footing. Following Collins (1985) each new idea is embedded in a 'web of concepts' to what has come before in science. For an idea to be acceptable it has to keep ties to the already established and major parts of the discipline it emerges in. Sheldrake's strategy was to show that his idea could keep the ties to the already established, but also transcend it and add to it. Sheldrake keeps his explanations of his theory simple, way simpler than they are in his books. The Guardian is for him a space to get public attention to his work and how his ideas could keep the ties as well as succeed the general view in a Kuhnian way. At the point in time where he writes the article, he is already being seen very critically in the scientific community. The opinion of him in other social spheres might still be malleable. The editorial in *Nature* might not have reached broad parts of the public. Following Collins (1985) once again, the robustness

of new knowledge is tied to the actors who support it, who use it and give it legitimation. This is not only done by scientists but also by actors outside of the social sphere of science. Sheldrake's act of writing in The Guardian, advertising his books and his ideas can be seen as a move to establish himself and his views more firmly in the public discourse and gain legitimacy.

This move was a move that Rose decided to act on. He saw the writing of Sheldrake as a danger of legitimating ideas that should not be part of science. Rose is familiar with the work of Sheldrake and sees it as propagating pseudo-scientific ideas which undermine the standing of science. Especially the publicity that a platform like The Guardian gives to Sheldrake is a thorn in Rose's side. This might be the reason why Rose extended his offer once again to test Sheldrake's hypothesis. Rose wants to move the discussion away from The Guardian, into the realms of scientific deliberation tied to experimentation. Here shines through what will, later on, become more apparent, which is Rose's stance towards the scientific process. Rose believes that only tried and tested knowledge should be propagated. Only knowledge that holds up to science high standards has the right to reach the minds of the public not wise in the ways of science. The public discourse should be kept clean from pseudo-scientific works since it dilutes the distilled knowledge circulating hard-earned by science. Rose believes that scientific hypotheses which are disproven are quickly forgotten and abandoned. A logical way to contain and dismantle Sheldrake's knowledge is to put it to the scientific test. A crucial experiment would decide if Sheldrake can, in a way, rightfully go on to publicly claim scientificness to his ideas.

7.2 The Second Arena: The Core Articles

7.2.1 Introduction

After the first arena, after The Guardian, Rose and Sheldrake got together to plan and execute a joint experiment. As we have seen in the timeline and introduction, this experiment quickly turned out to be not what either of the actors hoped it to be. The second arena now comprises the three core articles, all published in a peer-reviewed journal, which deal with the 'failed' joint experiment. Two of these articles (the first and the third in the timeline) are written by Rupert Sheldrake and one by Steven Rose. The three articles together compromise 25 journal pages, containing text as well as some figures. They were all three published in the same Italian biology journal, "Rivista di Biologia-Biology Forum".

7.2.2 Relations and Summary of the Experiment

Before the core articles are analysed, I want to describe the experiment and the role both actors played in it.

The experiment was designed to test the influence of a specific phenomenon on a test organism. In this case, the phenomenon was morphic resonance and the test organism chicks of a specific breed of hen. Sheldrake's hypothesis always revolved around a connection between organisms over time and space without a direct connection (genes, a social group, etc.) involved. This connection was explained through the morphic field, which would connect all organisms of similar kind. In theory, there would exist a morphic field that influences all chickens in the world. This field is not only acting on them but it is also acted upon. Sheldrake's hypothesis states that if an individual of an organism learns something, it is stored in the morphic field and passed on to other individuals of the same organism. When this effect is repeated in more and more individuals the information in the field becomes more fixed and exerts more action on the individuals who have not already learned it. Following this reasoning, if an organism learns something, it takes the next organism of the same kind less time to learn it, because the field helps pass on the information. This meant that the test of morphic resonance could be done as follows: one group of animals would be trained to do a specific action, they would learn something. The time it took the group to learn would be measured. Another group, not related and not in any contact with the first, would then learn the same action, and time would again be measured. If morphic resonance would exist, the second group of animals would take less time to learn to perform the action. If morphic resonance would not exist, the second group of animals would take exactly the same time to learn it.

Rose and Sheldrake planned the experiment together, and both agreed to run an experiment along the lines described above. As already mentioned, chicks were used as the model organism, mostly because they are cheap and easy to handle. To achieve the design described above the following was planned: the chicks were separated into groups. One group were the chicks that would be exposed to a specific stimulus and an injection, while the second group would be just exposed to the stimuli. Both groups were then divided into a control group and a test group. The test group would be injected with a substance that made them sick so that they learned to avoid the stimuli (a LED light on a rod). The control group was injected with a solution that did not make the chicks sick, which would mean that they did not learn to associate sickness with their stimuli (a chrome bead), and no learning effect would take place. Measured was the time it took the chicks to peck at the stimuli. In the actors' view of a perfect experiment, and given the existence of morphic resonance, the second group of chicks, which were not exposed to anything but the stimuli, would have taken longer to peck at the stimuli of the test

group. This would be the result of the morphic field acting on the second group of chicks. They had never been directly exposed to the negative injection but would have learned to associate the LED light with negative feelings. This measurement of time (also called latency) of the second group to peck at the LED-light was the crucial data obtained. This latency to peck was also the subject of the predictions done by Sheldrake and Rose. At the start of the experiment, they both made a prediction of what would happen. Sheldrake said that the chicks would take longer than the first group to peck at the stimuli, Rose said they would not.

The experiment was executed by Rose's assistant Amanda Harrison, working blindly (without knowledge of the hypothesis tested). She handled the chicks, the injections, the stimuli, and everything else. The data generated by the experiment was then statistically analysed by Rose as well as Sheldrake. Here a disagreement arose between the two because the data was inconclusive regarding their predictions. Other influences, such as differences in weather conditions that might have influenced the behaviour of the chicks, the training of Amanda Harrison, and others, were also considered. This inconclusiveness and the resulting disagreement will be covered later in more detail. As we see as well later in detail, Sheldrake's and Rose's views on the statistical analysis are quite different. Professor Bateson, a colleague of Rose is also drawn into the experiment by Rose and is in favour of Rose's statistical analysis.

In Figure 5 beneath, the relations of the experiment are graphically represented. It shows the relations between the important human and non-human actors as well as some of the important discourses as described by Rose and Sheldrake concerning the instance of the experiment. As one can see on the right-hand side, the people-like graphic stands for human actors, the round graphic for non-human actors and the rectangular graphic for discourses. There are different arrows, detailing different kinds of relations between actors. Black filled arrowheads mean an indirect influence (with small text to specify the details), an unfilled arrowhead means a supposed direct influence (defined by the actors) and a dotted line means a conflict/disagreement. 'S. analysis' stands for 'Sheldrake's statistical analysis' and 'R. analysis' stands for 'Rose's statistical analysis'.

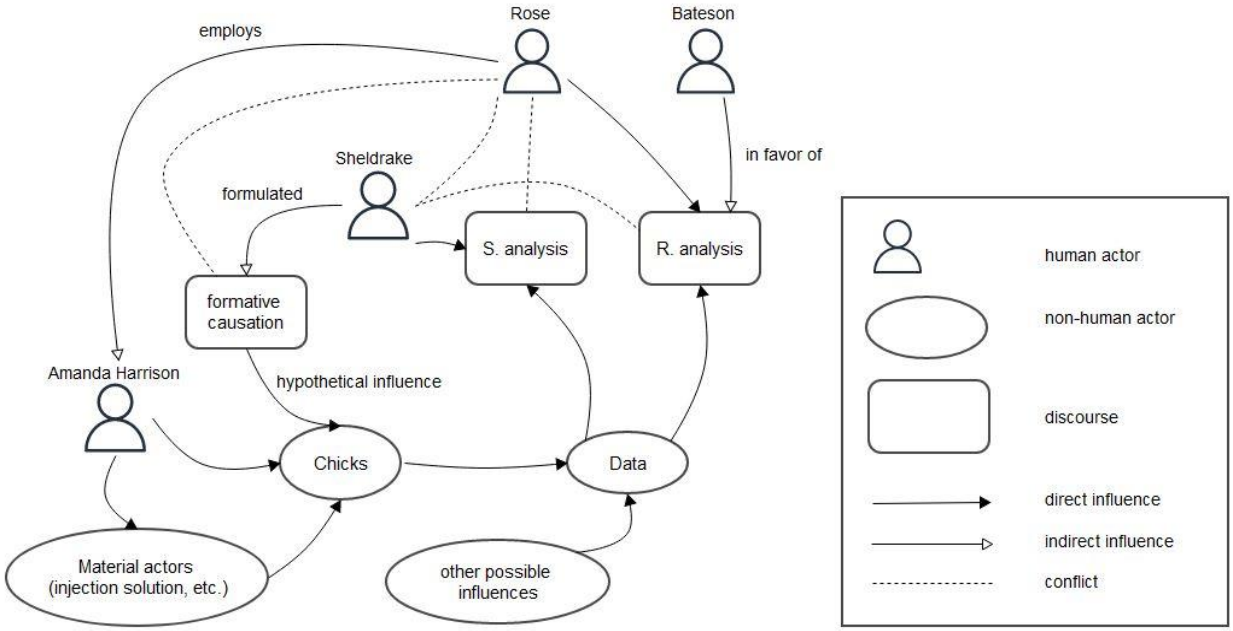

Figure 5: Relationship between human & non-human actors as well as discourses in the experiment

I will now look at the articles and what they contain. This first overview will be held rather short, whilst the important details, arguments and discourses will be later on taken up more thoroughly.

7.2.3 Article 1: Sheldrake

The first article is called “An Experimental Test of the Hypothesis of Formative Causation” and was written by Rupert Sheldrake in 1992. It compromises 12 pages and was published in the Italian journal *Rivista di Biologia- Biology Forum.* The article is structured following the general structure of published articles concerning biology with an abstract, an introduction, a chapter called experimental design, followed by materials and methods, a results section and at the end a discussion and bibliography. The abstract is used to describe the hypothesis put forward by Sheldrake, and then to describe in a few sentences the experiment that was jointly planned. It also includes a few lines on possible interpretations of the outcome. In the introduction, Sheldrake talks about his hypothesis in more detail. He explains how it works and addresses that there is already evidence for it and that he discusses questions in more detail in his books. He then also gives a short overview of his connection to Rose and how this experiment came to be. In the section on experimental design, Sheldrake gives an overview of the connection between his hypothesis and the experiment. He describes how they relate to each other and touches upon Rose’s work and its connection to the experiment. This section also includes the

predictions of both Sheldrake and Rose concerning the outcome of the experiment, which were written down before the experiment was started. In Material and Methods, Sheldrake talks more in detail about the different parts that make up the experiment. He talks about the chicks used as a test model, the beads used to evoke responses from the chicks, who handled the chicks, the time involved in the experiment, the chemicals used, and the statistical procedures which would be used. The results section is devoted to the outcome of the experiment, supported by six figures showing graphs of different kinds. Here Sheldrake talks about the effect of growing practice with the chicks by the experimenter (Amanda Harrison) and most importantly, about changing latencies (the time it took the chicks to peck at beads, part of the experimental design). Since the test of his hypothesis would, after this experimental design, rely heavily on changes in latency, they are described in detail. Both predictions made by Sheldrake and Rose rest on the latency of the chicks to peck at a stimulus. These latencies are shown in graphical format, discussed in the text, and described also with the use of statistical parameters. The discussion section is used to recapitulate the predictions done in the beginning, and to compare the results to the predictions. Sheldrake also addresses the problems with the results (them being inconclusive) but ends on the note that they could be in favour of his hypothesis, but that more research is needed. (A1)

7.2.4 Article 2: Rose

The second article is a response called “So-called “formative causation” – a hypothesis disconfirmed” and was written by Stephen Rose in response to the first article. It compromises 8 pages and was published in the same journal, in the same year and the same edition of the journal. This paper has not an overall visible structure. It is classified as a response and does therefore probably not adhere to any structural norms regarding biological publications. It includes four figures and one table in its 8 pages. The first three paragraphs are dedicated to commenting on Sheldrake’s hypothesis in general. Rose discusses his personal experience with reading Sheldrake's books and then goes on to draw on Kuhn and Popper (even though not explicitly mentioned) to argue that Sheldrake's hypothesis is ‘empty’:

> His book *A New Science of Life* seemed when I first read it, and still seems, to propose an entirely empty hypothesis. The circumstances in which novel hypotheses (paradigms) become important in science have been well described by Thomas Kuhn; they emerge when there is an accumulation of observational anomalies which existing hypotheses cannot account for, or when a theory becomes excessively cumbersome and "inelegant" and the alternative seems to handle the same material more coherently. To Kuhn's account we can, at least in the particular context of the present discussion,

> add the well-worn view that to have utility, a hypothesis should be capable of disconfirmation. (A2, pnna)

He then describes in more detail the logical shortfalls of the hypothesis and speaks about why this hypothesis is still in circulation. Here he gives the reason that Sheldrake is relentless and that the times they live in are anti-rational and support such hypotheses. The next paragraph is dedicated to Rose's view of the personal connection between him and Sheldrake and why Rose agreed to the joint experiment:

> So why did I offer to collaborate on the experiment Sheldrake describes? My original offer was made, as he points out, more than a decade ago, when his book first came out. As I am hostile to the fascist implications of book-burning, even when the suggestion is made in joke, I responded to the *Nature* Editorial to which Sheldrake refers by suggesting that he himself run some experiments in my laboratory. (A2, pnna)

The main part of the document is dedicated to Rose's description of his work, his laboratory and then, in turn, the experiment that was planned by Rose and Sheldrake. Here he gives his view on what the results mean, underlined by figures and statistical parameters. Rose also picks up the arguments Sheldrake put forward in the first article and describes why they are false interpretations of the data. In the last paragraphs of the document, Rose mentions that he sent the analysis of the results to Professor Bateson, a pioneer in the field regarding studies with chicks. Bateson looked at the analysis of the results by Sheldrake and found it misleading. Rose is quoting a letter from Bateson, giving space to Bateson's voice. Through this Rose then concludes the document in a way together with Bateson by having Bateson's voice also saying that the results are counter to the prediction of Sheldrake and that he obscures this fact. (A2)

7.2.5 Article 3: Sheldrake

The third article is called "Rose refuted" and was written by Rupert Sheldrake. It compromises 5 pages and was also published in the same edition of the same journal. It has no structuring headlines, is also written like a response and includes no figures. The first paragraph is a reaffirmation that Rose's predictions were refuted and adds more detail to the personal history of both researchers during the time of the experiment:

