Seeking the Controversies: Controversy, Pluralism and Knowledge in Psychoanalysis

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Abstract

This thesis offers an alternative understanding of the Controversial Discussions and reframes its stalemate by centring on the question of internal demarcation in psychoanalysis. The Controversial Discussions took place in London during the Second World War, in the aftermath of Sigmund Freud's death. It was an in-depth discussion concerning the validation, expansion, and status of psychoanalytic knowledge, wherein Melanie Klein's formulations were measured in relation to Freud's work, and challenged by a hostile audience captained by Freud's daughter, Anna. These two most prominent psychoanalysts of their time arguably stood for tradition versus change, and never reconciled their views.

This work investigates the similarities between the Controversial Discussions and scientific controversies, according to a sociology of associations and Actor Network Theory (ANT). In working systematically with defining aspects of scientific controversies, this thesis shows how it is possible to approach the dynamics of the Controversial Discussions using a knowledge production rationale. Investigating the criteria of internal demarcation, it is argued that the Controversies' participants relied mainly on phylogenetic and ontogenetic criteria to the validation of Klein's views. This is contextualised within the emergence of phylogenesis in psychoanalysis and is discussed according to its articulation in Freud's work on the theory of the Oedipus complex.

This thesis argues that Freud's use of phylogenesis was a relevant aspect of knowledge production in psychoanalysis, since it is an important aspect for psychoanalytic truth to be produced as fact. Thus, this work considers the use of biology in the Controversial Discussions as a criterion of internal demarcation between accepted views in psychoanalysis and those to be regarded as outside the psychoanalytic realm. Drawing on this process of knowledge production, this thesis argues that psychoanalysts should think about their differences by considering their conflicting views, the need for coherence, and a sense of crisis that ignores the constitutive process of facts in psychoanalysis.

Connecting phylogenesis with knowledge production and biology, this thesis offers new understandings of old problems where clashes among different schools of thought need no longer be entrenched, where psychoanalytic truth as fact may be revisited, and where the stalemate at the end of the Controversial Discussions can inform the question of psychoanalytic plurality.

I hereby declare that, except where explicit attribution is made, the work presented in this thesis is entirely my own.

Felipe Massao Kuzuhara

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I dedicate this thesis to Theo, Pedro, and Nuno. They showed me the urgency of time passing, which I learned either by playing with them or remaining alone working on this text. To my parents, I cannot appreciate enough their essential support which I always carry with me.

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for one who'd lived among enemies so long:

if often he was wrong and, at times, absurd,

to us he is no more a person

now but a whole climate of opinion

(W. H. Auden, *In Memory of Sigmund Freud*)

I don't want to persuade the reader that it's a real thing; I want to show it as it is. In a sense, I'm telling those readers that it's just a story—it's fake. But when you experience the fake as real, it can be real. It's not easy to explain.

(Haruki Murakami, interview with *Paris Review*)

Chapter 1: 'I should like to point out that there is an air raid going on…'

1. Introduction

'A noisy evening with bombs dropping every few minutes and people ducking as each crash came. In the middle of the discussions someone I later came to know as D.W. stood up and said, "I should like to point out that there is an air raid going on", and sat down. No notice was taken, and the meeting went on as before! (Little 1985 p 19)'.

Above is Margaret Little's account of one of the meetings of the British Psycho-Analytical Society. Her initial impressions became the stuff of legend, given that D.W.'s intervention is now told as an unverified anecdote (Gabbard and Scarfone 2002). Actually, D.W. stands for Donald Woods – the forenames of a then little-known Winnicott – who, years later, would embark on his own journey to psychoanalytic renown. His intervention that night was indeed a short one, but nonetheless tapped right into the surreal nature of an absorbing psychoanalytical controversy held to the soundtrack of crashing German bombs. This scene, in this sense, has been compared in the psychoanalytic milieu to spellbound 'theologians of Byzantium discussing the sex of the angels while the invaders are at the gate of the city' (Green 2001 p 26).

Whether portrayed as per Little's words, as debate, as legend, or as a discussion among theologians, all these references hark back to one single event in the history of psychoanalysis: the Controversial Discussions. Formally speaking, the Controversial Discussions was a special forum made up of a series of ten Scientific Meetings, to decide on the psychoanalytical compatibility between Melanie Klein's new, and Sigmund Freud's more established, notions. Yet, what was 'it' that made psychoanalysts oblivious even to D.W.'s reminder of the Blitz? References to the Controversial Discussions point to a myriad of threads: 'the hatred of a daughter towards her celebrated mother' (Ibid. p 27); a forum turned into 'a coliseum to the great detriment of the life of the mind and the health of the body' (Bollas 1993 p 815); a discussion between Anna Freud and Melanie Klein (Bergmann 1997); the process of mourning for Sigmund Freud and its effects some years later in the dispute over his legacy; the accommodation of psychoanalytic immigrants fleeing the Nazi occupation of Europe (Steiner 2000b); a resistance to accepting the meaning of death as a deeper layer in the psyche heralded by Klein (Rose 1993); questioning of the training programme (Bergmann

2004); a theatrical drama (Wright 1995) and so on. All these threads reflect the complexity of the situation of the psychoanalytic scene in London. Yet none of the themes above address what lies at the core of the Controversial Discussions: a debate established by its participants, in what they regarded as scientific, in order to evaluate Melanie Klein's psychoanalytic notions. The aim of this thesis is to explore this gap in the debate and eventually rethink psychoanalytic controversy according to a perspective informed by the terms 'science'[1] and 'knowledge'. This missing aspect could help to account for the Controversial Discussions' 'spellbinding' effect.

To confront this gap, I approach the issue of how knowledge is constituted in psychoanalysis. The overarching question guiding this research is how psychoanalysis demarcates itself internally, separating the concepts regarded as valid knowledge of the mind from those it rejects as invalid. This question is fundamentally informed by the Controversial Discussions as a key site, and by the way the term 'science' is drawn on to produce this internal demarcation.

2. Defining the scope of the thesis

Since the Controversial Discussions, Freud's views on psychoanalysis and science have been problematised by various critiques, and notably within philosophy of science. A central point of debate arises from Karl Popper and Adolf Grunbaum, who draw on different philosophical traditions to pose the same question: 'is psychoanalysis a science?' (e.g. Popper 1969; Grunbaum 1984). This question has driven philosophers to evaluate psychoanalysis against an ideal or normative stance, wherein the scientific status of psychoanalysis has traditionally been seen as the major point of debate. Both Popper and Grunbaum invalidate psychoanalytic ambitions in this regard; however, denying psychoanalysis a scientific status has also generated some ongoing debate (e.g. Hopkins 2014; Boag et al 2015). More recently, Isabelle Stengers (1997) has joined this debate on scientific demarcation and, while she offers a more nuanced view and a different range of considerations, she too fails to grant psychoanalysis a scientific status.

The three philosophers mentioned above share in common an interest in the demarcation of psychoanalysis as a whole. Consequently, they all aim at assessing the status of psychoanalysis as a scientific discipline. Such an approach is not central and, maybe, only marginally useful to this thesis as it is concerned with a problem of a different nature, and involving a different kind of demarcation. My research question considers the problem faced by Melanie Klein as she attempted to add to the existing psychoanalytic corpus of valid knowledge of the mind, established mainly by Freud. So my

[1] See glossary.

use of the term demarcation[2] is narrower in scope, and does not necessarily put the (un)scientific status of the entire psychoanalytic conceptual apparatus into question. The difference in scope can be justified by a different originating problem to be addressed. This thesis is concerned with the process of *internal* demarcation, which I define not as an attempt to demarcate the scientific status of psychoanalysis but rather as the attempt to validate or reject additional claims within a field. This thesis focuses on Klein's notion of the depressive position[3] as an example of a claim in need of internal demarcation, especially as it stood against the established status of the Oedipus complex, already regarded as a 'fact' by the time of the Controversial Discussions. Unless explicitly used in a specific context, the term demarcation should be considered along these lines, concerned with knowledge of the mind, and according to the criteria developed internally in psychoanalysis to validate and integrate its findings.