> Rose and I discussed various interpretations of the data over a period of eighteen months. At the outset, he seemed certain that the hypothesis of formative causation would be disconfirmed. He had already publicly denounced it in the strongest terms. He appeared to have no doubt that when tested in his own laboratory, under his own supervision, in my absence, by an experimenter working blind, the data would reveal

> no trace whatever of morphic resonance. But it soon became clear that there had been an effect of the kind predicted by the hypothesis of formative causation. […] After lengthy delays, Rose withdrew from our agreement to write a joint paper, and no longer wanted to publish the results. (A3, pnna)

The next paragraph is dedicated to what both researchers have in common concerning their interpretation of the experiment. The paragraphs after that are dealing with Rose and Bateson's statistical ways of analysing the results. Sheldrake reviews the statistical approach of Rose and Bateson and argues against it. He then proceeds to take a position to all the points put forward by Rose as to why Sheldrake's interpretation is wrong. Sheldrake gives his reasoning behind some points and tries to show how Rose is wrong in his interpretation of the data. The next paragraphs are used by Sheldrake to review the standpoint of Bateson and Rose and to argue against them by using his interpretation of the results. In the last paragraph of the document Sheldrake critiques Rose's style of writing, his use of evidence and his rhetoric. (A3)

7.2.6 The Data and its Revelation

It seems that both actors, Rose and Sheldrake, came into the joint experiment with the expectation of clear-cut results. They both noted their predictions, as a way to ensure that nothing would get twisted later on. Even in the predictions, it becomes clear that there was the held assumption that the data would speak for itself and in a way reveal who was right. Both often use words such as "apparent trends", "foresee", "see clearly", "reveal", "Obscure". It appears both went into this experiment with the idea that the results would show them clearly if either Rose or Sheldrake would be right since both made contrary predictions. Since both were surprised by the results, other ways of concluding had to be found. This is the point where for the first time the experimenter's regress opens up. The assumption that their experiment would yield a satisfactory and clear result was shattered. Following Collins' (1985) definition of the propositions of the experimenter's regress:

> *Proposition Five:* Proper working of the apparatus, parts of the apparatus *and the experimenter* are defined by their ability to take part in producing the proper experimental outcome. Other indicators cannot be found.
>
> *Proposition Six:* Scientists and others tend to believe in the responsiveness of nature to manipulations directed by sets of algorithm-like instructions. This gives the impression that carrying out experiments is, literally, a formality. This belief, though it may occasionally be suspended at times of difficulty, re-crystallizes catastrophically upon the successful completion of an experiment.

> *Proposition Seven:* When the normal criterion - successful outcome – is not available, scientists disagree about which experiments are competently done.
>
> *Proposition Eight:* Where there is disagreement about what counts as a competently performed experiment, the ensuing debate is coextensive with the debate about what the proper outcome of the experiment is. The closure of debate about the meaning of competence is the 'discovery' or 'nondiscovery' of a new phenomenon. (Collins, 1985, p. 129)

Following these propositions, the whole existence or non-existence of morphic resonance lies in the outcome of the experiment. Proposition five and six mean in this case that Sheldrake and Rose believe that it is possible to devise an experiment that can test the existence of a phenomenon. The main difference to the experiments described by Collins is that there are not many competing experiments done by different researchers in opposing camps, but one experiment done by both opposing actors together. This would make it seem as though there would be no room for discussion if the experiment was competently done (proposition seven) because both actors did the same experiment in the exact same way. There are nevertheless spaces that can be opened up to make room for negotiating the competency of the actors (proposition eight), and therefore their views on the existence of the phenomenon. More on how these spaces were opened up in the next chapter.

7.2.7 The Use and Role of Statistics in the Three Core Articles

What can be seen in the three articles are high amounts of space and elaboration on statistics. Statistics play a key role in the way both actors make sense of the experiment and its expected results. As we have seen above, data does not reveal itself to the experimenter unprompted. To look at this assumption and to look at the role statistics play in experimental design and knowledge production, I want to first look at how statistics are generally used in experimental design.

The experiment that Rose and Sheldrake planned was not ground-breaking in its experimental design. As already described above, they used chicks as their experimental model and wanted to look at the influence of a specific object on them (in this case the immaterial morphic field). Measured was the time it took the chicks to peck at the stimuli. In the actors' view of a perfect experiment, and given the existence of morphic resonance, the second group of chicks would have taken longer to peck at the stimuli. At the start of the experiment, they both made a prediction of what would happen. Sheldrake said that the chicks would take longer to peck at the stimuli, Rose said they would not. This is in line with their views about the existence of the phenomenon. The data in a perfect experiment would now have clearly shown who was right,

in the case of Sheldrake, it would be a significant rise in the time it took the second group of chicks to pick at the stimuli, while for Rose it would have to stay at the same level. The problem is that data generated in experiments seldomly fall into clear-cut, easily visible and accessible categories. Only very rarely can one look at numbers and see exactly ‘what is going on’. This is where in nearly all experiments statistics come into play.

The experiment and its data fall into a specific category of statistics, interferential statistics. Interference statistics is a branch of statistics where the goal is to look at a sample out of the population and from this sample make statements about the population (Lewis-Beck, 1995). This means that one looks at a sample (in our case a specific number of chicks) and by using statistics makes statements about the population (in our case all chicks in the world). This basic definition and understanding can now show more about the role of statistics in the experiment. The *right* use of statistics is important because it is the only way to say something about the existence of the phenomenon. Statistics is the way to look at what is happening in the population of chicks and carrying this thought further along, in the population of all biological systems. The right use of statistics is so important to the actors because on the right use of statistics the existence of the phenomenon rests. Statistics is for them a way to sidestep the experimenter's regress (although presumably not knowingly). They both agreed on an experimental design, which means that they cannot use it to close the regress in their favour since they both judged it credible. To close the regress, they must take the step into the right and credible use of statistics to close the regress.

This step takes many forms, it is played out in arguments about which data to use, which data could be excluded because of which reasons, which statistical analysis method is best, which manipulation of data is allowed, etc. The technical arguments are not as interesting as the fact that this was argued about as well as the amount of space given to it. Nevertheless, to give a short overview of what exactly was given so much space: the experiment is designed in a way that two batches of chicks are analysed, both divided into a test and a control group. As already described, the control group of the first batch pecked at a chrome bead and was then injected with something that did not cause any reaction, the test group pecked at a LED light and was then injected with something that made them sick. The second batch of chicks was then divided again into a control group and a test group, and they were subjected again to the chrome bead and the LED light. The main focus was now on the time it took the second test group to peck at the light. This number, the latency, was the key statistical factor that both actors were interested in. To see the changes in this number, the raw data (the hundred or so data points from the chicks) would have to be analysed through statistical measures. The choice of analysis is dependent on different factors, all trying to exclude data points that are seen as

being untrustworthy (due to errors in time measurement, random outliers, etc.). How and what is excluded quickly became the main point of argumentation between Sheldrake and Rose.

Sheldrake talks about contradictions concerning the "trend in the *differences* between the latencies" (A3, pnna), of wrong uses of the words "floor" and "ceiling" effects, of omitted data in the analyses and using a different statistical method than before. Rose talks about Sheldrake's failure to refer to a significant feature of the data, increases and decreases in latency with regression slopes running counter to Sheldrake's prediction, and Sheldrake ignoring the first days of the data. While the experimental design of the experiment was something very fixed from the start, it seems that Sheldrake and Rose did not fix in detail all the ways in which they would analyse the data, thus opening a space of possible movement to side-step the experimenters regress. As we have seen above, the experimental design left no place for questions of competency, since both competing actors were involved in its design. The statistical analysis nevertheless is something that could be used to argue about the competency of the other actor.

An interesting role in this is played by Professor P.P.G. Bateson. Since the statistical analysis was something that both actors did differently, but also wanted to reach a conclusion, they included Professor Bateson in the discussion about statistical analysis.

> "Although Sheldrake and I disagreed on our interpretation of the data, we did agree that he should also send his analysis to Professor P.P.G. Bateson at Cambridge. Bateson is an experienced ethologist and pioneer of imprinting studies in the chick […]" (A2, pnna)

Even though both actors take the statistical analysis as a means to open up discussion about the competency of the other, they both wish for something to close the regress, to lead to an ultimate decision. Bateson agrees with Rose's statistical analysis and disagrees with Sheldrake's. The competency of Bateson now weighs in on Rose's side. As Rose has already described it, Bateson is a prestigious figure in the realm of biology the experiment operates in. As can be seen in the description of the articles above, Sheldrake does not accept the combined competency of Rose and Bateson. He now has in a way two opponents, Rose as well as Bateson. He spends space in the third article then to question the competency of Rose as well as of Bateson. Since Bateson is quite an established figure, his competency is harder to discredit. He argues against Bateson along the lines of misunderstanding the grounds of the experiment and not explaining in detail the workings of his proposed analysis. Bateson's statistical analysis is seen as sound and competent, but his understanding of the underlying phenomenon at work and the fit of statistical analysis to that is questioned by Sheldrake.

There are other factors as well as the use of statistical analysis that can influence the competency and credibility of the actors. In the end, the credibility and competency of the actors are what results in the closing of the regress, in favour of whoever might be seen as more credible.

7.2.8 Let's Get Personal

As I have already pointed out in the previous section, credibility is key in the closure of the regress that both opened up. Apart from the discussion about statistical analysis and data, there were other factors involved in the attempts of both actors to close the regress. The core articles are full of attributes and associations surrounding both actors. By this I mean discourses, personal attributes, historical figures or even disciplines that have been used by both actors to make statements about themselves or the other. They were either attached by themselves or attached by their opponent. By attached I understand that the author of the article connects specific associations to himself or his counterpart. This can happen by making statements, claiming properties or just mentioning specific discourses in the verbal vicinity of an actor. These associations can be mapped and ordered to better show their connections and their attachment to the actors. Below in Figure 6 and Figure 7 are maps for both actors, showing the respective associations attached to them, either by themselves or their opponent.

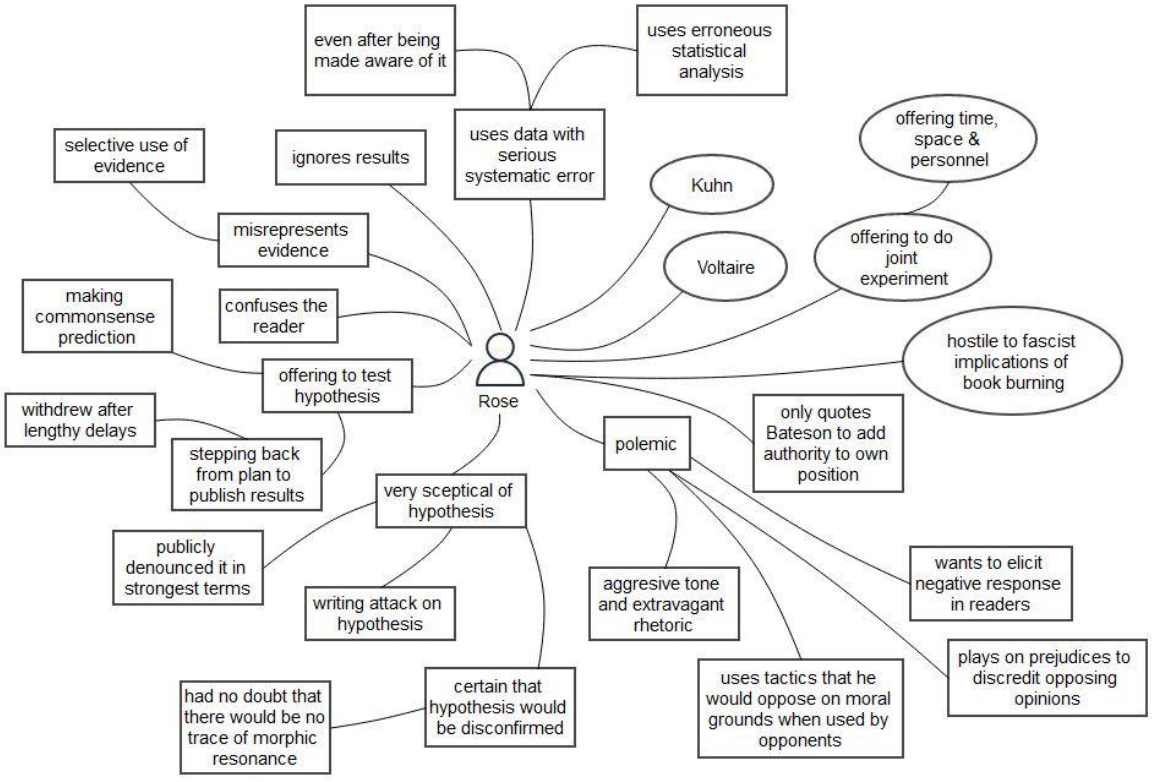

Figure 6: Associations attached to Rose, by himself (round shapes) and by Sheldrake (rectangles)