3. A rationale for this study

There are several schools of psychoanalytical thinking today, such as 'Kleinians', 'Freudians', 'Independents', 'Lacanians', and so on. This fragmentation of the field has preoccupied psychoanalysts, who have discussed it in terms of a 'crisis of understanding', 'hopeless divisions', and 'pluralism' (Wallerstein 1988, Green 2004). In 1987, the issue of fragmentation was even put forward by the president of the International Psychoanalytic Association (IPA), Robert Wallerstein, as the main theme guiding that year's annual IPA conference. It was a theme offered to be potentially reflected upon by many psychoanalysts, since divisions were a marked feature of psychoanalysis as it was practiced in different countries (e.g. Kleinians, Contemporary Freudians, and Independents in the UK; Freudians and Lacanians in France; Kleinians, Bionians, and Freudians in Brazil).

My interest in the Controversial Discussions relates to this question of fragmentation. While I refer in this thesis mainly to Wallerstein's official address at the 1987 IPA conference, and subsequent exchanges between him and Andre Green (2002, 2004) to anchor this issue here, I have been interested in the problem of fragmentation as it has pervaded my journey as someone in training in psychoanalysis, who was perplexed by these different schools, by what looks like a veritable confusion of tongues, by heated debates and external inter-group animosity. Which direction to choose? Why all these divisions? Why can't psychoanalysts agree more? Why all this need to find

[2] See glossary.

[3] The depressive position is defined by Melanie Klein as a mental constellation, which is normally first experienced in the middle of the first year of life. It involves unconscious feelings such as anxiety, and the confluence of love and hatred towards an object.

one's sense of identity, showing superiority of one's choice to the detriment of others'? Informed by this experience, I look at the Controversial Discussions not simply as an important event in British psychoanalytic history but as a critical moment marking the very beginning of fragmentation at an institutional level, where post-Freudian psychoanalysis began to fragment from within. While my research question on internal validation is focused on the Controversies, it is also my intention that my study resonates with contemporary psychoanalysis. By offering a new angle on the Controversial Discussions, I am also considering the role of knowledge production[4] in psychoanalytic divisions today. Since I argue that psychoanalytic fragmentation is connected with how psychoanalysis produces its knowledge, it is not only an internal and institutional matter, but reflects the very constitution of psychoanalytic truths, facts, and knowledge.

4. Theoretical framework and the criterion for internal demarcation

While philosophy of science approaches psychoanalysis according to a normative perspective on its scientific status, a more recent sociological field of investigation invites a different question. Instead of focusing on the debate in terms of whether or not psychoanalysis is a science, sociologists of science have dislodged the centrality of such a question and have instead asked how psychoanalysis produces its knowledge (Knorr Cetina 1992; Krause and Guggenheim 2013). Such a question calls for an alternative kind of emphasis. A move away from a focus on scientific status and towards an analysis of knowledge production is a foundational element of the sociological strand pursued by Science and Technology Studies (STS) since the 1970s, with important developments provided by Actor-Network Theory (ANT).

STS has established a connection with psychoanalysis by comparing the couch to the laboratory as a site of knowledge production (Knorr Cetina 1992; Krause and Guggenheim 2013) 'designed to produce placelessness' (Krause and Guggenheim 2013 p 188). STS tenets have been applied to psychoanalysis based on such shared features, allowing an investigation of psychoanalytic knowledge production at the setting, and according to the clinical methods devised by psychoanalysts.

While the laboratory has traditionally been the site of STS research, ever since the foundational studies of the discipline (Collins 1985; Shapin and Schaffer 1985; Latour and Woolgar 1986), scientific controversies have also been a subject for STS examinations. Indeed a substantial number of STS studies rely on scientific controversies as the primary locus of investigation (e.g. Bloor 1976;

[4] See glossary.

Latour 1988). In their postscript to *Laboratory Life: the Construction of Scientific Facts* (1986), for example, Latour and Woolgar consider that 'the laboratory should not be studied as an isolated unit (…) the full story will establish that there is a continuum between controversies in daily life and those occurring in the laboratory' (p 281). Various authors (e.g. Knorr Cetina 1992; Latour 1988; Stengers 1997) explicitly point to a chained process involving different actors in the production of knowledge. A good example is Bruno Latour's *The Pasteurization of France* (1988), a study connecting a laboratory (Pasteur's) with a controversy (against Pouchet[5]), and within a much larger process involving other actors and sites in nineteenth-century France that together established the principles of vaccination and microbiology (Latour 1988, 1999b). This example shows that knowledge production involves processes and aspects of scientific activity beyond the laboratory. Given the couch-laboratory analogy, this thesis will argue that the Controversial Discussions provides grounds for pointing out important aspects of knowledge production in psychoanalysis beyond the couch and clinical technique – and beyond the scope of debate so far established in STS.

According to the STS and ANT literature, scientific controversies are specific situations where scientists disagree and clash. Controversies are regarded as situated events, and in this context STS and ANT are concerned with the status of new claims under dispute. In this sense, both STS and ANT are suitable to address the question of internal demarcation at the Controversial Discussions since they attempt to address a specific type of demarcation, where new claims are held against a previously existent body of knowledge, and whether or not these claims should be incorporated as validated facts. This is how I have chosen to approach the Freud-Klein tension at the Controversial Discussions.

The criterion of internal demarcation for a scientific controversy in both STS and ANT is the production of a scientific fact[6] because, as the outcome of a process of knowledge production, this event has the capacity to stop debate and clashes among scientists. Along these lines, the event of a claim being turned into a fact is then what demarcates accepted from rejected notions. Authors like Collins (1985), Bloor (1976) and Latour (1987) all conceive the establishment of fact as the key

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[5] Félix-Archimède Pouchet (1800-72) was a French naturalist and a proponent of spontaneous generation of life. He was an opponent of Louis Pasteur's germ theory, as they engage in scientific controversy in France, mainly in the 1860s.

[6] See glossary.

event[7] in connection with knowledge production in scientific activity, which allows internal demarcation and therefore ends controversies.[8]

Within ANT, inanimate objects and concepts are also incorporated into the model as 'actors', and while there is a network of actors to be considered in this thesis, I am mainly interested in following one: Klein's depressive position, in its struggle to become a fact. Melanie Klein's concepts such as unconscious phantasy, projection-introjection mechanisms, regression, and the depressive position were in full display at the Controversial Discussions. It has already been established that Klein's contributions offered a new psychoanalytic perspective at the Controversial Discussions (e.g. Segal 1979; Wallerstein 1988; Bergmann 2004). However, with an ANT framework, the investigation stresses that new claims at a controversy are addressed in terms of how such notions gain credibility, through a network of actors, so that they are eventually taken as 'black boxed'[9] – as a fact.

I follow Klein's notion of the depressive position in order to understand how it could become established as a fact, and how it can provide an understanding of controversies, fragmentation, and pluralism in contemporary psychoanalysis. For the discussion here, I take Klein's notions as integrated and centred around her notion of depressive position. In other words, any claim about the factual status of any aspect of Klein's theory (unconscious phantasy, projection-introjection mechanisms, regression) is taken as underpinning the depressive position as an umbrella notion.[10] This focus on the depressive position as both a 'fact' and as an umbrella notion parallels the framing of the Oedipus complex during the Controversies. While this notion also has many associated concepts (e.g. the unconscious, sexuality, desire, repression, denial, and so on), it was arguably used during the debate as a template for a psychoanalytic fact. My interest, then, is to follow and compare the fate of Klein's depressive position in relation to this usage of the Oedipus complex.

5. The Controversial Discussions as a two-way opportunity case

The proceedings of the Controversies were formally established to accept or reject Klein's alleged contributions on 'the development of the psyche from infancy to the close of the Oedipus phase – roughly the first five years' (King and Steiner 1991 p 212), with special attention to the first year of

[7] Curiously, the logic of event is very similar to the psychoanalytic logic of temporality, and *après-coup* (e.g. Perelberg (2006). See also Deleuze's influence on Latour (Schmidgen (2014)).