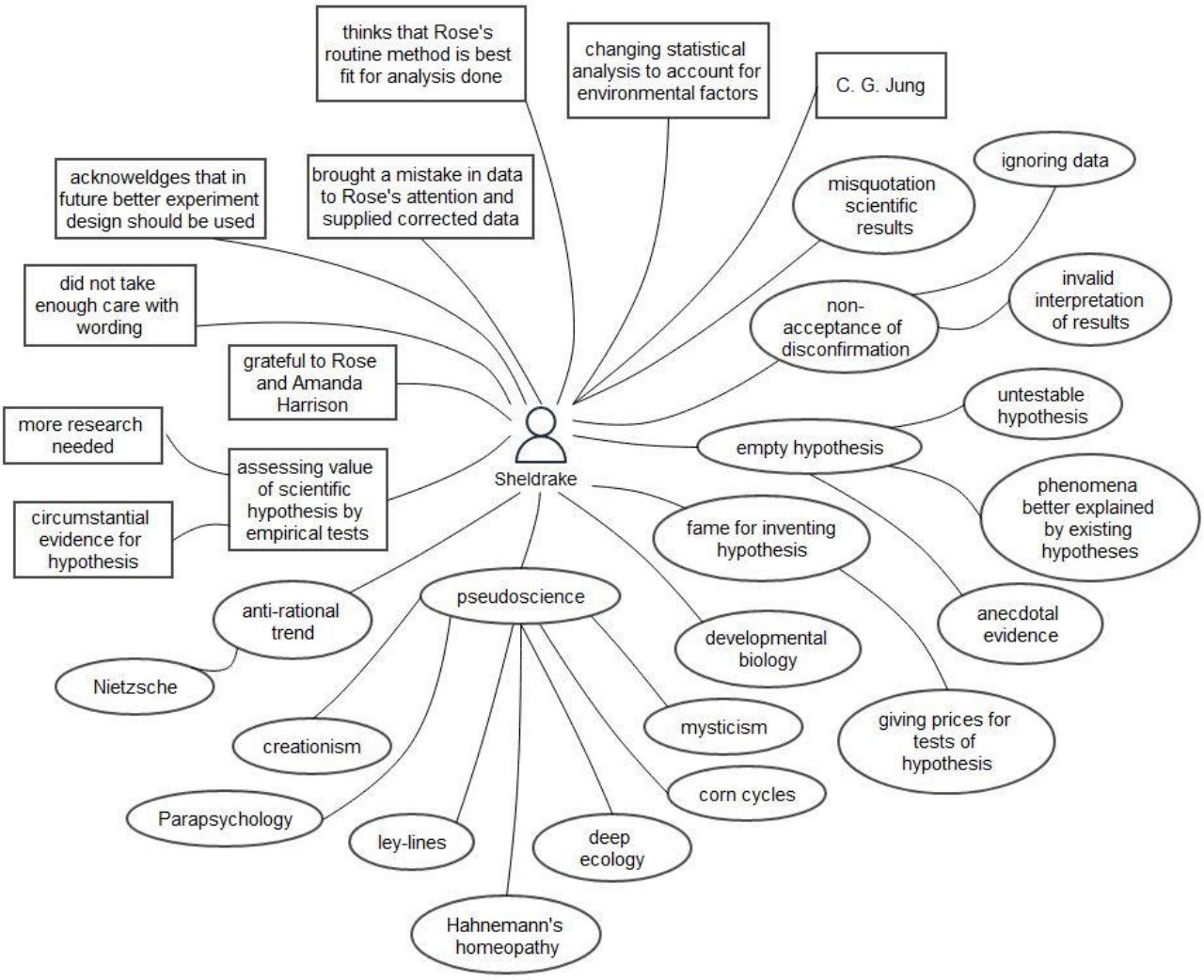

Figure 7: Associations attached to Sheldrake, by himself (rectangles) and by Rose (round shapes)

Looking at these maps shows us a few things. First, it can be useful to see the different proportions of associations that get attached. Both actors attach more associations to their opponent than to themselves. Second, and more importantly than a sheer quantitative approach, is the kind of associations that are attached to the actors. I will first start the analysis with Rose's associations and then move on to Sheldrake's.

7.2.8.1 Rose

Rose has not so much to say about himself, but quite a lot about Sheldrake. The attributes that Rose is attaching to himself can be seen as a kind of willingness to test Sheldrake's hypothesis. Rose talks in the article he wrote about his offers of time, space, and personnel as well as the general offer of testing Sheldrake's hypothesis. Apart from his openness to the testing of hypotheses, Rose points out that he is "hostile to the fascist implications of book-burning, even

when the suggestion is made in joke" (A2, pnna). These sentences convey a general openness regarding new hypotheses, a willingness to help science move forward. He also mentions Kuhn as well, showing knowledge over how science can be said to progress:

> The circumstances in which novel hypotheses (paradigms) become important in science have been well described by Thomas Kuhn; they emerge when there is an accumulation of observational anomalies which existing hypotheses cannot account for, or when a theory becomes excessively cumbersome and "inelegant" and the alternative seems to handle the same material more coherently. (A2, pnna)

While Rose has for himself words of openness towards innovation in science, the words he has for Sheldrake are far less positive. Sheldrake gets associated with a plethora of different discourses. The most grave revolves around how Sheldrake has been behaving quite unscientifically. Here Rose writes about the different ways in which he thinks Sheldrake has been doing things in a way to hinder a positive end of the experiment. Rose talks about misquotations of scientific results and Sheldrake's nonacceptance of disconfirmation by ignoring data and interpreting the results invalidly. He even describes Sheldrake's hypothesis as an empty one, being untestable, being superfluous because the phenomena it tries to explain are already better explained by existing hypotheses, and it being based on anecdotal evidence alone:

> [Sheldrake's] book *A New Science of Life* seemed when I first read it, and still seems, to propose an entirely empty hypothesis. […] To Kuhn's account [of how science changes] we can, at least in the particular context of the present discussion, add the well-worn view that to have utility, a hypothesis should be capable of disconfirmation. (A2, pnna)

Rose here connects empty and untestable, meaning that a hypothesis that is not testable (in the Popperian sense) is empty since it brings nothing to the scientific discourse.

On top of that, he hints that Sheldrake may have formulated his hypothesis in this way because: "For the inventors of such hypotheses the rewards include a degree of instant fame which is harder to achieve by the humdrum pursuit of more conventional science." (A2, pnna). These discourses paint a picture of Sheldrake as someone who dreams up a hypothesis to reach fame but will not accept that it is not up to scientific standards, even when presented with the results that show it. While critiquing Sheldrake on a level of his personal choices regarding hypothesis design as well as emotional attachment to his hypothesis, Rose attaches other discourses of broader scope to Sheldrake. Rose asks:

> Granted its scientific and philosophical implausibility it is worth asking why the Sheldrake hypothesis has continued to receive any publicity. Partly this must be due to the tireless and ingenious advocacy of its author, who has encouraged regular public involvement in devising "tests" of his hypothesis, with prizes for the winners. Partly too, I believe it is in tune with a powerful anti-rational trend in these post-modern times, in which Nieztsche is more frequently cited than Voltaire. Along with parapsychology, corn circles, creationism, ley-lines and "deep ecology", "formative causation", or "morphic resonance" has many of the characteristics of such pseudosciences […] Morphic resonance, with its mixture of seemingly straight scientific concepts drawn from developmental biology and mysticism, offers to the 20th century something akin to what Hahnemann's homeopathy offered to the 19th." (A2, pnna, spelling errors in original)

It is as if anybody should see at first glance that Sheldrake's hypothesis is a dupe. Rose views Sheldrake's hypothesis as something that could only be believed by people following an 'anti-rational trend' and believing in other humbugs such as homoeopathy, corn circles and ley-lines. Rose here also talks about Sheldrake devising "tests" for his hypothesis. This is at first glance seemingly in contrast to the fact that Rose himself is involved in testing Sheldrake's hypothesis. Nevertheless, the tests Rose mentions here are with reason put in quotation marks. Rose sees them not as scientific tests, but rather as pseudo-scientific ways of involving the public, of gaining publicity and therefore more attention. These tests are standing apart from the experimental tests of science, which are done with rigorous experimental design and the involvement of the scientific community. While he in this paragraph distances himself from these 'tests', there is a paragraph further down in the article where Rose writes:

> Bateson is an experienced ethologist and pioneer of imprinting studies in the chick, who originally initiated me into the world of avian learning in the late 1960s […] and who has (like me) acted as a judge for some of the competitions Sheldrake has run to "test" morphic resonance. (A2, pnna)

This paragraph then leaves the question open of why Rose agreed to be a judge for something that he found quite clearly to be pseudo-scientific. This question along with some others concerning the reasons why Rose was willing to work together with Sheldrake in the first place, could have the possible answer that Rose changed his mind regarding Sheldrake and his hypothesis during the experiment. This could be one possible explanation for Rose's strong denial of all credibility here to Sheldrake. This will a little further down be picked up again.

Rose here in this article quite clearly draws a boundary between what counts as science and what does not. Parapsychology, mysticism, homoeopathy, untestable hypotheses, emotional

attachment to a hypothesis so it cannot be let go in case of it being disproved, all these things have no place in science. And Sheldrake with his hypothesis fits right into that.

7.2.8.2 Sheldrake

Sheldrake attaches some discourses to himself, not as few as Rose but also not a lot. The discourses Sheldrake attaches to himself can be seen as him being grateful, regretting not taking more care as well as a willingness to keep working on the experiment with Rose. Sheldrake states that he is grateful to Rose for providing facilities as well as their work together. He is also grateful to Rose's assistant as well as Professor Bateson. Sheldrake paints himself by mentioning this as someone who is holding community and the work done together in high regard. Sheldrake also acknowledges that there might have been some things that could have been done better. By this, he means that he could have worded his prediction better since this wording was then also used as a way of fitting the results to the underlying phenomenon: "I should have worded my prediction more carefully, emphasising that what was important was an increasing difference between the response to test and control stimulus." (A1, pnna). He also feels that some parts of the experimental design could have been better. This is opening a further dimension of regress. Even though the experimental design was fixed and both researchers agreed and went with it, that does not mean that it cannot also become the focus of a regress. This keeps open the possibility for further designs and can be seen as a kind of detour from the regress at hand. To argue that something could have been done better can be seen as wanting to move further, leaving this experiment behind. That would mean that the regress about this experiment is closed by opening up another about the next experiment, which might have better conditions to work in for Sheldrake. While from Sheldrake's view some things were not perfect, Sheldrake paints himself always as the one willing to make it work, to hold up the contract between the two. He talks about Rose's method of analysis routinely used in the lab as being a good fit for the experiment, bringing attention to mistakes Rose might have made in the analysis and supplying the correct data, and changing his analysis to better fit with the factors of the experiment. Sheldrake sees the experiment not necessarily as failed, but rather as having unsatisfactory results, which could be improved and done better with future experiments. He shows himself optimistic that with enough care the results could show even better that the phenomenon he is interested in exists. All the talk about better experimental design means not that Sheldrake sees his hypothesis as something in need of revision, on the contrary, he sees the experimental design as something that could be even better tailored at eliciting definitive answers to his hypothesis.

What Sheldrake has to say about Rose is quite ambivalent. This can be in part traced to the fact that his tone changed between the first and the third core article. His response to Rose's

response to his first article is more personal and critical about Rose. Sheldrake credits Rose's openness to the testing of his hypothesis. Sheldrake also talks about Rose making "the common-sense prediction" about the results of the experiment. This means that Sheldrake acknowledges that his hypothesis is radical, and that common sense might be to not really at first believe it. Then the first critical instances of Rose arise. Even though he was open and willing to make the experiment, Rose was the one in Sheldrake's view who withdrew from the agreement to publish the results. As we have seen already above, Sheldrake always styled himself as the one being open to continuing the experiment. Not only does Rose break their agreement, but Sheldrake also critiques Rose's general stance regarding the experiment. Rose is seen as being very sceptical from the start, writing an attack on the hypothesis in The Guardian, publicly denouncing it in the strongest terms and from the start being certain that the hypothesis would be disconfirmed. While Sheldrake acknowledges the fact that Rose was willing to test the hypothesis, he paints Rose as doing this because he was sure that Rose would remain in the right. From Sheldrake's view, even though never openly discussed as such, Rose is shown as one who would only be forthcoming in his security of winning. This would run counter to the argument that Rose makes that he is willing to help move science forward. Sheldrake also critiques Rose's way of work. Rose is using data with systematic errors even after being made aware of it, he misrepresents evidence and ignores results. Rose is also just quoting Bateson to add to his authority. All these things can already be seen as Sheldrake showing how science should not be done. Breaking agreements and mishandling data are quite clearly from Sheldrake's view unscientific. But furthermore, Rose is also behaving amorally in the way he deals with the experiment. Sheldrake critiques Rose's "polemical [writing] style" (A3, pnna) along with the lines:

> Rose has had many years of experience in the realm of political controversy. His technique is to try and endow his own belief with a tone of objective authority. At the same time he tries to discredit opposing opinions by playing on prejudices. In the present case, he gratuitously attempts to associate the hypothesis of formative causation with creationism, pseudoscience, parapsychology, crop circles, deep ecology, homeopathy, anti-rationalism and whatever else seems likely to arouse negative responses in readers who share his general beliefs. I imagine Rose would disapprove of such tactics on moral grounds if they were used by his political opponents. (A3, pnna)

Here Sheldrake quite clearly states that he thinks that Rose's style of argumentation is too political, too lacking morals for a scientific argument. Here the boundary is again drawn between what counts as scientific (rational argumentations, a kind of willingness to cooperate,

a kind of amiability, all shown by Sheldrake in his view) and unscientific (aggressive tone, dirty tactics, shown by Rose).

7.2.9 Moves Made in the Second Arena

We entered the second arena right on the heels of the moves of the first. The starting point was a clear idea: a crucial experiment. A crucial experiment, an experiment that would be seen as the defining factor of a hypothesis test, had benefits for both actors. For Rose, it was a possibility to move Sheldrake's knowledge away from the public sphere of The Guardian, and into a scientific context, where the hypothesis would be judged merely on its scientific merit and robustness. If the crucial experiment would go in favour of Rose, which Rose believes highly plausible, Sheldrake would lose power in his claims about his hypothesis. For Sheldrake, the experiment was a possibility to gather further evidence in favour of his hypothesis. A result proving his hypothesis, which Sheldrake believed would be likely, would give it a high amount of credibility. For both actors, the crucial experiment seemed like a good idea, even though it held from the start also the possibility of being wrong.

While Sheldrake and Rose clashed in the first arena, between the first and the second, they made their moves together. They planned together, discussed and agreed on an experimental design, and made clear what both believed the data would show. The plan had been to publish also together, no matter the outcome so that each would accept whoever was proven right and whoever was proven wrong. Their combined voices would further cement the results in the scientific spheres. Nevertheless, we know that it did not come to this. Rose and Sheldrake disagreed and could not reach a shared interpretation of the results. An experimenter's regress opened, where the closure of it would determine the outcome of the experiment. This is the start of the second arena, this is the start of the published articles. This is where the moves were made individually again, not jointly.