[8] This is a consensus among Collins, Latour, and Bloor. But they have different understandings of what a fact is. In ANT for example, it is a highly conceptualised notion. See for example Latour's account of fetishism of fact ('*un fait est fait*') (Latour 2010). See also the Theoretical Framework in Chapter 3.

[9] See glossary.

[10] Or in ANT terms as an obligatory passage point. See Chapter 8 for the meaning of this term. See also glossary.

life.[11] This took place at a moment of transition in psychoanalysis, one that involved the most prominent living psychoanalysts of the time. After four years of dispute, the Controversial Discussions resulted in a truce – a 'gentlemen's agreement' – involving mainly Melanie Klein, Anna Freud, and their respective followers. So, in spite of a lack of resolution, psychoanalysts remained within one and the same institution in the UK. This 'gentlemen's agreement' can be understood as a token of the British compromising spirit (Kohon 1986; Hinshelwood 1995). But this truce can also indicate both sides' failure to resolve what were originally deemed to be, according to their own views, scientific differences, or to maintain a unitary understanding of psychoanalytic knowledge. Furthermore, heralding a so-called compromising spirit understates the ruthless confrontation, brutal attacks, exasperation, and lack of emotional control among psychoanalysts that characterised the Controversial Discussions (Hinshelwood 1995). Even more crucially, it fails to convey the sense of a psychoanalytic society on the verge of dissolution and fragmentation (Steiner 2000b p 6). Therefore, the 'gentlemen's agreement' may stand for the projection of a necessary and 'natural' solution to psychoanalytic dilemmas of the time, given that it mirrors in the past that which exists and has been accommodated in the present: a division between Kleinian, Freudian, and Independent psychoanalytic groups in Britain. For example, it is only since 2005 that training programmes at the British Psychoanalytical Society are no longer segregated according to these three psychoanalytic orientations.

It is important to remark that this thesis relies on texts as its primary sources of argument since its main object of study is a psychoanalytic controversy that took place during the Second World War. The main reference for the study here is King and Steiner's *The Freud-Klein Controversy 1941-45* (1991). This book is considered by some to be 'the most important work ever achieved in the history of psychoanalysis' (Green 2001 p 26), and has been used as a major reference for every recent significant study on the matter (e.g. Bergmann 2004; Steiner 2000a, 2000b). Through the material contained in *The Freud-Klein Controversy 1941-45* (King and Steiner 1991), the Controversial Discussions have been studied as a conundrum, a coliseum, an example of alienation, and a dramatic struggle for the survival of institutional psychoanalysis. While there were previous figures of dissent in psychoanalysis – Reik, Adler, Rank and especially Jung (Bergmann 2004; Gay 2006; Makari 2008) – the Controversial Discussions are unique in that they took place in the aftermath of Freud's death, from which time no debate could resort to the weight of his opinion. Freud's absence also coincided with the collapse of Vienna, Budapest, and Berlin at the hands of the Nazis, so that London would

[11] This is a central aspect of the formal programme at the Controversies, drafted by Marjorie Brierley and ratified by other participants at a meeting held on 21 October 1942. Brierley's programme was accepted as a guiding plan to structure following meetings and discussion, when controversy took place. The actual development of the Controversial Discussions did not necessarily follow this programme or was only centred in some aspects of it as the debate unfolded.

become the last relevant psychoanalytic centre in Europe (Makari 2008 p 468). Furthermore, given the wide range of issues (institutional, educational, scientific, generational, etc.) in need of resolution (Kohon 1986), the Controversies can arguably be regarded as the setting of important pathways within modern psychoanalysis, at least in the Anglophone world.

Many, if not all, psychoanalysts in 1940s London seemed to place great faith in what they called science to guide the discussion on what constituted knowledge in psychoanalysis and to resolve their differences. Yet again, it is minor voices witnessing the Controversies that help to gauge the depths of this faith. According to Bollas (1993):

> In the very first meeting Karin Stephen speaks the unmentionable: 'Do we really know what we are doing' she asks, referring to the practice of psychoanalysis, uttered in the presence of the two most powerful female analytic thinkers in the world who most definitely knew what psychoanalysis was meant to do and in exactly what way. (…) At the next meeting Kate Friedlander candidly states that 'our scientific activities are severely hampered by this undercurrent of hostility between the various groups in the Society' (p 810).

From the material provided by King and Steiner (1991), it is evident that the participants' pursuit of scientific standards was often invoked without further ado (Steiner 2000b). Friedlander's comment on 'our scientific activities' was one of several recurrent references to a supposedly unproblematic connection to what participants regarded as science in psychoanalysis at that time, even if in clear contrast to Karin Stephen's moot intervention. Pearl King, co-editor of *The Freud-Klein Controversy 1941-45,* writes in the book's opening paragraph: 'overtly, controversy was mainly couched in terms of scientific differences of opinion about what was considered to be accepted psychoanalytical theory and technique' (1991 p 9). Furthermore, Susan Isaacs quoted from the IPA statutes: 'let me remind you of Statute III: "The aim of the Association is the cultivation and furtherance of the psychoanalytical branch of science founded by Freud"' (p 57). Indeed, the Controversial Discussions were, at the time, called Scientific Meetings – with four original papers presented, followed by several rounds of discussion, spread over ten meetings between January 1943 and May 1944. Also, the psychoanalytic debate was imbued with a sense of mission, pursued according to what was then regarded as a scientific outlook (e.g. p 49, p 52, p 59) whose aim was 'the truth, the whole truth, and nothing but the truth' (p 49), and at the frontier of knowledge ('an unrestricted search for progress in our scientific work' (p 90)). In addition to this, Winnicott spelled out a methodology at the Controversies according to 'piecemeal objective observation; construction and testing of theory based on observed facts, and imaginative reaching out in front of accredited theory' (p 87). So psychoanalysts arguably shared what they regarded as a scientific methodology, an ethos, and a sense of mission towards the furtherance of psychoanalytic knowledge. In other words, they would have agreed that their dispute was, as Brierley said, 'essentially a scientific problem, and we must insist on a scientific resolution' (p 174).

By definition, scientific controversies are situations of uncertainty wherein the usual parameters of scientific knowledge cannot be taken for granted (Bloor 2015). Instead, they are situations of great strain and so become open to investigation (Latour 2005). In this sense, controversies could be seen as erupting 'hot spots', with STS and ANT methodologically exploring the 'punching' of a system (Pinch 2015), so that knowledge production from scientific activities may be investigated through the cracks opened by destabilization. In this way, the Controversial Discussions can become a site of investigation, a site that offers an opportunity for evaluating aspects involved in the process of knowledge production in psychoanalysis. On the one hand, I use the ideas of knowledge production and internal demarcation to make sense of the Controversial Discussions. On the other, the Controversies offer an opportunity; a moment of openness where aspects of knowledge production can be articulated, followed, and better understood. This is mirrored in my investigation of *Totem and Taboo,* where several aspects of knowledge production were originally articulated in pursuit of establishing the Oedipus complex, and later imported directly into the Controversies.

6. Final introductory comments

Considering the above, there are a range of questions that will be part of my investigation here. I want to find answers for why the term 'science' and associated notions remain bracketed out of accounts of the Controversial Discussions. I want to investigate if a new angle to the Controversies can be carved out through the issue of internal demarcation. These questions lead me to pose more questions: What does an ANT framework bring in terms of analytical tools? What are the limitations of these tools in relation to psychoanalysis? Furthermore, how exactly did psychoanalysts demarcate their claims internally? What are the central aspects in the process of knowledge production at stake at the Controversies? What is more, how does a fact become established in this context, and what does it say about the status of truth as facts in psychoanalysis? Finally, can the Controversial Discussions, knowledge production, and the nature of truth as facts help to make sense of the fragmentation of psychoanalysis? These are the questions I am confronting myself with, through this thesis.

Chapter 2: Literature Review

1. Introduction

This chapter examines how an understanding of the Controversial Discussions has taken shape within the literature on psychoanalytic controversies. A full survey of psychoanalytic controversies, their immediate background, and their different social-historical contexts fall beyond the scope of this thesis. Instead, this chapter focuses on post-Freudian psychoanalytic controversies in order to situate the Controversial Discussions.

The literature review reveals a range of elucidations on the dynamics of dissension and confrontation in key controversies within psychoanalysis, but none consider the psychoanalytic efforts and project to produce knowledge in reference to scientific terms. Furthermore, the literature on psychoanalytic controversies fails to consider the Controversial Discussions together with the available literature concerning psychoanalysis and science according to a comprehensive framework. Thus, the existing literature considers the problem of internal demarcation in psychoanalysis to be mainly a matter of power struggles, unresolved psychological issues and a conflict of ideas, rather than the production of knowledge. In order to explore this gap, the second half of this chapter reviews the current literature on scientific demarcation and psychoanalysis to provide a backdrop for a more systematic discussion on the subject of internal demarcation that is established through the Controversial Discussions.

2. Dynamics of dissidence and controversy

In 2000, Martin Bergmann chaired a two-day conference in New York on dissidence in psychoanalytic history. The participants included high-profile psychoanalysts such as Andre Green, Otto Kernberg and Robert Wallerstein, and the symposium proceedings were later edited into a book called *Understanding Dissidence and Controversy in the History of Psychoanalysis* (2004). In his opening text, Bergmann provides an extensive backdrop to the topic and claims that dissidence should primarily be regarded as 'a battle of ideas, leaving aside the social aspects of these controversies' (p 1). Bergmann justifies his emphasis on ideas, as opposed to social contexts, by the fact that controversies occur throughout time and in different socio-historical contexts. As he notes, 'as time goes on, (…) sociology will be of interest only to historians, whereas controversy and dissidence as a history of ideas is in my view of more enduring interest' (p 2). In terms of opposing ideas, for example, Bergmann lists Adler's notions of the 'safeguarding tendency' and 'masculine protest' (p 10), Jung's 'collective unconscious and archetypes' which deemphasise the role of sexuality in neurosis (p 19), Rank's 'trauma of birth' (p 23), Horney's views on feminine psychology (p 39), Klein's work with children and emphasis on phantasy and aggression, Fromm's political ideology (p 60), and Lacan's emphasis on language, desire, and *jouissance* (pp 64-72). According to Bergmann, these challenges clashed with various aspects of Freud's theory in its different stages of development. The dissidents' challenges confronted, for instance, Freud's emphasis on the ego's fear of libido, his emphasis on sexuality and the Oedipus complex, the notion of narcissism, or his emphasis on a drive theory.

According to Bergmann, within the classical-Freudian period, 'psychoanalysis had discovered a truth many people find difficult to accept, namely that sexuality does not begin in adolescence, but during infancy, and that infantile sexuality culminates in the Oedipus complex' (p 4). Freud saw this resistance to psychoanalytic 'truth' as a mark of dissidence, and equated it to a 'pathological phenomenon based on resistance or relapse from a painful truth' (p 88). However, this position was unsustainable once psychoanalysis started to fragment. Consequently, the very capacity to dissent within psychoanalysis becomes a problem with the transition to a postclassical period, as 'psychoanalysis became divided into different schools, [so] to continue to maintain that the other school is the resisting one became untenable' (pp 4-5).

Taking stock of the development of ideas in psychoanalysis, Bergmann marks 1945 as a watershed, with the Controversial Discussions sitting exactly at this moment of transition between the classical and postclassical eras (pp 53-59). 'Did psychoanalysts wait for Freud to die to let all these controversial issues come into the open?' (p 56). With this interrogation, Bergmann refers to the numerous disputes during the Controversial Discussions such as Klein's endorsement of children being analysed through symbolic play, the presence of early phantasy life in children, and the role of aggression in mental illness (p 56). Bergmann regards Melanie Klein as opening a new chapter in psychoanalysis because she inaugurates internal fragmentation without dissidence from psychoanalysis, and therefore paves the way for the division into different schools of thought.

Without a sense of agreement around what constitutes valid knowledge, psychoanalytic aspirations to being considered a natural science become problematic. On the one hand, Bergmann notices that 'Freud (1912) and his followers firmly believed that psychoanalysis would apply the techniques of natural science to the interiority of men' (p 2). On the other, the very disagreements between different 'psychoanalytic schools were not those encountered in the natural sciences' (ibid.). So Bergmann laments that with the establishment of different schools of thought in psychoanalysis, 'we cannot claim in good conscience to be entirely in the realm of science. Psychoanalysis with all its schools may be on a road that will justify its claim to be a science but at the moment we are not at that point yet' (p 95). For him:

> Controversies of course occur in the natural sciences, but they are by their very nature temporary. Either a crucial experiment puts an end to them or further knowledge accumulates, leading to a decision, whereas the question of which of the psychoanalytic schools achieves the aim of mental cure most effectively is fought out in the form of public opinion, where the skill of presentation and literary appeal count for more than the presentation of scientific data' (p 94).

For Bergmann, psychoanalytic fragmentation is embarrassing (p 86). He concludes that dissension is not simply due to the existence of competing ideas: 'it is not the differences with Freud that determined dissidence. It is the basic attitude of gratitude or criticism (…) It was this work [*on dissidence and controversy*] that led me to the realization that an incomplete or unsuccessful analysis plays an important role in dissidence' (p 89). So Bergmann develops a psychological argument as the fundamental reason for understanding controversies and dissension in psychoanalysis.

If Bergmann (2004) deserves credit for establishing an arena of discussion for controversy and dissension in psychoanalysis, it is also important to point to other aspects of his assessment. A main point of contention would be his reliance on his psychological argument to justify the dynamics of psychoanalytic dissension. Furthermore, although he often invokes the term science in his text, and natural sciences are used as a symbol of unifying truth in psychoanalysis, Bergmann's views on science are neither rendered explicit nor used to illuminate dissidence and controversies in psychoanalysis.

3. Calling for better debates

An alternative perspective has been provided by Bernardi (2002), Widlocher (2008), and Barros (2013). Like Bergmann (2002), these authors are concerned with psychoanalytic controversies but focus their analyses solely on the postclassical period. Bernardi (2002), for example, studies the debates on Klein and Lacan in the Río de la Plata in 1972. His point of departure is the existence of different schools of thought, with a focus on the conditions for a true debate amongst them. Widlocher also looked at the question of a better debate when he edited the book *Les Psychanalystes Savent-Ils Débattre?* (2008). This book studies four post-Freud controversies with the aim of establishing a proper methodology for debate in psychoanalysis (p 200). Reflecting upon Widlocher's book, Barros (2013) also emphasizes the importance of sound debates in psychoanalysis while pointing out the conditions for fulfilling them.

Combining psychoanalytic fragmentation with controversy, these authors look at the shape of controversies as the way forward in dealing with diversity in psychoanalysis. Bernardi (2002) considers that 'a true debate implies the construction of a shared argumentative field that makes it possible to lay out the different positions and see some interaction between them and is guided by the search for the best argument' (p 851). His analysis emphasizes that a true debate, even if no agreement can be reached, helps develop psychoanalysis by promoting the examination of different theories. Put differently, Bernardi (2002) is less concerned with 'what' is discussed than 'how' psychoanalysts debate their ideas, as a preliminary step towards reaching potential consensus. For example, considering the debate in the Río de La Plata, he writes that 'I would now like to comment on these dialogues, emphasizing not so much the content of the discussion but the way of arguing and especially those aspects that facilitated or made it difficult for the two hypotheses to be examined in depth and on equal footing' (p 862). So, drawing on a theory of argumentation, he establishes that:

> This path for resolving discrepancies implies a number of steps: 1) identify the disagreements between the two parties; 2) establish agreements regarding the means by which the disagreement can be settled; 3) allow indefinite exploration of the merits of each position; which culminates in step 4) reach agreement, or mutually recognize that it is not possible to achieve one for the time being.
>
> In my opinion, the agreement provided for in point 2) (…) is often lacking in psychoanalytical discussions, with the problem never becoming a topic for discussion' (p 856).