Sheldrake once again made the opening move. Whether he knew it or not, the experimental design, which would normally be the focus of discussions about an experimenter's regress, was now not open for debate. Sheldrake had to find a different way of closing the regress in his favour. We see here what was already pointed out, that credibility is a main deciding factor for the closure of the regress. Credibility is not something that one achieves on one's own, but credibility in the case of a controversy is always closely tied to a part of society. The social network surrounding the protagonists of a controversy is very much involved in the way it shifts and eventually, with luck, closes. Collins calls this the "core set" (1985, p.142) of actors, people involved in a controversy. Every actor in the core set can contribute something to the outcome of the controversy. If within this core set the controversy is closed, the knowledge will be seen

as certified, as robust, and can travel to the further reaches of the social network unchallenged. Sheldrake can be seen as already enrolling allies for his hypothesis by writing his articles in The Guardian. The readers favourable for his hypothesis would join the core set of the controversy on his side. Now that the controversy moved into another arena, some of these allies had no access anymore to the happenings of the controversy. Scientific journals are not open to the general public, one has to have knowledge and money to access them. Inevitably, Sheldrake might have lost some allies, or at the least has moved further away from them. That meant that the enrolment of other allies was in order. Parts of the scientific community, which would read the articles had to be convinced of Sheldrake's point of view. Sheldrake tried to do this in several ways. Being the first one to write about the failed experiment was a start. Tied together with his stance of being thankful for the opportunity, his logical reasoning behind his writing and his general openness to still work together with Rose, all these give a picture of a serious, yet open-minded researcher. Everything about Sheldrake's first article showed that he was not the one who broke the contract, he was willing to publish, he was willing to see the experiment in a way that could be made sense of (in favour of his hypothesis). Sheldrake can be seen through all this as just a 'good guy'. However cliché-laden that may seem, I think that it is not unimportant for him to choose this way of portraying himself. I would argue that it is much harder to oppose someone, who you think is 'nice'. Amiability is also not something that falls into the premade conception of pseudo-scientific charlatans who vociferate about orthodox science's shortcomings. Apart from that, Sheldrake keeps his criticism seemingly civil. His critique of Rose is done seeming on grounds of sound reasoning. Along the lines of which analysis would be the more logical fit, which data should be used, or that Rose's writing style is unfit for the discussion, Sheldrake is seen to take a more distanced and neutral stance. Sheldrake's writing is a move to position himself as amiable and reasonable, qualities which might be seen as positive ones for a scientist. Positive qualities are important to win allies, to gain credibility and to push the closure in a more favourable position for himself.

Rose has the next move, and he goes on the offensive. While Sheldrake's moves centre mainly around himself, Rose has a lot to say about Sheldrake, not a lot about himself. While Sheldrake shows that he does science in a specific way, Rose is more concerned about what lies beneath. Rose's main target is Sheldrake's hypothesis. Credibility as a scientist is not only conveyed by how one does things but also by what somebody has to contribute. To both Rose has something to say. As we have seen Rose paints Sheldrake's hypothesis as an empty one. He also connects the hypothesis to a plethora of other discourses, all of them seen as unscientific by the scientific public. Ley lines and homoeopathy are not seen as belonging to the realms of science. Rose here also addresses his core set. While Sheldrake is dependent on the goodwill of people reading the publications to enlist them as allies, Rose has an easier

position. He already stylised himself as the one who was willing to test every hypothesis, no matter how strange it might seem and offering up his time and space. But in difference to Sheldrake, Rose is closer to the view the scientific majority holds towards Sheldrake's hypothesis. Rose has been pushing the controversy into the scientific realm, moving it from a newspaper to a published source. He hoped to dismantle and discredit the hypothesis before it gained more traction. All this nevertheless should be done after the implicit rules he holds for science. While these will become clearer later on, here they surface in his views on the *Nature* editorial. He strongly condemns the insinuation of book-burning and stands for an open-minded discussion about science. Even though he was sceptical from the beginning of Sheldrake's hypothesis, he wanted it to be given a chance. After the experiment, he saw himself confirmed in his view that Sheldrake's hypothesis was wrong. Sheldrake could not accept this result, and this led Rose to step away from their agreement. While one can only speculate the reason behind it, it seems that for Rose what he wanted to achieve was already achieved. Rose wanted to see for himself if the hypothesis would be confirmed. It was not, Rose was satisfied. It might be that he was willing to publish this result, but Sheldrake did not agree to publish a result that was clearly not right in his view. From Rose's point of view, it might have been easier to just step away from the experiment than to spend more time and energy on deliberating with Sheldrake. When Sheldrake then published the first article and painted the whole experiment in favour of himself, Rose had to answer. Sheldrake was twisting the results to favour his hypothesis. Rose now answered in a way he saw fit. He pointed out in which ways Sheldrake's hypothesis was flawed, and in which ways Sheldrake himself kept holding on to something that was disconfirmed. Not only did he point those things out, but he also brought Professor Bateson into the discussion. Rose managed to give himself credibility through the enrolment of Bateson as an ally. Sheldrake's credibility Rose undermined by connecting the hypothesis Sheldrake created with other examples of pseudoscientific work. By putting them on the same pedestal, they were shown to be nearly the same. To Rose, this was pointing out what most of the scientific community suspected, but he himself had now witnessed. That Sheldrake worked unscientifically, but still wanted to have his hypothesis accepted. Rose once again publicly denied Sheldrake's claims for truth, cementing now also in a scientific journal, that Sheldrake's hypothesis is nothing but humbug. With this Rose was finished, and even though Sheldrake wrote another article critiquing Rose and his work, for Rose this was only once again a sign that Sheldrake could not accept the inevitable, that the knowledge he wanted to produce would not be taken up by science.

7.3 An Aftermath: Other Scientific Articles

To get a feel for what is left of the controversy apart from the two actors, a look into the databases for citation statistics is useful. This controversy was never contained in a specific manner, it was always accessible to specific parts of the public, or rather, defined publics. While there were certainly parts of this controversy that cannot be reconstructed, such as private communications between the two actors, the documents used here are all accessible in one way or the other. While it is not necessarily part of the analysis to look at what was picked up by the different publics, I want to look at the published resources which cite the core articles written by both actors. While it is not always possible and feasible to see where knowledge travels, citations of the core articles give a sort of partial view on how the controversy has been picked up. This is certainly not an arena, since the actors are not actively involved in it, but it is nevertheless a part of the controversy, what is left behind once our actors cleared their stages in the arenas.

I will look at the three core articles and see where and when they were picked up. Four different search databases were used.

The first article, „An experimental test of the hypothesis of formative causation" was according to Google Scholar (Gs) cited 36 times, according to Web of Science (WoS) 7 times, according to Scopus (Sc) 9 Times and according to PubMed 1 time.

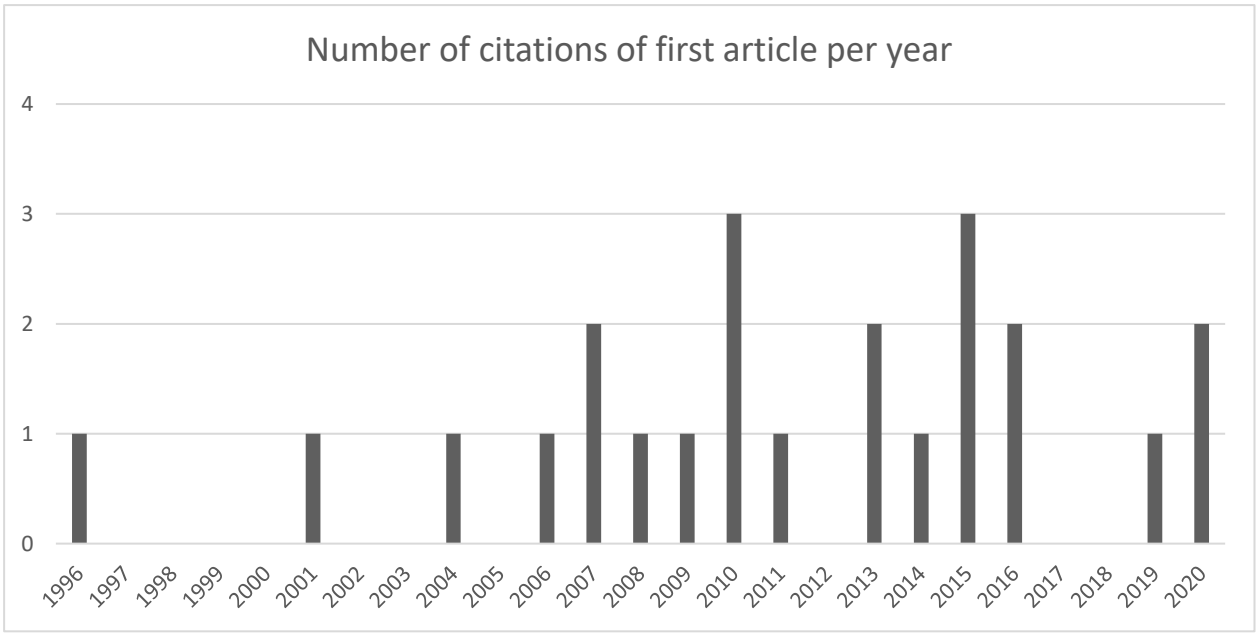

Figure 8: Graphical representation of citations of the first article by year

Looking at the graph above one can see the distribution of citations over the years. The core articles were published in 1992, and the first citation of them (excluding the pop-science books

by both actors) is 1996. After that, there is a gap, with the next citation in 2001. The bulk of citations is starting in 2006. From 23 citations, 20 happened after 2006. That is 14 years after the core articles. It seems like the controversy was not widely discussed in academic literature for quite some time. Only starting in 2006 sees a bigger uptake of the first core article.

Comparing that to the second article, “So-called “formative causation” – a hypothesis disconfirmed” was according to Google Scholar (Gs) cited 16 times, according to Web of Science (WoS) 3 times, according to Scopus (Sc) 3 Times and according to PubMed 0 times.

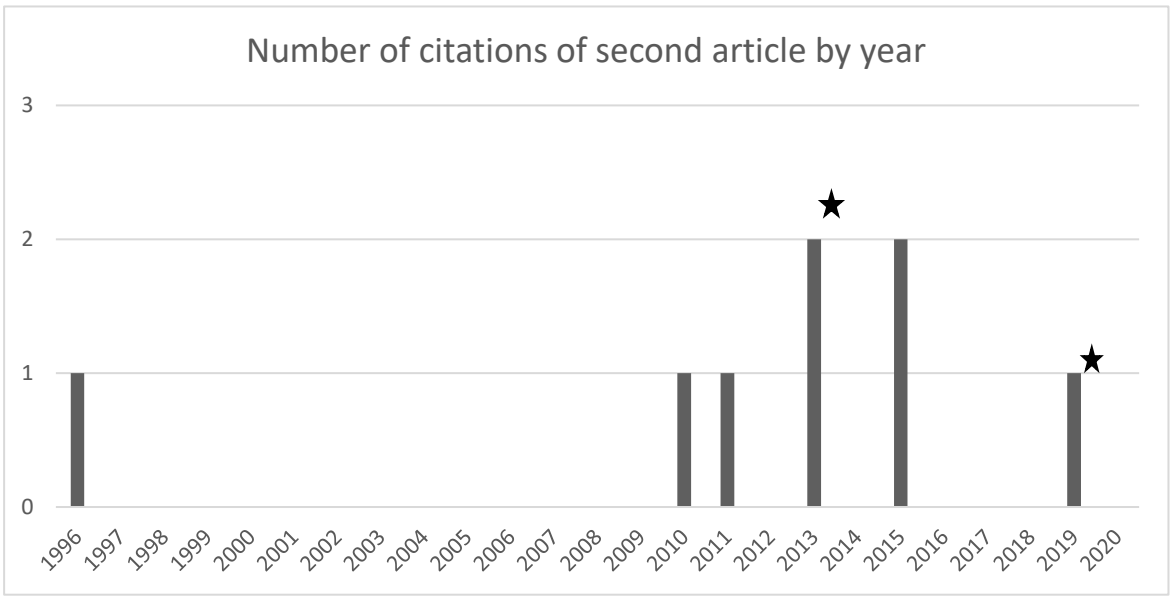

Figure 9: Graphical representation of citations of the second article by year

The distribution here looks slightly different, with the bulk of the publications quoting the article being published after 2010. What stands out, is that two articles solely quote the second article (marked with a star), while all the others quote the first and the second.

This can once again be compared to the citations of the third article, “Rose refuted” which was according to Google Scholar (Gs) cited 13 times, according to Web of Science (WoS) 1 time, Scopus and PubMed could not find the article in their databases.

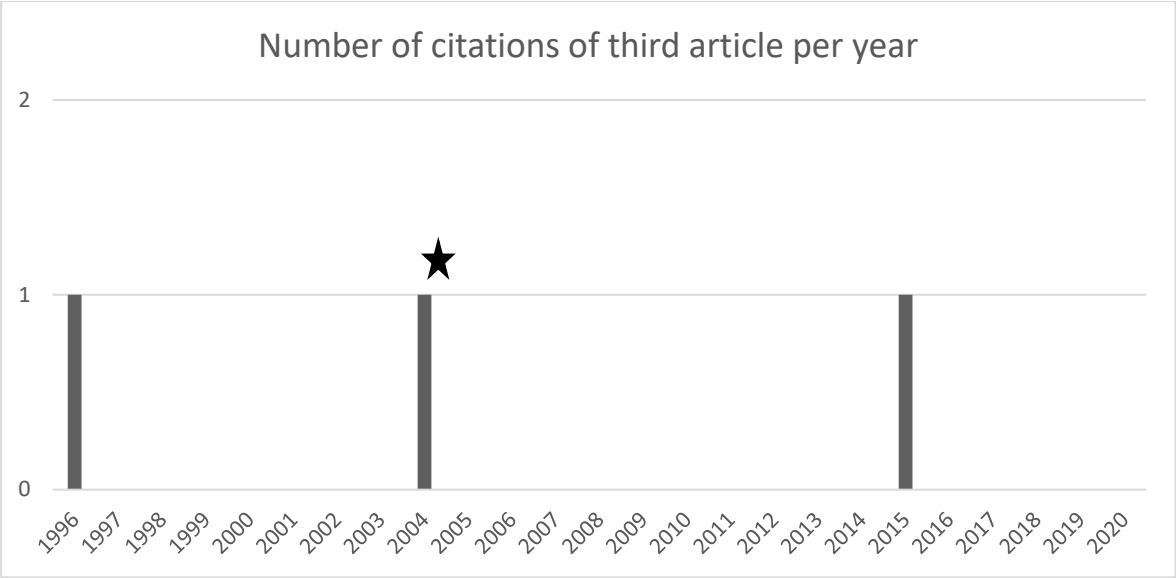

Figure 10: Graphical representation of citations of the third article by year

One can see very clearly here that the number of citations has shrunk here. Only three publications mention the third of the core articles. The star marks one publication that leaves out the article by Rose.