Drawing on these steps, Bernardi concludes that there was no true examination of ideas there because 'each party took for granted the intrinsic superiority of certain concepts over others, with no possible recourse to the source of this evidence' (p 863). As a result, psychoanalysts from the Kleinian and Lacanian camps would rely on philosophical ingenuity (intentionality and unconscious phantasy on one side, division of the subject on the other), leading to a discussion based on alternative metaphors (p 863). Consequently, no systematic confrontation could take place. To counter such outcomes, Bernardi asserts that empirical sources must be provided and a shared language must be developed (pp 867-68).

Widlocher (2008) looks at the issue of debate in psychoanalysis by revisiting four controversies, all of them in the postclassical period: the Controversial Discussions, the Debates on Melanie Klein and Jacques Lacan in the Río de la Plata, one between Laplanche, Widlocher and Fonagy on Freud and Bowlby, and one between Widlocher and Miller on Lacan. None of these debates had a winner, and Barros (2013) notes that the main conclusion of these impasses was that psychoanalysts urgently needed to formulate a methodology for debate (Delattre 2008 p 200).

These three authors' focus on debating methodology reflects their postclassical standpoint. Barros (2013 p 89), Widlocher (2008 p 11) and Bernardi (2002 p 860) take pluralism and fragmentation in psychoanalysis as a given, and take for granted the impossibility of a unified approach to the unconscious. They focus instead on improving the debate, so their aim becomes a gain in clarity and articulation of psychoanalytic concepts (Bernardi 2002 p 869; Widlocher 2008 pp 109-110; Barros 2013 pp 84-85). Thus, the word 'science' is set aside, but not necessarily forgotten. Bernardi (2002), for example, writes that 'though not always explicitly, examining argumentative discourses reveals that what is being discussed, too, is each party's way of conceiving the rationality and scientific nature of psychoanalysis' (p 869). In fact, the words 'science' and 'scientific' – in relation to psychoanalysis – pervade his text. Similarly, Widlocher regards psychoanalytic fragmentation as the result of the diversity of models within human sciences (2008 p 20). For him, psychoanalysis is not a technique of observation but rather a modality of intersubjective communication leading to hypotheses and models (p 17). Even so, Widlocher keeps on referring to the term 'science', albeit only through a diffuse and implicit notion of what science is.[12]

Barratt (1988), on the other hand, attempts to provide an overarching approach to psychoanalytic controversies, rather than any particular controversy, in what he calls a 'post-modern' era (p 238). Drawing on Lacan, he criticizes the traditional psychoanalytic reliance on a misplaced '"hermeneutic validity" (…) on the basis of the prevailing notions of "science", "truth", and "reality"' (p 226). For Barratt, psychoanalysis should instead offer an alternative to the 'conventional notion of what is and is not scientific' (p 228), challenging the assumption of a rational scientific consciousness, based on a 'unitary or univocal subject capable of apprehending the world' (p 236). But while Barratt questions traditional scientific values of truth, and even empirical verification, he does not provide any concrete alternatives apart from an abstract revolutionary notion of science.

Indeed, these authors do not offer more than principles or guidelines for discussion. For example, they do not provide an answer to how exactly to address the term 'science', the status of

[12] See for example, p 11, p 13, p 15, pp 17-20, pp 24-29, and p 201, in Widlocher (2008).

psychoanalytic knowledge or even how exactly to handle the use of clinical cases in contemporary debates. Yet the term 'science' insists on reappearing in this literature, either as a term evoked without being fully developed, or as a notion to be overcome, as in Barratt (1988). This is part of what this thesis aims to do with the case of the Controversies: not to call for principles, but to provide a structured argument articulating these terms in a consistent manner. Furthermore, instead of taking fragmentation and pluralism as a starting point, I am interested in casting light on 'better debates' by understanding how fragmentation became established in the first place. With this thesis, I will regard pluralism and fragmentation as an implication of internal demarcation outcomes. I believe that a 'call for better debate' among different schools of thought needs to be informed by a better case for how pluralism came to be in the first place.

4. The Controversial Discussions

The Controversial Discussions are situated within the history of psychoanalysis in the UK in relation to four topics: autocracy, Melanie Klein's arrival, child analysis, and the Freud family in London.

a.) Autocracy

In 1942, when the Controversial Discussions took place, Ernest Jones was president of the British Psycho-Analytical Society. He was also the founder of the Society, had been its president since its inception in 1919, and was a close associate of Freud. Yet, by the time the Controversies ended in 1945, he was in semi-retirement (Roazen 2002) and was no longer the president of the British Society. He was replaced by Sylvia Payne, rather than by Edward Glover, his second-in-command and the person he had envisaged would replace him (King and Steiner 1991).

The beginning of psychoanalysis in the UK is entangled with Ernest Jones' own trajectory. After reading Freud's *Studies on Hysteria* (1893-1895), and impressed by Freud's clinical case 'Dora', Jones set out in 1908 to meet Freud personally at the first Psycho-Analytical Congress in Salzburg (King 1979). Originally a physician and a neurologist, Jones managed to establish the British Psycho-Analytical Society in the UK in 1919 after some false starts (Ibid.). Having founded the British Society, Jones was also instrumental in establishing the UK's psychoanalytic institutional life. He took a lead role in establishing the *International Journal of Psycho-Analysis* in 1920, founded the Institute of Psycho-Analysis in 1924 – the institutional body responsible for overseeing the training of new candidates – as well as the London Clinic of Psycho-Analysis in 1926 (Kohon 1986). It was also during that time that Jones, with great help from Glover, managed to obtain official recognition for psychoanalysis from the British Medical Association as a 'serious branch of science' (King and Steiner

1991 p 12), so that psychoanalysts could be recognised as a professional group, and be 'treated in much the same way as any other specialism in medicine' (p 13).

Jones' leadership stemmed from his position as the only president of the British Psycho-Analytical Society between 1919 and 1944. So his role arose out of his position within that institution, his international presence in the psychoanalytic community, and his proximity to Freud in the psychoanalytic movement – not to mention that Jones was directly responsible for saving Freud and his family from Nazi hands in Austria, bringing them safely to England. Yet Jones's autocratic position was being questioned even before the outbreak of war, as members of the British Society were growing in discontent, seeking to limit the tenure and multiplicity of offices within the UK's psychoanalytical institutional life (Grosskurth 1986 pp 284-5; King and Steiner 1991 p 33). In other words, psychoanalysts were eager to establish more democratic rules for their institutions. According to Grosskurth (1986), this discontent was one of reasons why psychoanalysts called an Extraordinary Business Meeting in February 1942, the occasion wherein the Controversies formally began. It was also halfway through the Controversial Discussions that Jones departed to the countryside in semi-retirement, for reasons not entirely clear (e.g. Gillespie 1979; Grosskurth 1986; King and Steiner 1991; Roazen 2000), in effect leaving Edward Glover to run the Society (Grosskurth 1991 p 281). Thus, during the most relevant meetings of the Controversial Discussions, Jones remained absent while Glover acted as one of the main opponents to Melanie Klein's ideas. Glover's institutional importance can be gauged by the offices he held at that time: scientific secretary of the British Psycho-Analytic Society, secretary of The International Psycho-Analytic Association, director of the London Clinic, chairman of the Training Committee, and president (in effect) of the British Psycho-Analytic Society in Jones' absence (p 283).

b.) Melanie Klein's arrival

Melanie Klein came to live in the UK in 1926 and was already officially a member of the British Society in 1927. Born in Vienna, but practising as a psychoanalyst in Germany, she left behind her place at the Berlin Society and lost her major supporter there, with the death of Karl Abraham in 1925 (King and Steiner 1991 p 18). Her arrival was very significant in the life of the UK's local psychoanalytic community. Commentators point to the shared views between Klein and British analysts. Pearl King, for example, notes the common belief in the importance of 'pre-genital and innate determinants over and above the influence of external and environmental stress (…) [and] the role of hate and aggression and their relation to morbid anxiety and guilt' (p 19). What is more, Klein seemed to have the majority of the British Society standing solidly behind her ideas for some time (Grosskurth 1986 p 183). In fact, her 1932 book, *The Psycho-Analysis of Children*, was considered ‘the most important work yet published by a member of the British Society’ (p 195).