A few things can be drawn from these graphs. The first, which was already mentioned is the shift in time between the core articles and the publications quoting them. Finding out the reason behind this is not something that is the scope of the master thesis at hand, but some speculations can nevertheless be made. It seems that for quite some time the core articles were either not known or it was not acceptable to quote them. Quoting controversial research is not something that will bring one very far in the modern peer-reviewed system, and Sheldrake's standing in the scientific community was already controversial. Nevertheless, there are fields where such things might be easier to do than others. These fields also give a view into which allies were engaged in different parts of the public with the possibility to publish articles and are discussed below. The second thing that can be drawn from the graphs, is that the first core article was quoted most often, followed by the second and then the third. This would suggest that the publications who quote it might be favourable towards Sheldrake more than towards Rose. On the other hand, two publications only quote Rose's article, which would mean that these are more strongly favourable on Rose's side. The third thing that can be drawn from the graphs, and connected to the other things mentioned already, is the range of fields the core articles were taken up. The fields which cite the articles can be summarized as the following:

Parapsychology: 8 (1 leaving out Rose's article)

Psychology: 4 (2 of them only citing Rose)

Economy, business, innovation & consumer behaviour: 2

Plastic surgery: 2

Computer science: 2

Literature studies: 1

Neuroscience: 1

Counselling and psychotherapy: 1

Alternative medicine: 1

Unknown / not possible to classify: 3

When looking at the fields in which the core articles were taken up, one can see that Parapsychology is the field that has quoted the articles the most. While parapsychology is not an established scientific field, it nevertheless has journals. The reasons for this would also go over the range of this thesis, but it is the place where most of the allies for Sheldrake come from. Rose's allies come more from the field of psychology.

A detailed analysis of the published resources taking up the controversy is not the scope of this thesis. This chapter was rather a supplementary look into how it has clearly been taken up and still is to date. While the actors might have left the stage, their work, the moves they have done, have had a lasting impact on the ones watching.

7.4 THE THIRD ARENA: THE POP-SCIENCE BOOKS

After the three main articles which were published in a scientific journal, the controversy moves further into another arena. This is the arena of the pop-science books. Sheldrake and Rose have both written books on a range of subjects that also in chapters deal with their interaction. They can shed more light on the personal opinions of both researchers since they can be more strongly voiced in a book than in a scientific article. The books that quote the three main articles or generally include representations of the other researcher are:

- Lifelines, 1997, by Steven Rose
- A New Science of Life, 2009, by Rupert Sheldrake
- The Presence of the Past: Morphic Resonance and the Habits of Nature, 2011, by Rupert Sheldrake
- The Science Delusion, 2012, by Rupert Sheldrake

As can be seen above, there are several books written by Sheldrake and one by Rose who in part deal with their joint experiment. I want to now give an overview of what the books deal with and then I will go into detail on how the authors respectively describe their opponent and the joint experiment.

7.4.1 Lifelines: Biology Beyond Determinism

'Lifelines: Biology Beyond Determinism' was published by Steven Rose in 1997.

The book argues against the reduction of a plethora of biological considerations to the basis of genes. Rose heavily criticizes genetical determinism and starts the book by arguing against explaining social behaviour away by the occurrence of specific genes in humans. He then goes on to revisit the methodology of how scientists in biology arrive at their conclusions and their attempts to understand nature. He draws here also heavily on the philosophy and history of science. He quotes Popper, Kuhn, Latour and others known to the field of STS. The next chapters he spends arguing against reductionism and its habit to only analyse parts of organisms and to derive knowledge only from that. He closes the book with his thoughts on how to enrichen biology again by turning away from reductionism.

Sheldrake is mentioned in the index four times, with three different parts of the book involved. The joint experiment is found in the chapter 'Knowing What We Know'. In the chapter 'Knowing What We Know' Steven Rose gives an overview of how knowledge production in biology has changed over the last decades leading up to the printing of his book. He goes from Popper to Kuhn to Haraway and Latour and describes the way these authors have had an impact on biology. Of importance to him is what he sees as the dismantling of the claims of science onto reality. This seems to not go easy with him since he objects in several points to what Latour and Haraway have written. His strongest objection is that even though it is right that we cannot know reality, we can make hypotheses and check them with reality. He holds onto the view that one can see if predictions come true, and that is a guiding light in the scientific endeavour. To underline this point one can also look at an article he has written three years before the publishing of his book in The Guardian. In this article, Rose talks about his experience on a television show on BBC where 'heretical' scientists were portrayed, among them Sheldrake. Rose was invited to make an appearance, where he also said concerning Sheldrake that "the morphic resonance hypothesis was scientifically vacuous, and moreover that in joint experiments with me Sheldrake had conspicuously failed to find any effect which might require such a counterintuitive explanation." (Rose, 1994, p. 63). While this was the reason he was on the show, what is interesting is the way Rose talks at the end of the article about the scientific progress and the stance of these 'hereticals'.

> What was striking was how anxious the subjects of each of the six studies were, however heretical they felt themselves to be, to have their theories verified according to the orthodox canons of scientific proof – what they wanted above all was acceptance by the community. None challenged the claims of orthodox science to provide a Popperian gold standard of truth, of hypotheses tested by experiment. This isn't the case with the real heretics, who confront such assertions head on, at best allowing science to be but one story amongst many, at the more extreme challenging its very capacity to make truth claims or valid experimental inferences. Such critics come from both right and left. From the right they include religious fundamentalists, creationists and mystics. The left has its deep environmentalists who turn Gaia's science into poetry as well as those contradictory strands within the animal rights movement which on the one hand deny the biological continuity of human and non-human animals and on the other insist on the rights and moral value of all species with more complex nervous systems than oysters. Most importantly it includes those feminists who lay siege to the "one-sided" reductionist objectivity of a science which denies the validity of subjectivity, and who offer instead a more synthetic, holistic vision." (Rose, 1994, p.63)

What we see here is a strong stance against anything that would argue against that science has a right to truth claims. Even though Rose mentions scholars such as Haraway in his book, he can be seen here quite clearly distancing and denouncing strands of feminist science that critique the supposed objectivity of science. Parts of these sentiments can be traced, as we have already seen, also in his book. These sentiments can explain in part why Rose believed in the possibility of the joint experiment in deciding over Sheldrake's hypothesis. Without this strong belief in the capacity of science to make truth claims and the usefulness of predictions, the experiment would probably not have happened. Apart from Rose's strong stance as a believer of science's capacity to make truth claims, Rose has also specific views on how science should be done. He mentions that "to be scientific, hypotheses must be shared, tested and eventually agreed among a community" (Rose, 1997, p.66). As this quote illustrates, he believes that science should be done as a communal endeavour, where hypotheses are made, then shared, then tested, and then discussed in the community.

The experiment with Sheldrake seems to have been seen by Rose as a way of doing proper science in the beginning. He objected to the implications of book burning made within *Nature* and offered to test Sheldrake's theories. He wanted to follow his principle of share, test, agree. The problem with the experiment lay in the last part, to agree. In his view, this is a crucial step in the scientific endeavour and something of a boundary to other ways of knowing the world. Sheldrake and Rose could not reach a conclusion, their ways were too different. Because the

experiment ended in a stalemate, Rose brought in other members of the scientific community, foremost Bateson. Since Bateson agreed with Rose's interpretation of the experiment, that concluded the last step of his principle, that hypotheses have also to be accepted by a community, even if Sheldrake did not agree. Rose's principle was devised to "diminish the chance that [the hypotheses] are the idiosyncratic consequences of a particularly unusual brain at work". (ibid., p.66) In my opinion, Rose sees Sheldrake as exactly that: an unusual brain, but not exactly with a positive connotation. Something that the community of scientists should smoothen out with their way of doing science. He sees Sheldrake as a stubborn New Age philosopher, who nevertheless sees himself as making science. In the passage in the chapter where Rose details the experiment from his view, he writes that "I was proved right – to my satisfaction and to that of the other researchers in the field. Sheldrake, however, was able to convince himself that, viewed in a particular way, the data supported his hypothesis" (ibid., p.49) Here one can see again how the last part of Rose's principle was the problem to the closure of the experiment, and that Rose in his view was in accord with the broader scientific community, while Sheldrake could not accept the disconfirmation of his hypothesis. In the next sentence, Rose writes: "This just goes to show how little facts 'speak for themselves'. We all cling tenaciously to our views of the world; rather than accept an interpretation which destroys our paradigm, we wrap the paradigm in supplementary hypotheses" (ibid., p.49f). This quote shows an interesting mix of reflexivity as well as labelling of Sheldrake as stubborn. Rose points out that facts do not speak for themselves but means here more that the facts could not convince Sheldrake, even though they should have. Rose means by this not that facts, in general, do not speak for themselves. He believes that an interpretation is necessary, but that there are nevertheless clear results, as we have seen with his views about the way predictions work. Sheldrake meanwhile did not accept disconfirmation and goes on to add more supplements to his hypothesis to keep it working. From Rose's point of view, Sheldrake is against all better judgement holding on to his hypothesis and nothing could make Sheldrake see otherwise. Rose speaks about this with a sense of regret. He speaks of "incautiously" (ibid., p.49) offering the joint experiment. I think that Rose had the idea that Sheldrake would follow the same scientific principle as Rose and would accept a disconfirmation if he saw the outcome of the experiment did not match his prediction as well as that the scientific community did not agree with him. As we have seen in the second core article, Rose has been involved in Sheldrake's "contests" before the experiment. After the experiment Rose quite strongly distances himself from these contests and paints them in an unfavourable light. This rapid change of view regarding Sheldrake connected with Rose's regret of offering to do the experiment leads to the conclusion that Rose's view of Sheldrake changed in the experiment. It is as Rose sees with disappointment that Sheldrake has not the same scientific standards

as Rose. The experiment showed that Rose's view of Sheldrake did not hold up, they could not agree, which led to the way the experiment ended.

In the chapter 'The Triumph of Reductionism?' Rose critiques reductionism in the field of biology. His core argument is that biology cannot be reduced first to biochemistry, then chemistry, then physics. He believes that epistemological diversity is needed to capture the ontological unity that is the world. He argues for the necessity to look at the different 'levels' to understand biological phenomena. Rose then draws on 'holons' and 'the sum being more than its parts' to explain why reductionism is misplaced in biology. In his opinion, the interactions between parts are crucial for biological systems and that they cannot be explained by reducing systems to their parts and looking at the parts. Rose aligns himself here with a long tradition of critique in biology. One of the main critiques from the holistic materialism side brought up against the mechanistic materialism side is that biological systems cannot be explained by taking them apart, by reductionism. Rose, therefore, aligns himself with Holism, even though he does not mention it explicitly. What Rose mentions is that he does not like the saying 'the sum being more than its parts' because it leads often to "hand-waving or New Age sloganeering" (ibid., p.93). It can generally be observed that Rose has a strong dislike against everything connected to 'New Age philosophies'. In several passages in his books, he paints New Age philosophies as being misguided and as kind of a counterpoint to science. When he talks about New Age philosophies, he also often brings Sheldrake by name personally into it. The two passages in this chapter, where Sheldrake's name is mentioned are relatively short, but both paint Sheldrake quite negatively:

> This search for other meanings [apart from reductionism] lies at the heart of New Age philosophy's rejection of reductionism – a rejection abetted by a few ex-biologists, well exemplified by Rupert Sheldrake and his theories of 'morphic resonance' – indeed, I can think of no one who better fits Dawkins' epithet of 'holistier than thou'. (ibid., p.73)

Rose then goes on to say that others, himself included, critique reductionism systematically and "based upon a coherent philosophical and political analysis" (ibid., p.73). This shows that Rose sees himself as offering a systematic, well thought through and scientifically guided critique, while Sheldrake searches for meanings in the realms outside of science. The other passage mentioning Sheldrake goes as follows:

> And, historically, writers and poets who opposed the reduction and mathematization of the universe, the Blakes, or Goethes, the nineteenth-century 'nature philosophers' with their romantic pleas for a non-reductionist alternative, the philosopher Bergson with his vision of a non-physical life force, or their twentieth-century avatars like Sheldrake,

> have simply been unable to come up with an effective alternative experimental programme. (ibid., p.78).

Here we see again that Rose thinks Sheldrake has no real scientific merit in his hypothesis, that he only critiques without delivering a better explanation to the scientific world. Meanwhile, Rose has something to propose that he wants to put instead of reductionism, which as mentioned before, is a specific way of thinking inspired by Holism. He even mentions Driesch in the next paragraph, who at the end of his career was a strong proponent of Vitalism. What is fascinating is the way Rose positions himself and Sheldrake. Rose in his book critiques reductionism and proposes as an answer a kind of Holism. While arguing against reductionism Rose points to a tradition of resistance against reductionism which started with Driesch. While he strongly opposes Driesch in later parts of his book, in the chapter “The Triumph of Reductionism” Rose aligns himself unreflexively with a line of reasoning started by Driesch. This conflict can be interpreted in the way that Rose opposed Driesch’s way of work, not necessarily all his ideas. Rose banishes Sheldrake to the fringes of science and into New Age philosophy, Sheldrake is seen in Rose’s view as a vitalist dressed up in the clothes of a holist, which in my opinion is why Rose is opposing Sheldrake quite so strongly. Both are doing nearly the same things. Sheldrake also critiques reductionism, proposes Holism, and draws on vitalistic writers. But for Rose Sheldrake nevertheless went too far. He is in Rose's view a vitalist, undermining Rose’s efforts for a holistic approach because he uses the same theoretical and literary resources but achieves a different goal. For Rose Sheldrake does not stop at Holism, but goes straight to Vitalism in his argumentations, but since they base on some of the same ideas this influences the credibility of Rose as well.

In his chapter ‘Genes and Organisms’ Rose talks about the influence of genes on the shape and appearance of organisms. This quite long chapter is going into detail about the biological foundations and history of the term gene and its importance as a subject of study in biology. The chapter describes the biochemical interactions between genes, proteins, cells, etc. The topic of the development of organisms is something that has sparked vigorous debates in the history of biology. This is also where most of the battles between Vitalism and Mechanism took place. While Vitalism is today nearly completely absent from the biological discourse, this was not the case in the late 19th and early 20th centuries. Rose also speaks about the history of biology and the stance of different biologists of the time regarding the development of organisms. Rose describes a conflict between Driesch (avowed vitalist in his later career) and Loeb (avowed mechanist). While this is befitting for his chapter about the history of the theories about genes, what is striking is that he draws a connection to the biologists at the time of the writing of the book. “Driesch’s mystic formulations, oddly analogous in his day to Sheldrake’s

in ours […]" (ibid., p.108) "If Driesch was the Sheldrake of his day, Loeb was certainly the Dawkins." (ibid., p.109). Rose compares here quite clearly historical figures with contemporary figures and sets them one to one. Sheldrake = Driesch, Dawkins = Loeb. Sheldrake, as well as Dawkins, are figures in the realm of biology that Rose is heavily opposed to. Sheldrake because of his mysticism, Dawkins because of his rampant reductionism. With the sentences shown above, Rose does a specific rhetorical move of boundary work. With setting Dawkins and Sheldrake equal to well-known figures in the history of biology, he attaches to them also the views that are held about these figures. While Rose seems to criticize Dawkins quite heavily (his name is mentioned more often than Sheldrake's), I want to focus more on the way Rose compares Sheldrake. Driesch was quite a known figure in biology around 1900. While his early work was setting standards for others in the field of developmental biology, Driesch saw in his experiments proof that the mechanistic way of looking at the world could not be held up and started proposing vitalistic explanations for his findings. He held on to his beliefs and his work was then more and more disregarded and he is nowadays by most remembered as being misled. When Rose puts Sheldrake equal to Driesch, that means that Sheldrake is misled in his views and does not follow the scientific consensus and that his work will probably be only remembered as a side note in biology, not having contributed anything of worthiness. While comparing Dawkins and Sheldrake to these historical figures, Rose positions himself as well. As I have shown in the excurse above, Loeb and Driesch were the most prominent proponents of Mechanism and Vitalism in their time. In a spectrum ranging from Mechanism to Vitalism, the ends would be Loeb and Driesch. When he so clearly puts his contemporary biologists in the boxes of Mechanism and Vitalism, there is only the box of Holism left. In the rest of the chapter, this is also what Rose proposes. To view genes and organisms as influencing each other, like two-way communication. His opinions and views seem very clearly to be holistic, even though Rose never openly mentions that.

7.4.2 A New Science of Life

'A New Science of Life' was first published by Rupert Sheldrake in 1981 but was updated, revised, and republished in 2009. This is Sheldrake's first book that proposes the theory of morphic resonance, and which caused such uproar. The joint experiment is logically only integrated into the updated version.

As one can see, this book revolves at first about the unsolved problems in biology and then mostly about the form of biological organisms. This is the core of Sheldrake's theory, that form is passed on through fields from past organisms to future ones. He then goes on to apply this not only to the physical form but also to forms of behaviour and thought. The joint experiment is only mentioned in 'Appendix A'. This chapter is subdivided into different possible tests to

(dis)prove Sheldrake's hypothesis. The chapter 'Appendix A: New Tests for Morphic Resonance' covers 10 possible experiments to test for morphic resonance, detailed on 52 pages. These experiments are designed by Sheldrake as potential crucial experiments, to test for the influences of morphic resonance. The experiments are designed in a way that they are falsifiable and are thoroughly explained as to why specific choices of experimental design are made. Where possible, already existing experimental results are mentioned. The chapter gives the feeling of well-thought-through experiments that are designed to act as crucial tests for already existing evidential evidence in favour of morphic resonance. Sheldrake gives the impression as if it were just a matter of showing more clearly that his hypothesis is correct. While the experiments are designed in a way that shows them to be falsifiable, the underlying feeling of the chapter is one of the joys of discovering, of hunting for something that is there but only needs to be shown more clearly.

The sixth sub-chapter and sixth proposed experiment features a description of the experiment done by Rose and Sheldrake. In the chapter called 'The Transmission of Aversion', Sheldrake talks about the possibility to test his hypothesis by conditioning animals to show aversion to specific stimuli. He would then look at unrelated animals from the same species and test if an aversion can also be found in them. The experiment that was done by him and Rose had this character. In this chapter, Sheldrake talks on three pages about their joint experiment with one included graphic.

On these three pages, Sheldrake revisits the circumstances that brought the experiment into being, describes what was done and the results. While the description is not used to propose a similar crucial experiment, the description serves as an anecdote showing results already in favour of the hypothesis of morphic resonance. The description focuses mostly on the experimental design of the joint experiment and a short history of how it came to be. This in itself does not open up any interesting points, rather some formulations stand out.

At the beginning of the part detailing the experiment, Sheldrake writes that Rose challenged him to test the hypothesis and states that "Rose was well known in Britain for his strong political views – he was a Marxist – and his robust polemical style." (ibid., p.267) The mentioning of Rose's polemic is something that we have already seen in the third article. We already know that Sheldrake is strongly opposed to using a polemical style while writing and arguing. What has until now not found mention is Rose's political view. While Sheldrake touched upon it in the third core article, it was just about Rose's amoral argumentation, which he uses with his political opponents. Now Sheldrake explicitly mentions Rose's political affiliation. The utterance of this in the same breath as the polemical style means that Sheldrake views this as a negative feature of Rose. Marxism never had a strong footing in Britain, with the membership of the

Communist Party of Great Britain steadily declining since the Second World War. Mirroring a general decline in Marxism across Europe, Marxism in Britain nevertheless always had a difficult footing because of many societal features. A Mixture of societal, political economical and other factors influenced the view of the general public on Marxism in Britain (Laybourn, 2005). While a historical analysis is not in the range of this thesis, it suffices to say that the general view on Marxism was not favourable at the time of Sheldrake's writing. Sheldrake uses here the discourses associated with Marxism to discredit Rose and his views. Through associating Rose with these discourses, Sheldrake also positions himself. Once again, Sheldrake positions himself as non-polemic, rational and factual, which could be seen as an important norm for him of how science should be done. Sheldrake seemingly ascribes to the view that science should be apolitical, a norm that Rose so clearly does not uphold.

Another interesting point is how Sheldrake describes the end of the joint experiment. In the description given in this chapter he simply states: "In my view the data were consistent with the operation of morphic resonance. In Rose's view they were not." (ibid., p. 269). With this quote ends the description of the joint experiment and the explanation of the possible crucial experiment starts. Mirroring Rose, Sheldrake here also talks about the way that they saw the data as different. From Sheldrake's point of view, Rose was the one who did not see the data for what it was. Sheldrake puts this here uncommented, as a small matter in a short sentence. This is the first time Sheldrake starts to distance himself from Rose, which will come up later again.

7.4.3 The Presence of the Past

'The Presence of the Past' is Sheldrake's second book and a more expanded account of his theories. It was first published in 1988 and then revised and expanded in 2011. As with 'A New Science of Life', the joint experiment is only mentioned in the revised edition.

As one can see by the page count as well as the overview of the chapters, this book spans a broader range of topics than 'A New Science of Life'. Sheldrake starts with a description of the basics of physics in the first few chapters. He then looks at the philosophical understanding of form, and at the problems that arise when investigating form in a biological setting. He then talks about his theories and hypotheses before going on to talk about genes and their relevance. The next chapters are then concerned with memory in animals as well as in humans. Building on that Sheldrake follows by talking about the role of morphic resonance for societies. He closes the book by sharing his view on evolution in a biological as well as a cosmic sense.

In the chapter ‘Animal Memory‘ Sheldrake gives an overview of the history of the theory that memory is stored as traces in the brain. He talks in this chapter about the research that has been done regarding the inheritance of memory in animals and the traces it leaves physically in the animal's brain. Sheldrake critiques this theory on many grounds, and the chapter can be seen as a critique in general against the mechanistic view of memory formation. Sheldrake sees the search for traces in the brain as being misguided since it rests on the basic assumption that there must be traces in the brain because thinking otherwise would not be compatible with the mechanistic view of memory. He then points out that there are already many experiments that showed the difficulties of locating traces in the brain. The experiments rather showed that there are many instances where memory was in effect that were not necessitated by a brain, or memory that was functioning even after damages to the brain. He sees these experiments as showing that the theory is becoming more and more cumbersome and that his theory of morphic resonance could fill the spaces that the mechanistic theory cannot explain. What comes through here is Sheldrake’s view of the process of the scientific endeavour. He believes that scientific theories are only good as long as they can explain the phenomena they investigate. If there are too many phenomena that cannot be explained by the old but could be explained by a new theory, the new theory should be taken up. This highlights again Sheldrake’s Kuhnian view on the scientific endeavour. Sheldrake sees the way memory formation and other biological phenomena are currently explained as lacking, and his hypothesis as being key to a better understanding of these phenomena. Therefore, his writing style is heavily focused on showing the faults and rough edges of the ‘old’, mechanistic theory, and how his theory could explain the phenomena better. In general, his writing has also something slightly detached from itself. His presence as an author is not always felt in the writing: the connection between himself and his hypothesis is rarely mentioned. He simply shows the faults of a specific way of thinking and supplements another, that could be more helpful. He then also gives examples of researchers that have looked at the phenomena and found evidence for the hypothesis of morphic resonance. This space willingly left between himself and the theory seems to give the theory more substance. It is through this not merely something he dreamed up and propagates, but something he discovered, what might be ‘out there’ waiting to be examined and explained.

The segment about the joint experiment is spanning 3-4 pages with one graphic included. The four pages concerning the joint experiment are mainly dedicated to explaining the experiment in terms of experimental design and predicted outcome. The subject of the last paragraphs is the misinterpretation of data by Rose. After the passage concerning the experiment, Sheldrake talks about the possibility to test his hypothesis with other experiments, which concludes the chapter. There is not a lot that Sheldrake has to say about Rose and himself, solely that

Sheldrake views the data as supporting him, Rose does not, but Rose's argumentation and data is flawed. Here once again the tone is very distancing, very factual. Rose is wrong because his argumentation is flawed, and that seems to be all that needs to be said. Sheldrake seems to want to push the argumentation always back onto the statistical and experimental design part of the experiment. It seems like having a sound argumentation and statistical interpretation of the data suffices for Sheldrake to show that his hypothesis is correct and Rose's view is irrelevant.

7.4.4 The Science Delusion

The Science Delusion was published in 2012 and recapitulates much of Sheldrake's work but then branches out to become a more general critique on central dogmas of science from Sheldrake's view.

The book speaks about the central dogmas of science itself and if they still are upholdable today. The chapters are dealing with one dogma each and bring up questions or examples of contradictory evidence. They each conclude with thoughts on what could be gained by abandoning the dogma.

Interestingly, even though the references include mentions of two of the three core articles (the two written by Sheldrake) they are not used in the book as quotations or in the notes. While it seems that the core articles influenced the writing of the book, the joint experiment is not mentioned explicitly. Nevertheless, some of Rose's work is used and his name is mentioned in a few passages.

The mentions are found in the chapter 'Are Memories Stored as Material Traces?'. This chapter is close in style and substance to the first Guardian article written by Sheldrake. The chapter deals mainly with the question already posed in the chapter's name: if memories are stored as material traces in the brain. This was also the topic of the first article analysed here. Sheldrake starts the chapter thematically by addressing that the storage of memory as material traces in the brain is a theory. He also addresses quite clearly that that is a mechanistic understanding, therefore incorporating Rose's research in a mechanistic framework. He then goes on with the assumption that memories are stored materially in the brain and shows the state of art in the research done on the localisation of memory traces. While recapping the state of the art, Sheldrake points out all the ways in which researchers came short of finding the traces and how they explained their 'failure' to find them. After pointing out how researchers are baffled by the unsuccessful location of memory traces, he goes on to tell the reader what other researchers have critiqued in the theory of memory traces. He shows examples of different researchers and quotes what they said about where the theory is lacking in explanatory power.

He then goes on to say that the reason that the theory is still held by the vast majority even though it is critiqued by many shows the strongly held assumption that the traces must be there, therefore they must be possible to find. Sheldrake then shows the experimental evidence which points towards this assumption also being wrong (in a kind of Popperian disproving manner). After all this, he proposes his own theory as a better fit with the evidence, which helps solve the problem of locating memory traces. His theory would locate memory in the 'morphic field'. He then goes on to describe in more detail how existing phenomena could be explained by his theory as well as the problems of the existing theory. With the description of the merits of his own theory the chapter ends.

His argumentation follows here a Kuhnian and Popperian schematic, even though it is never explicitly mentioned as such. Sheldrake first shows the existing paradigm in neuro-biology. He then points out where the paradigm becomes cumbersome and cannot explain everything anymore. Then he shows that there is reason to believe the paradigm is wrong because there is evidence speaking against it. At last, he points out how his own theory and paradigm could help solve all the problems the old one could not. This follows the classic steps of the scientific revolution described by Kuhn (1962). It appears Sheldrake fully believes that the mere argumentative showing of a necessary paradigm change would lead to one.

Rose and the joint experiment hold a strange place in the book. The joint experiment is never mentioned, even though Rose is. But Rose is also not mentioned in connection to the joint experiment. Rose is mentioned at the beginning of the chapter, twice, and both times by name. The first mention is a quotation of Rose talking about the location of memories. This is used by Sheldrake as an entry point and summary of the prevalent paradigm. There is no direct critique of this quotation, but rather Sheldrake goes on to say that it is not so easy as the quotation makes it seem. Then starts the part of the chapter where Sheldrake talks about the shortcomings of the prevalent paradigm.

The second mention of Rose comes a little bit later. Sheldrake talks about the 'new generation' of researchers investigating the localisation of memory traces. He then talks about Rose and his colleagues and their experiments, supplied with quotations of the publications of Rose and his colleagues. After describing what kind of experiments they did Sheldrake concludes with: "Once again, the hypothetical memory traces proved elusive, and once more those who searched for them were forced to postulate unidentified 'storage systems' somewhere else in the brain." (Sheldrake, 2012, p.177). The way Sheldrake talks about Rose, his colleagues, and the experiments, seems as if Rose were just another of those researchers that held so strongly to what they believe that they disregard the evidence calling for another paradigm. Rather than accept that they might think in the wrong direction altogether, those researchers would hold

onto their own theories but supplement them to still fit with the paradigm. With this Sheldrake makes the same argument as Rose in his book. Both paint the other as holding on too strong to their own views whilst not accepting the evidence that contradicts them. This similarity of reasoning is something I will pick up again a little further below.

All considered, Sheldrake seems to have distanced himself from the experiment with Rose. Throughout the three books he has written which include Rose and the core articles in some ways, Sheldrake seems to have become more distant from the experiment as time moved on. The mentions of Rose and the experiment become gradually less and less personal and detailed. While he still talked in his first book about Rose's polemical style, the last book fails to mention anything personal about Rose rather than that his research could not find the results he hoped for. Rose is pushed more and more to the sidelines. The conflict between the two takes less centre stage. It seems that for Sheldrake the close retelling of the experiment and his relationship with Rose seems not any longer necessary.

7.4.5 Moves made in the third arena

Some time has elapsed between the second and the third arena. This time Rose will be the first to make a move. While we will see how both our actors make moves uniquely, there are some moves made that are closely related.