In the subsequent years, Klein published some of her great papers such as *A Contribution to the Psychogenesis of Manic-Depressive States* (1935), *Weaning* (1936), *Love, Guilt and Reparation* (1937), and *Mourning and Its Relation to Manic-Depressive States* (1940) (Grosskurth 1986 p 231). All of these texts were written before the Controversial Discussions and were developed through Klein’s psychoanalytic work with children. At the same time, the consolidation of her work meant that the ‘“English School” was beginning to be defined far more clearly as the “Kleinian School”’ (p 207). According to one of Klein’s most famous followers, Hanna Segal, ‘A Contribution to the Psychogenesis of Manic-Depressive States is a watershed in the development of Melanie Klein’s thought’ (1979 p 78). Segal (1979) argues that this text contains Klein’s first articulation of her previous experience with children and her convictions on the importance of the first years of life for the psyche according to a theoretical framework. It was there that Klein posited her notion of a depressive position as a psychological configuration in the development of children in their first year of life (p 78).

With the consolidation of her work, Klein attracted both supporters and detractors in the UK. Her 1935 text, while marking the birth of a ‘Kleinian School’, is also regarded as the moment wherein Klein’s honeymoon with psychoanalysis in the UK ended (Kohon 1986). Opposition to Klein’s idea began, for example, with Edward Glover, Melitta Schmideberg (Klein’s daughter), Barbara Low and other psychoanalysts emigrating from continental Europe (Kohon 1986; King and Steiner 1991). In addition to personal and political differences, the main arguments for opposition concerned Klein’s use of phantasy, her interpretation of Freud’s death drive, the early dating of the superego, and her notion of internal objects (p 21). In short, Klein ‘was putting forward a view of early development and the genesis of psychic functioning (…) which they [*her detractors*] did not feel were consistent with psychoanalysis as they knew it’ (p 22). This division between detractors and supporters took shape in the UK during the 1930s and was later re-enacted during the Controversial Discussions.

By the time of the Controversies, Klein’s situation was even more delicate, given the growing opposition outside the UK to her work. With the development of Klein’s views on child analysis, Anna Freud became one of her most visible critics. Anna Freud and Melanie Klein quarrelled over the subject of child analysis for several years, with acrimonious replies from both sides prior to, and continuing into, the Controversial Discussions. The strain between them became evident with the former’s 1927 book, *Introduction to Child Analysis*. The British Society promoted a symposium on the question of child analysis later the same year, where Klein and other British analysts criticised this book (Kohon 1986 p 39). The symposium led to a polarisation between London and Vienna, with Jones and Freud subsequently exchanging letters on the matter. The former was seen as diplomatic with the latter portrayed as furious (Steiner 1985; Young-Bruehl 1988 p 171). The differences between Klein and Anna Freud mainly concerned 'the dating of the Oedipus complex, the emergence of the ego and the superego, the question of the possibility of transference neurosis in children, the role of early anxieties, and indication for treatment' (Kohon 1986 p 38). While Klein took the view that children could and should be analysed in depth, Anna Freud countered that children could not be analysed on similar grounds to adults (Young-Bruehl 1988 p 167). The growing differences between Vienna and London on psychoanalytic theory and technique led to an exchange of lectures from analysts of both societies. Even with the institutional engagement between Vienna and London, King writes that 'it is doubtful how much clarification or mutual understanding was achieved' (King and Steiner 1991 p 24).

Prior to the Controversial Discussions, it was already possible to discern two important strands of opposition to Klein's work, which would later inform the Controversies.

c.) Child Analysis

The possibility of using child analysis to confirm psychoanalytic theory or to make ground-breaking discoveries has understandably fascinated psychoanalysts from early on. Since its inception, psychoanalysis has been concerned with the role of memory, so-called infantile aspects and the primary workings of the mind, and the development of the psyche and theories of aetiological causes of illness. In this respect, the potential novelties provided by child analysis would meet the desire to observe 'a neurosis *in statu nascendi*' (Geissmann and Geissmann 1998 p 17), with Freud's 'Little Hans' (1909) seen as an encouraging and successful case study for emulation:

> Children have become the main subject of psycho-analytic research and have thus replaced in importance the neurotics on whom its studies began. Analysis has shown how the child lives on, almost unchanged, in the sick man as well as in the dreamer and the artist; it has thrown light on the motive forces and trends which set its characteristic stamp upon the childish nature; and it has traced the stages through which a child grows to maturity' (1925b p 273).

In addition to Freud's sense of excitement and optimism, the quote is interesting for other reasons. For instance, Freud clearly asserts the central relevance of child analysis while also referencing how the 'child' affects and illuminates the adult by living on, almost unchanged, in the adult's psyche, in the adult's 'childish nature', and by laying bare the developmental stages of growth towards maturity. Therefore, from a psychoanalyst's perspective, child analysis should 'raise and solve a large number of theoretical problems concerning not just children but psychoanalysis in general' (Geissmann and Geissmann 1998 p 20). At the same time, child analysis would provide 'an empirical forum to test, confirm, falsify, and create new theory about infantile needs, wishes, and fears' (Makari 2008 p 421), so that 'from now on, psychoanalytic theories of childhood would require coherence and support from such first hand, empirical study' (p 425).

Another aspect of child analysis, established by Isaacs during the Controversial Discussions, relates to the genetic principle of childhood primary processes and conflicts living on in adulthood (King and Steiner 1991). This principle would guarantee that adults and children be directly connected via the unconscious. That is, in spite of all mechanisms of defence, distortion, and repression that the adult mind develops, the deeper strata of the mind where 'the child lives on' would not be altered. This sense of continuity was extremely important because it granted child analysis an important status and set the terms in which it related to the analysis of adults. From this perspective, then, the child gains prominence by anticipating, shaping, and never ceasing to exist in the adult. So any new discovery in the child could inform the whole psychoanalytic chain of concepts – extending, modifying, and confirming psychoanalytic knowledge. It is hardly by chance, then, that the genetic principle was a relevant feature during the Controversial Discussions (King and Steiner 1991); it gave shape to the debate in terms of psychic development and early infancy. The implication was that the younger the pool of analysed patients, the closer psychoanalysts would be to the very beginning of psychic development, and to the roots of the mind. For Gould (2006), this stance in psychoanalysis could be understood in much the same way as it was in evolutionary psychology – according to the search for knowledge based on the modularity of basic mental operations (pp 452-453). These operations would be typified as basic and universal patterns, stemming from the study of children during the Controversial Discussions, and in relation to Klein's work. In other words, the 'purity' of knowledge stemming from child analysis would lay bare the foundations of the human mind according to a biological rationale, centred on phylogeny and ontogeny (Gould 1977; Sulloway 1979; Young 2006).

d.) The Freud family in London

Freud spent the last year of his life in the UK, passing away in London in September 1939. Following Hitler's invasion of Austria in 1938, Freud's family, including Anna, left Vienna with the assistance of the British government and the personal efforts of Ernest Jones and Marie Bonaparte (Gay 2006). Like Freud, many psychoanalysts immigrated to the UK prior to the war; some, such as Paula Heimann and Kate Friedlander, came from Germany after the Nazis took power in 1933. Barbara

Lantos arrived from Budapest in 1935, followed by her compatriot Michael Balint in 1938. Melitta and Walter Schmideberg arrived from Vienna in 1932, with Viennese couple Hedwig and Willi Hoffer following in 1938 (King and Steiner 1991). There were many others taking part in this psychoanalytic dispersal, leaving the continent in waves; some stayed in the UK while many left for the US.