Rose starts in this arena since his book is the first to pick up on the joint experiment. While Sheldrake technically published his books earlier, they were published too early to incorporate the joint experiment. Only in the revised editions Sheldrake talks about the experiment. Because of the format of the books, Sheldrake and Rose have more space to talk about their views. The published articles were limiting, and we have seen that many of the discourses that both have already touched upon are elaborated more clearly in the books. Nevertheless, new details emerged and have become important. Rose reviews Sheldrake and the experiment after his own ideals of science. Firstly, Rose believes in the possibility of experiments deciding over predictions and therefore informing about theories and their underlying phenomena. While this was implicitly clear from the start, since this was the ground for the joint and crucial experiment, it is spelt out in Rose's words in the books. Secondly, Rose believes in the ideal of 'share, test, agree' in science. Rose here puts the importance on the community. The scientific community is in the end the deciding factor if hypotheses are proven or disproven. Compared to these ideals, Sheldrake fails on all fronts from Rose's point of view. He articulates again in his books how Sheldrake could not accept the results. From Rose's standpoint, the data was conclusive, it was clear that he was in the right, and in his model of scientific work, he was also proven right. It was Sheldrake who could not accept being proven wrong. What

makes it worse is that Sheldrake still denied the possibility as Bateson supported Rose. In Rose's ideal that means that Sheldrake also did not accept the community's verdict. It seems that in his book Rose searches for the reason behind that. He finds it in Sheldrake's philosophical stance towards biology. Rose sees this as the underlying reason for Sheldrake's nonacceptance of disconfirmation. As we have seen Rose associates Sheldrake with a historical figure, Driesch, and therefore also with Vitalism. Rose believes that Sheldrake has a vitalistic stance regarding biology, but dresses it up as holism. Through this description of Sheldrake Rose also does boundary work. He reinforces the placement of Vitalism as outside of science and includes Sheldrake in it. This argument reaches further than the other arguments Rose had before about Sheldrake. While it might be debatable how exactly scientists should do their work, some philosophical traditions are seen as deeply questionable and non-connectable with science. Vitalism falls into that category. No matter what the hypothesis looks like, if it is built on a foundation of Vitalism, it is doomed to fail. The move Rose makes to discredit Sheldrake goes deep into the philosophical roots of Sheldrake's hypothesis. It is a move to demarcate Sheldrake as a *former* scientist, who for whatever reasons, travelled down the path of Vitalism and through this is from now on always apart from science. All these moves made by Rose are also done to speak to the core set. Through them, he tries to further cement the view of his readers of Sheldrake's un-credibility. It is the last step of many in which Rose has attached a variety of discourses to Sheldrake, all speaking of the way his views lie outside of science. Vitalism and a failure to be part of the community are the latest way of engaging the core set, and the ones which might hold a lot of persuasion power for Rose's readers.

For Sheldrake, some of the same things happen as for Rose. Sheldrake also details some strands of thought that were already in existence before. Most notably, Sheldrake brings up again his view on scientific progress. As we have already seen, he holds on to the Kuhnian idea of progress, where old paradigms become cumbersome and need new paradigms. For Sheldrake, his hypothesis is the new paradigm that can replace the old. Although he never states this explicitly, his writing is heavily influenced by this notion. What Sheldrake does not mention, and maybe knowingly so, is that new paradigms are not established by one person. There is always a community that supports the new paradigm, that uses and spreads it. In many parts of this analysis, the core set was of importance. Exactly this core set has the power to help by using and spreading a hypothesis and therefore giving it more weight. Sheldrake here once again tries to sway his audience in becoming allies in the core set. The audience of the books is once again different from the scientific articles but might lie closer to the audience also reading his Guardian articles. There he also advertised his books, which means that this is also one of his target audiences. If these people are swayed and act as allies, they can help

to spread the word of his hypothesis and it would gain publicity. A scientific hypothesis that is publicly known by many people is in better shape to become a new paradigm than if it would not be known. Following Collins (1985) the core set has not to be only scientists, but people outside of the scientific community can nevertheless contribute to giving a hypothesis and its scientist credibility. By painting his hypothesis as the solution to problems in the status quo of science, Sheldrake tries to do exactly that. Rose through these falls more and more into the background. To engage his part of the core set, Sheldrake does not need to hold up a close connection between himself and Rose. Rose is more stylized as part of the old paradigm, who could not accept the data supporting the new, and still unreasonably holds on to his views. This argument is identical to the one used by Rose. Both paint the other as not being able to accept the data right in front of their eyes, to hold on to something only because of personable reasons, not because of scientific merit. Sheldrake also uses another argument already used by Rose. Sheldrake positions Rose as a mechanist, even though not openly, but through association with an existing mechanistic paradigm. Sheldrake positions himself as a holist, the same what Rose would say about himself. For Sheldrake Mechanism is the philosophical tradition that is unwelcoming to change, not able to let go of their entrenched views. A tradition not fit to have a say at the modern table of science. For Sheldrake, that also includes Rose. In a way, both actors want to position themselves as holists, as promoters of innovation in science, as the ones willing to move forward with new ideas. Both also position the other as belonging to a philosophical tradition with no power of truth claims, as emotionally attached to their views and not accepting the data which so clearly proves them wrong. As before, both try to achieve with this the enrolment of allies. Through their discourses, they hope to speak to an audience and convince them of their point of view. Here the medium of pop-science books is especially powerful in influencing the audience that takes it up. The stance of a learner (approaching the pop-science books to learn something) means an openness to be influenced, to be swayed by the views of the books, and through this to be recruited as an ally.

These moves are the last ones in the third arena, and the last ones done by our two actors. This does not necessarily close the controversy since they were never the ones able to close it. There were always others involved, a community that has not really gotten a say in the controversy. Apart from Professor Bateson, no other voices were heard in the discussion. Most notably, there is a silence of Amanda Harrison, the assistant who carried out the experiments. While deeply involved in the controversy, there is no statement from her to be found. There are nevertheless some others that, years after the experiment, had something to say about it. The scientific community cited and used the three core articles in their own publications. While this is not a way of assessing in which way the controversy was taken up in general, it is a small insight into the way how the controversy resounded.

8 DISCUSSION

Throughout the empirical chapter, I have already answered some of the research questions, even though sometimes only in parts. I want to use the space in this discussion to answer them more concisely and draw together the different theories and concepts used throughout the thesis.

The main research question was **How does the controversy between Sheldrake and Rose about the formative causation experiment unfold?** The answer to this question is tightly connected to the first sub-question**:** what the controversy is about in the actor's views. I gave a lot of detail in the empirical chapter to answer these two questions, but I want to recapitulate the most important parts here. It started in the Guardian, where Sheldrake was sharing his views on how neurobiology could and should be done, questioning paradigms that have been upheld for a rather long time. Rose saw this as a potentially harmful situation where Sheldrake was feeding misinformation to the general public, because of his misconception of general findings in neurobiology. Sheldrake was already in a marginalized position before the controversy with Rose began. Sheldrake had already published pop-science books and was excluded from scientific publications. The article in Nature (concerning book-burning) only cemented that more. Rose was opposed to this marginalization and wanted to bring Sheldrake back into the sphere of science, to resolve and dissolve the controversy **in** science. The first episodes of the controversy were about the following: for Sheldrake, it was about changing the status quo of biology, of inducing a paradigm shift through means of his own publications in different formats. For Rose, it was the containment of untrustworthy knowledge spread by Sheldrake in public fora but through the means of scientific conduct. This meant pulling Sheldrake back into the sphere of science to dismantle his hypothesis there.

In the next part of the controversy, it was about navigating the shared goal of testing the hypothesis. For a short time, both actors here pulled on the same strings to make it happen that the hypothesis could be tested.

After their disagreement, the stances resolved back to the ones before the experiment. Sheldrake believed in the rightness of his hypothesis and the possibility of inducing a paradigms shift. Rose believed in the misconception of Sheldrake and that it is untrustworthy and has no place in the scientific discourse. Though they revert to the stances they held before the experiment, both have now more arguments for their positions. This leads then also to the second sub-question of the status of the experiment. The experiment was meant as a deciding factor in the controversy, as crucial and definitive. That it failed to deliver this certainty meant

that the certainty of proof had to be found through other means. The experiment had still the capacity to decide in the actors' views, but both added parts and bits to it to still uphold its definitive character. Both actors saw themselves confirmed through the experiment. Since the experiment was supposed to be definitive, it could only prove one person right. This meant that for the other person reasons had to be found why he was not right. Rose and Sheldrake have plenty of those reasons that are supposed to withdraw the right to make claims for the experiment from the other. These range from the way statistics and data are handled, used and argued, to the way science should be done, with the importance of communal agreement or the courage to rethink and discard cumbersome traditions of thought, to the philosophical foundations of biology both actors hold. Each of these arguments draws on a plethora of cultural resources already used in debates about science. The status of the experiment was one that is unique in this specific case. While most of the research done has been on the ways scientists handle theoretical differences through each designing their own experiments (see State of the Art), in this controversy the experiment was designed by both opposing actors. That meant that how the experimenter's regress would normally be used as a concept did not fit perfectly. Nevertheless, the experimenter's regress helped as a concept to flash out the most important part: experiments alone cannot decide. As the concept states, the credibility of the actors is the deciding factor if a theory and the related experiments are seen as being appropriate approximations of reality. Credibility is influenced in quite a number of ways. The combining factor to all of the controversy is the fact that alone, both actors cannot decide it. They are aware, even if they might not openly discuss it, that they need allies, without their allies, they will not gain credibility. As we have seen in the State of the Art the production of knowledge is always dependent on a social sphere around it, which gives what is produced its right of existence. Without people to interact with what they do, the controversy will be just a quarrel between two scientists. This was as well underlined by the concept of theater of proof. The concept was crucial in highlighting how experiments are designed to be convincing, to show those watching in an unquestionable way what is happening. The documents that both actors produced are ways of convincing those who are watching/reading of their own way of seeing. This meant that arguments were constructed, literature used, rhetoric applied, which would leave those who are watching/reading convinced that the author's version is the one that has the claim to truth. All the produced documents speak of the need of both to enrol allies wherever they can.

The enrolment of allies is done in different arenas, which was the third sub-question. As we have seen, each arena opened up new possibilities of movement to both actors, and other audiences for whom they performed. The Guardian as a newspaper had its restrictions on the length of arguments but was open to a broad range of readers. The peer-reviewed articles

gave more space and weight to arguments made but were more restrictive in what kind of arguments were possible and how many people could access these texts. The arena of the pop-science books gave both actors the freedom to elaborate quite detailed their views as well as reaching a broad range of people, through its already established form of communicating in the United Kingdom.

Allies take many forms, therefore many ways of communicating have to be used to reach the maximum of possible people. Therefore, all the different media were used, newspapers, pop-science books and journal articles. Both enact their disagreements openly, for an audience to see and hopefully to judge and carry further. This case highlighted the complexity of science communication. The trajectory it followed (newspaper, peer-review, pop-science books) is unusual but became a logical trajectory following the possibilities of the different arenas. The key in all the documents was that they were always designed as ways of interacting with a public, how different it might be in the different arenas. This meant that for the public, which was to be enrolled as allies, credibility had to be gained for one author or the other. The how of this enrolment was mostly navigated through boundary work. The concept of boundary work helped to highlight that both actors actively draw on existing cultural maps, some of which have been around for many centuries. The history of biology as well as the norms of good science were important maps in this controversy. Only through engaging with these maps was it possible to see how both actors tried to enrol as many allies as possible. Because if they cannot decide on how the experiment ended, then the last verdict lies with the community, be it scientific or not.

8.1 A Hypothesis of Sorts

Now I want to try to answer the fourth sub-question of why no closure was possible in the controversy. This question is layered, as there is not one closure of a controversy. Controversies can live on in one person, who will not accept it as closed. One answer to the question then is that indeed, the controversy is closed in the broader mainstream scientific community. With the involvement of Bateson as well as how these documents have been taken up in other scientific publications, one can confidently say that the controversy is closed from a standpoint of a broad part of science. It nevertheless lives on in some other parts. The fringes of mainstream science or generally groups outside of science (I will not draw boundaries here, this is not the scope of this work) still come back to the controversy, as the chapter on the uptake has shown. Both actors as well have seemingly not really let this controversy go. It might look different for both of them, but both still took up the experiment and their disagreements in their pop-science books. As long as one talks about it, it is not yet closed. So, this last research question concentrates more on why both actors could not between

themselves close this controversy. Why questions are notoriously hard to answer. I am sure that the answer I try to give here is not fulfilling the scope of the question, but nevertheless, I believe it important to try and understand.

It is impossible to trace everything that happens in a controversy since one can always only research it from a specific point of view. Through the situatedness of the research, some views will always be hidden. An answer to the question of why a controversy could not close will always be only partial and might look different from other views.

The view I have achieved through this research for myself could be described as the following: I believe that Kuhn and Fleck have in their works (Fleck, 1975 [1935] & 1986 [1947]; Kuhn, 1962) described important parts of (scientific) social life. Whether one calls it thought style or paradigm, and admittedly there sure are differences to what definition you choose, so is the focus nevertheless on the impact of the social life around a subject onto the way the subject thinks. I will work more closely with Fleck here because I find it more fitting to what I want to convey. Fleck has talked about how our perception of reality is shaped by the way we are enmeshed in social life, by the knowledge transferred to the individual by the group (Fleck, 1986 [1947]). The social surrounding tells us what counts as making up our reality and what not. These views of what is real are internalized as ontological commitments. These commitments are notoriously hard to let go of again. These ontological commitments were shaped by a community and are also one of the factors determining our inclusion into the community. There is only so much leeway to people who have competing views in a social group before they are expulsed. Examples of this are plenty, as we have already seen in the State of the Art, where controversies arise around people doing and thinking science differently, and if they think too differently, they have to fear losing their place in science. So far this was only a combination of already established theoretical thoughts. What now comes from me in form of a tentative hypothesis, to answer the question at hand is that shared ontological commitments are necessary for creating shared realities, and ontological commitments are somewhat hierarchical. I believe that there are ways of thinking, of perceiving reality, that have higher or lower importance in social groups. Deviance in lower importance ontological commitments can be accepted, deviance in higher importance ontological commitments not. These higher ontological commitments are often seen as that what defines science. I believe that these ontological commitments of higher importance are the place where the fiercest cases of boundary work happen. These commitments are the ones that have the highest impact on the lives of the individuals, a danger to these commitments is a danger to the way of living to the individuals. What I believe is happening in controversies is the negotiation of ontological commitments. Only if the parties involved can find a common ground,

bridge the differences in their diverse ontological commitments, a shared reality can be created. Where this is not possible, no shared reality can be created. The reason why this might often not be possible, I see in conflicting and unbridgeable ontological commitments in other places of higher importance. To bring this back to the case in hand: Sheldrake and Rose have many shared ontological commitments, they have both been educated in the same educational system in England at roughly the same period of time, they are both parts of the scientific community in biology, they both believe the status quo of science int heir field to be unsatisfactory and they both believe that the philosophical tradition of holism is the way to better biology. Yet there is a place that both divert in their commitments. Sheldrake's hypothesis, which is in my belief more than a hypothesis, but has become a sort of ontological commitment itself, is built on the belief that there is a new, a fifth, physical force, that makes it all possible. This would radically change the ontological commitments of nearly all scientists educated in the natural sciences (Collins has discussed something similar with this metaphor of the spider's web, see Collins, 1985). While Sheldrake and Rose can agree on many things, on scientific conduct and even made an experiment together, it always boils down to the one thing: Sheldrake's reality, informed by his hypothesis, and Rose's reality, informed by his hypotheses, will always divert massively in specific places. If one would accept the other's reality, it would mean to give up on ontological commitments that shaped their life up until this point. Giving up on such commitments of high importance is nearly psychologically impossible for them in my opinion. Other reasons will be found why there could be no common ground (I have discussed them already above) and the common ground they have will be ignored because the gulf between their commitments can not be bridged. This 'bridge-hypothesis' is only one possible answer to the question of why.