The immigrants listed above formed the quorum of psychoanalysts at the Controversial Discussions. According to King, 'the main argument [at the Controversies] was between Edward Glover, Melitta and Walter Schmideberg, Willi and Hedwig Hoffer, Barbara Low, Dorothy Burlingham, Barbara Lantos, and Kate Friedlander, who, along with Anna Freud, opposed the new ideas of Melanie Klein' (p 3). This heterogeneous group clashed with Klein 'whose main supporters were Susan Isaacs, Joan Riviere, Paula Heimann, Donald Winnicott, and John Rickman' (Ibid.). There was also a third group of participants 'who were not committed to either point of view but who wanted some compromise to be reached (…) Among these were Ernest Jones, Sylvia Payne, Ella Sharpe, Marjorie Brierley, William Gillespie, John Bowlby, James Strachey, Michael Balint, and Adrian and Karin Stephen' (pp 3-4). These three groups would later be known as the Freudian group, the Kleinian group, and the Middle or Independent group.

One peculiar characteristic of the Controversial Discussions was that it was the first time a psychoanalytic controversy had to be handled without Freud. Instead of a clash of personalities, centred on Freud versus Jung, Adler, or Rank, for example, now matters had to be settled according to the configuration of the aforementioned persons and groups. It took these analysts, as a group, four years to set up and discuss their psychoanalytic differences, before they could finally deliberate and reach an outcome.

4.1 The Controversial Discussions as forum for debate

From a formal perspective the Controversial Discussions can be regarded according to four principal moments: the Extraordinary Business Meetings, the Scientific Meetings, the Training Committee report, and the Gentlemen's Agreement. These divisions highlight the establishment of a forum for discussion, the examination of Melanie Klein's ideas, the impact of psychoanalytic differences on the training of candidates, and a stalemate as the formal resolution to the Controversial Discussions.

The Freud-Klein Controversies 1941-1945 (1991), edited by Pearl King and Riccardo Steiner, is considered the fundamental source of information regarding the Controversies. This book presents verbatim discussions, minutes, presented papers, letter exchanges, organisational statutes, and personal statements.[13]

a.) Extraordinary Business Meetings

An Extraordinary Business Meeting was called in February 1942 to address issues regarding the manner in which the British Society was being run. Members 'were asked to send in any other resolution on topics which they wanted discussed' (King and Steiner 1991 p 34), and indeed sixteen submissions, dealing with different sources of discontent, were received. The resolutions encompassed suggestions as varied as: the formal arrangements for decision-making (e.g. concern for members away from London during wartime), limitations on the tenure of offices, protests about behaviours, concerns regarding the relations between the British Society and the public, fears of the domination of a Kleinian ideology, the affirmation of the British Society as furthering psychoanalytic knowledge, and the delimitation of deviations from Freudian psychoanalysis (King and Steiner 1991 pp 37-41). This Extraordinary Business Meeting can be regarded as the formal beginning of the Controversial Discussions.

With the wide range of issues brought forward, it became clear that a single meeting would not suffice. In fact, these issues were raised over five Extraordinary Business Meetings, with Ernest Jones as chair and an average of thirty psychoanalysts in attendance. The meeting would open with initial procedures (such as reading resolutions, minutes, or summaries of previous meetings), followed by a debate, and concluding with either a decision by vote or postponement to the next meeting a month later. In total, this stage of the Controversies lasted about eight months.

While it is not within the scope of this review to investigate all of the issues discussed in these meetings, two dynamics of these meetings are of great interest. Jones summarises them in the first meeting as 'changes in our constitution' and 'a thorough discussion of what some people call our scientific aims and methods, others scientific differences' (King and Steiner 1991 p 56). By the second Extraordinary Business Meeting, the latter was already becoming the primary point of discussion, as Anna Freud remarked 'if we alter the rules [of the constitution] before settling the scientific differences it seems that we take neither the rules nor the difficulties very earnestly' (p 77). Anna Freud's emphasis on what she regarded as scientific differences echoed Isaacs' similar comment that 'it is very important to discuss scientific differences before any attempt is made to alteration of rules' (p 67). Their views were endorsed, for example, by Jones who wonders in the

[13] See Appendix.

same meeting 'what is the most profitable way of coming to grips with the essential thing, namely the problem of scientific differences?' (p 81).

It was also in the second meeting that the debate became more confrontational. Walter Schmideberg (Melanie Klein's son-in-law) provocatively affirmed that the aim of the British Society was to 'further Freudian psychoanalysis' (p 84), noting that Klein and her followers resembled a religious sect displacing the centrality of the Oedipus complex in psychoanalysis (p 86). Melitta Schmideberg (Klein's daughter) added insult to injury by stating that 'it is sufficient to say that every Member who was not 120 per cent Kleinian has been attacked systematically, directly or indirectly' (p 94). Glover in turn overtly accuses Klein and her group of using 'the machinery of the Society to increase its own influence' (p 101). In the following meeting, one month later, Joan Riviere criticised the participants above for their intolerance and personal ill-feelings (p 112-3). Accusations escalated further with Low's (p 119) retort against Riviere. It was only in the fifth Extraordinary Business Meeting – after a series of considerations from the participants on how to proceed against criticism, accusations, and charges – that the debate could return to scientific matters. Brierley, who had proposed the unanimously accepted armistice, stated that:

> These meetings have made at least two things clear. (1) We must find out whether the differences in theory and practice amongst us are, or are not, so fundamental as to make it impossible for us to remain one Society. This is essentially a scientific problem, and we must insist on a scientific resolution. (2) There is no reasonable hope of solving this problem unless we secure a period of public truce (p 174).

So all animosities were to be channelled into what psychoanalysts regarded as a scientific debate, once a month, in a series of Scientific Meetings. Brierley drew up a memorandum with the overarching question on object-relations theory and how it related to instinct-driven theory, and whether they were antithetic or complementary (p 212). Without mentioning names, Brierley was diplomatically paraphrasing the potentially antithetical positions between Klein's and Freud's theories. In more objective terms, the meetings were supposed to elucidate their respective views 'on the development of the psyche from infancy to the close of the Oedipus phase – roughly the first five years' (Ibid.) so that their implications for theory and practice could be discussed.

Immediately after Brierley's memorandum was presented, Glover noted that 'in a scientific controversy (…) the onus of proof lies on those who advance new theories' (King and Steiner 1991 p 215-6), and the members agreed that Klein and her supporters should take the initiative in presenting their ideas.

b.) Scientific Meetings

A series of ten Scientific Meetings were dedicated to the discussion of four papers presenting Klein's ideas. The opening text of the series was Isaacs' *The Nature and Function of Phantasy*, followed by Heimann's *Some Aspects of the Role of Introjection and Projection in Early Development*, Heimann and Isaacs' *Regression,* and Klein's *The Emotional Life and Ego-Development of the Infant with Special Reference to the Depressive Position*. All of them were carefully overseen by Klein in order that her ideas be conveyed with her consent and under her strict supervision (King and Steiner 1991). The first paper was discussed over five meetings, while the second, third, and last papers took up two, one, and two meetings respectively. In Jones' absence, the meetings were chaired by Glover. Members would come to the meetings having read the papers, a series of comments would be put forward by whomever wanted to manifest his or her opinion, the author would respond and a new round of comments would follow. The first two papers generated the most heated debates, mainly because, between the first and second pair of papers, Glover resigned from the British Society and Anna Freud resigned from the Training Committee. Neither of these psychoanalysts, nor their close followers, personally attended the remainder of meetings. So the second pair of papers was only discussed only among Klein's supporters and the Middle Group.