8.2 Ethics and Limitations

Ethical issues are limited in this research. The topics analyzed are not sensitive except maybe for the people involved. Caution was taken to not take sides in the analysis (which is a must for symmetrical analysis) and that the project would not influence or re-open the existing controversy about the topic. The end result of this project is in itself a document that might be used by others to influence the controversy, but this lies outside of the scope of issues to be dealt with in this thesis.

This thesis is limited only to the controversy between Sheldrake and Rose and does not include the wider repercussions of Sheldrake's and Rose's theories on the scientific community. The personal opinions of both researchers are only included in the analysis of how both represent themselves and their work and publications. No interviews to gather further insights into the views of both are planned. This would lie outside the scope of the project since only the steps

taken to close the controversy in different publication venues are analysed. As I mentioned earlier, the closure of this controversy still lies in the hands of the community. To analyse this part of the controversy further research would have to be done, which was not included in the scope of this thesis.

There are certain paths not taken, as there always are in research. The role of Amanda Harrison is one absence that has stood out in this project. It is a strand that the methods and focus of this thesis could not incorporate, nevertheless it is a strand that came to light. The role of Professor Bateson was touched upon briefly, but here lies another strand that could be taken up. The bibliographic analysis was only done rudimentary since it did not add much to the empirical analysis, but it is a strand that could be taken up as well.

I am sure that there are many other strands as well, that have been unidentified by myself because I am after all situated. I have done my best to trace the knowledge through its many forms, spaces and times. I hope that through following the knowledge produced and transformed in these controversies I have given a small insight, changed how things can be seen slightly so that something new has been said and done.

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10 APPENDIX A: ARTICLES AND BOOKS CITING THE CORE ARTICLES

The first article, „An experimental test of the hypothesis of formative causation" was according to Google Scholar (Gs) cited 36 times, according to Web of Science (WoS) 7 times, according to Scopus (Sc) 9 Times and according to PubMed 1 time. From these, double entries in different databases were fused (marked with short symbols) and double mentions in different languages omitted. The popular science books discussed above were also excluded. The entries are ordered chronologically and then alphabetically, to give an overview when the article was cited.

Searches were done at the following dates: Google Scholar and Web of Science, Scopus and PubMed on the 06.02.2021

Mikulecky, M. (1996). Sheldrake vs. Rose: chronobiology is the winner. In *Riv. Biol.-Biol. Forum* (Vol. 89, p. 469í477). (GS, WoS)

McTaggart, L. (2001). *The field: The quest for the secret force of the universe* (pp. 6-7). London: HarperCollins. (GS)

Kenny, R. (2004). What can science tell us about collective consciousness. *Collective Wisdom* (GS)

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van der Lei, B., Cromheecke, M., & Hofer, S. O. (2007). Mini face lift with suspension sutures: Historical analysis of development and morphic resonance. *Plastic and reconstructive surgery*, *119*(7), 2317-2319. (GS, WoS, Sc)

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Krajíček, J. (2009). Information hypothesis: on human information capability study. In *International Conference on Brain Informatics* (pp. 25-35). Springer, Berlin, Heidelberg. (GS, WoS, Sc)

Dace, Ted (2010). Analysis of Russell. *Journal of Consciousness Studies* 17 (5-6):41-54. (Sc)

Krajíček, J. (2010). Human Inspired Self-developmental Model of Neural Network (HIM): Introducing Content/Form Computing. In *International Conference on Ubiquitous Computing and Multimedia Applications* (pp. 29-43). Springer, Berlin, Heidelberg. (GS, Sc)

Robbins, K., & Roe, C. A. (2010). An empirical test of the theory of morphic resonance by using recognition for Chinese symbols. *Explore: The Journal of Science and Healing*, *6*(4), 256-262. (GS, Sc)

Roe, C. A., & Hitchman, G. A. (2011). Testing the theory of morphic resonance using recognition for Chinese symbols: a failure to replicate. *Journal of the Society for Psychical Research*, *75*(905), 211. (GS)

Bonilla, E. (2013). Distant mental influence on living organisms. *Investigacion clinica*, *54*(4), 427-454. (WoS, Sc)

Jockers, M. L. (2013). *Macroanalysis: Digital methods and literary history*. University of Illinois Press. (GS, WoS, Sc)

Rouleau, N., & Dotta, B. T. (2014). Electromagnetic fields as structure-function zeitgebers in biological systems: environmental orchestrations of morphogenesis and consciousness. *Frontiers in Integrative Neuroscience*, *8*, 84. (GS, WoS, Sc, PubMed)

Pereira, C. (2015). Quantum resonance & consciousness. *Journal of Consciousness Exploration & Research*, *6*(7). (GS)

Schorn, R. (2015). *Kollektive unbewusste Markenkenntnis*. Springer-Verlag. (GS)

Sheldrake, R., Smart, P., & Avraamides, L. (2015). Automated Tests for Telephone Telepathy Using Mobile Phones. *Explore: The Journal of Science and Healing*, *11*(4), 310-319. (GS)

Kos, J. L. (2016). *A Study of Energy Psychology and the Efficacy of Emotional Freedom Techniques in Trauma-focused Therapy* (Doctoral dissertation, California Southern University). (GS)

Takacsy, E. C. (2016). The embodied experience of The Knowing Field: focus on individual psychotherapy. (GS)

Sakel, K. L. (2019). *Age differences in religiousness and psychological well-being* (Doctoral dissertation, Bowling Green State University). (GS)

Leskowitz, E. (2020). A cartography of energy medicine: From subtle anatomy to energy physiology. *EXPLORE*. (GS, Sc)

Nolting, T. (2020). *Odyssee im 21. Jahrhundert: Über die Liebe als Quelle wahrer Zufriedenheit und Gesundheit im Leben*. tredition. (GS)

The second article, “So-called “formative causation” – a hypothesis disconfirmed” was according to Google Scholar (Gs) cited 16 times, according to Web of Science (WoS) 3 times, according to Scopus (Sc) 3 Times and according to PubMed 0 times. From these, double entries in different databases were fused (marked with short symbols) and double mentions in different languages omitted. The popular science books discussed above were also excluded. The entries are ordered chronologically and then alphabetically, to give an overview when the article was cited. The entries were furthermore divided into two categories: already quoting the first article, not quoting the first article.

Searches were done at the following dates: Google Scholar, Web of Science, Scopus and PubMed on the 06.02.2021

Entries quoting the first article as well:

Mikulecky, M. (1996). Sheldrake vs. Rose: chronobiology is the winner. In *Riv. Biol.-Biol. Forum* (Vol. 89, p. 469í477). (GS)

Robbins, K., & Roe, C. A. (2010). An empirical test of the theory of morphic resonance by using recognition for Chinese symbols. *Explore: The Journal of Science and Healing*, *6*(4), 256-262. (GS, WoS, Sc)

Roe, C. A., & Hitchman, G. A. (2011). Testing the theory of morphic resonance using recognition for Chinese symbols: a failure to replicate. *Journal of the Society for Psychical Research*, *75*(905), 211. (GS)

Jockers, M. L. (2013). *Macroanalysis: Digital methods and literary history*. University of Illinois Press. (GS, WoS, Sc)

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Sheldrake, R., Smart, P., & Avraamides, L. (2015). Rupert Sheldrake. *Explore: The Journal of Science and Healing*, *11*(4), 310-319. (GS)

Entries solely quoting the second article:

Roberts, B. L. H. (2013). *A Quantitative Examination of Ostensibly Extrasensory Experiences Occurring Spontaneously and in Laboratory Conditions* (Doctoral dissertation, Coventry University).

Fasce, A., & Picó, A. (2019). Conceptual foundations and validation of the pseudoscientific belief scale. *Applied Cognitive Psychology*, *33*(4), 617-628. (GS, Sc)

The third article, „Rose refuted" was according to Google Scholar (Gs) cited 13 times, according to Web of Science (WoS) 1 time, Scopus and PubMed could not find the article in their databases. From these, double entries in different databases were fused (marked with short symbols) and double mentions in different languages omitted. The popular science books discussed above were also excluded. The entries are ordered chronologically and then alphabetically, to give an overview when the article was cited.

Searches were done at the following dates: Google Scholar, Web of Science, Scopus and PubMed on the 06.02.2021

Mikulecky, M. (1996). Sheldrake vs. Rose: chronobiology is the winner. In *Riv. Biol.-Biol. Forum* (Vol. 89, p. 469-477). (GS)

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11 APPENDIX B: ABSTRACTS

Abstract

Knowledge has been a topic of study in science and technology studies (STS) nearly since its disciplinary beginnings. From the production of knowledge to the contestation of knowledge and to the circulation of knowledge. The simultaneous entanglement of all three of these instances of knowledge in motion has seldomly been studied. In this thesis, I take a case from the realm of experimental biology to showcase the entanglement of all three instances. The thesis revolves around the Sheldrake vs. Rose controversy, a controversy between two biologists concerning the testing of a controversial hypothesis made by Sheldrake. The case at hand follows Rose and Sheldrake in their controversy from the beginning in the newspaper The Guardian, over published articles in a peer-reviewed journal, to the seemingly end in the popular science books written by both actors. Through situational analysis applied to the documents produced by both authors, I come closer to how the controversy unfolded. The different media are seen as arenas in which both actors move and try to change the outcome of the controversy. At the heart of it lies a jointly executed experiment, designed to settle their arguments over whether Sheldrake's hypothesis of formative causation, which had been widely criticized by the scientific community, could be true or not. Rose had challenged Sheldrake to test it together, to have a crucial experiment decide between opposing sides. Since the experiment yielded an outcome deemed unsatisfactory by the actors involved, the controversy over the plausibility of the hypothesis was left open. The concepts of Collin's experimenter's regress, Gieryn's boundary work as well as Latour's theater of proof are used to show what was at stake in the controversy, how it moved as well as why it was seemingly left open. Through the analysis, it becomes apparent that both researchers have their own standards of good scientific practices, as well as philosophical foundations which are at odds with each other. Both actors have goals they want to achieve by specific means, which at times align (the experiment) and at times digress (the aftermath of the experiment). Through this thesis, insights are achieved into the different strategies used by researchers to solidify their claims (and undermine their opponents) as well as into the unusual instances where opposing factions try to work together to collectively create deciding factors over debated hypotheses.

Zusammenfassung

Wissen ist schon seit den disziplinären Anfängen der Fokus von Untersuchungen im Bereich der Science and Technology Studies (STS). Von der Entstehung von Wissen über das Anfechten von Wissen bis zum Zirkulieren von Wissen. Nur selten wurde die simultane Verstrickung aller drei Instanzen von Wissen in Bewegung untersucht. In dieser Arbeit zeige ich anhand eines Falles aus der experimentellen Biologie die Verstrickung aller drei Instanzen. Die Arbeit dreht sich um die Sheldrake vs. Rose Kontroverse, eine Kontroverse zwischen zwei Biologen über die Testung der umstrittenen Hypothese von Sheldrake. Der Fall folgt Rose und Sheldrake in ihrer Kontroverse von den Anfängen in der Zeitschrift The Guardian, über Artikel in einem peer-reviewed Journal bis zu dem scheinbaren Ende in den populär-wissenschaftlichen Büchern beider Akteure. Durch ‚situational analysis', angewandt auf die Dokumente produziert von beiden Autoren nähere ich mich der Art und Weise wie sich die Kontroverse entfaltete. Die unterschiedlichen Medien werden als Arenen wahrgenommen, in welchen beide Akteure sich bewegen und versuchen, den Ausgang der Kontroverse zu beeinflussen. Im Herzen der Kontroverse liegt das gemeinsam geplante Experiment, designet um die Auseinandersetzung der beiden darüber, ob Sheldrake's These der Formbildungsursachen, die von der wissenschaftlichen Gemeinschaft großflächig kritisiert wurde, wahr ist oder nicht. Rose hatte Sheldrake dazu herausgefordert die These gemeinsam zu testen, ein Entscheidungsexperiment sollte eine der beiden entgegengesetzten Seiten recht geben. Da der Ausgang des Experiments unzufriedenstellend für die involvierten Akteure war, blieb die Kontroverse über die Plausibilität der Hypothese offen. Collins Konzept des ‚experimenter's regress', Gieryn's ‚boundary work' und Latour's ‚theater of proof' wurden angewendet, um zu zeigen, worum es in der Kontroverse ging, wie sie sich entwickelte und warum sie scheinbar offenblieb. Durch die Analyse hat sich gezeigt, dass beide Forscher unterschiedliche Vorstellungen von Standards des guten wissenschaftlichen Arbeitens haben, als auch sich gegensätzliche philosophische Grundlagen. Beide Akteure haben Ziele, die sie mit bestimmten Mittel erhoffen zu erreichen, welche manchmal übereinstimmen (das Experiment) und manchmal nicht (die Nachwirkungen des Experiments). Durch diese Arbeit werden Einsichten gewonnen in die unterschiedlichen Strategien welche von Wissenschaftlern verwendet werden um die eigenen Forderungen zu verfestigen (und die der Gegner zu untergraben), als auch die ungewöhnlichen Fälle in denen gegensinnige Fraktionen versuchen zusammenzuarbeiten um zusammen Faktoren zu kreieren die über diskutierte Hypothesen entscheiden können.