With her opening paper, Isaacs argued for the existence of early phantasy in the psyche according to a developmental perspective (p 267). In fact, Isaacs asserts the existence of pre-verbal phantasies in the first six-months of life (p 299-307) as 'a fundamental fact of general development' (p 285); 'it is the facts which have compelled this extension' (p 271). These phantasies were sensorial and expressed unconscious mental processes as subjective interpretations of experiences (p 313). In terms of evidence, she relies on theoretical postulates such as genetic continuity, behaviouristic data, clinical evidence from the analysis of young children (as early as two years old), and the analysis of adults (p 298-99). However, the debate focussed mainly on the mental functioning of the first year of life, and centred on two main concerns: the evidence and validation of Isaacs' views on phantasy as a fact (e.g. Jones' statement, p 324), and the emergence of a new metapsychology in psychoanalysis (e.g. Glover's comment, p 325).

The next paper was presented by Heimann, wherein she posited the centrality of introjection and projection as basic mental mechanisms of early phantasy. She proposed that these mechanisms operated in object-relation terms, motivated by Melanie Klein's views on the death drive. In this regard, Klein's group differed from Freud's views on psychic development by displacing autoerotism and primary narcissism with the primacy of object-relationality (pp 520-1). The validity of these views and their implied metapsychological changes continued to dominate the discussions, and no agreement was ever reached.

The third paper discussed Klein's views on the death drive based on fixations and unresolved conflicts in psychic development. Klein and her supporters argued that they were expanding Freud's views on libidinal fixation, developmental arrests, and unresolved conflicts according to the death drive (pp 702-3). Klein's subsequent paper dealt with her notion of the depressive position and the emotional life of the infant.

c.) The Training Committee report

In parallel with the Scientific Meetings, the Training Committee set out to explore the effects of psychoanalytic differences on candidates (p 593). All members of the committee – Glover, Strachey, Brierley, Anna Freud, Klein, Sharpe, and Payne – ultimately presented memoranda on their own views on technique. A discussion ensued and a preliminary draft report was written by Strachey (p 595), which included the recommendation that 'in choosing the members of the Training Committee, the Society should deliberately bear in mind the undesirability of appointing persons who are prominently involved in acute scientific or personal controversies' (p 595). Although the details of these memoranda are beyond the scope of this review, Strachey's Draft Report was important for the Controversial Discussions because its call for diversity tolerance prompted the departure of Anna Freud and Glover from the Scientific Meetings, clearing the way for Klein's views to be accommodated within the British Society. Anna Freud took offence at some of the suggestions in the report, and resigned from the Committee at the next Scientific Meeting (p 665). As for Glover, after making his comments on the Draft Report, he resigned from the British Society altogether.

d.) The Gentlemen's Agreement

With Jones' retirement and Glover's resignation, Sylvia Payne was elected the new president of the British Society (p 902). In addition to new tenure and eligibility rules (p 897), it was established by an unwritten agreement that 'there should be representatives of all three "groups" on the main committees of the Society' (p 907). This agreement led to the reorganisation of the training of candidates. Payne and Anna Freud negotiated the conditions under which the latter would return and take part in training (p 906), resolving that there would be a course A with 'teachers being drawn from all groups' (Ibid.), and course B 'which would teach technique along the lines supported by Miss Freud and her colleagues' (Ibid.). Further criteria established the dividing of training into groups (p 907), and this artefact of the truce following the scientific stalemate lasted in the British Society until 2005.

4.2 Review of the literature on the Controversial Discussions

Reflecting upon psychoanalytic controversies, Green (2004) remarks that 'except for the *Controversies* of 1941-1945 in London, which led to a unique cross-examination with deep scrutiny of the divergences, there is no real other debate like this one [in psychoanalysis]' (p 124). His strong statement suggests a central significance to the Controversial Discussions, and helps to explain why so many different authors have tried to make sense of them.

Hayman (1994), for example, regards the Controversial Discussions as a series of meetings 'held for the specific purpose of attempting to resolve some major disagreements about theory, practice and teaching of psychoanalysis that were due to the growing differences between the psychoanalytic theories of Freud and the new theories emanating from London, especially Melanie Klein' (p 343). In other words, she portrayed the Controversies as revolving around ideas (theory), clinical work (practice), and institutional matters (the teaching of psychoanalysis). She also considers the divergences between Freud's and Klein's ideas to be the fulcrum of the debate. In other words, she subscribes to Bergmann's understanding of the Controversies as a conflict of psychoanalytic ideas, involving different metapsychologies (2004). She adds: 'the Discussions were an attempt to clarify the theoretical differences between what was developing into two schools of thought, and to assess whether Klein's views were compatible or incompatible with those of Freud' (Hayman 1994 p 343).

Hayman considers the notion of 'unconscious phantasy', presented in Isaacs' opening paper, to be a key idea in the Controversial Discussions. In brief, Isaacs posited the existence of sensorial oral-cannibalistic phantasy and unconscious conflict from birth in terms of object-relation dynamics and mechanisms of introjection and projection. This clashed with Freud's views on early infancy which instead emphasised autoerotism and narcissism. Hayman (1989) noted that the conceptual differences underpinning the notion of phantasy were first clearly presented during the Controversial Discussions. She lists several disagreements, including: questions concerning the existence of unconscious phantasy from birth, its primary contents of sensations, aggression, and oral-cannibalism, its validity, the emphasis on object-relations and theoretical implications, the reliance on genetic continuity and the sophistication of early experiences, and the importance given to psychic mechanisms such as projection and introjection (pp 344-355). Klein's belief in phantasy as the primary content of the unconscious was also controversial (p 350). This new use of phantasy in psychoanalysis prompted Hayman to ask 'whether the extension of the concept of "phantasy" helpfully widened the reference or confusingly blurred necessary distinctions' (p 355). Accordingly, Hayman considers the dispute to be an unsuccessful attempt to clarify and improve communicative and conceptual difficulties (1994 p 356). She comments on the confusion brought about by different understandings of one notion, which created great difficulty and ultimately led to the impasse: ‘the contradictions alleged in the Discussions, in aligning “phantasy” with perceiving, thinking, hallucinating, remembering, judging, interpreting, defending, longing and so on, which were argued without resolution, remain a difficulty’ (p 356).

Certainly, these divergent ideas contributed to an alternative metapsychology (relative to Freud’s). For example, Brierley asked: ‘is (…) object-relation compatible or incompatible (…) with theory in terms of instinct vicissitude? Are such theories antithetic or complementary?’ (King and Steiner 1991 p 212). Also, Glover notes in the first discussion on Isaacs’ paper that ‘it becomes clear that Mrs Isaacs’ main concern is to build up a new metapsychology’ (p 325). Further theoretical differences were clearly articulated in Heimann’s paper; as she says ‘our views differ from Freud’s explicit statements in the following points’ (p 521), and goes on to give a series of modifications. However, as relevant as differences in metapsychology were, Hayman’s account fails to mention that these ideas were not simply a theoretical construct; participants considered them to be couched in clinical practice and asserted them to be knowledge, truth, and fact. Consequently, Hayman’s account skirts any reference to the problem of psychoanalysis in relation to these terms, and the dynamics of internal demarcation at the Controversies.

Bergmann (2003) draws attention to the role of unconscious phantasies (p 55) yet chooses to emphasise a developmental perspective. He therefore approaches the Controversial Discussions not so much in terms of a Klein-versus-Freud debate, but rather as a dispute between Klein and Anna Freud, their respective theorisations in terms of child development and, above all, who would represent ‘Freud’s own thinking faithfully’ (p 56). As such, Bergmann (1997) mostly frames his understanding of the Controversial Discussions as a tension between orthodoxy (Anna Freud) and modification (Klein) – accepted opinions and challenges to them. In this respect, he sees the Controversial Discussions as a political (if not religious) problem of belief in ideas, characterised by a power struggle between sparring factions. In fact, he concludes his analysis by writing that ‘Anna Freud, no longer supported by the authority of her father, failed to persuade the British Psycho-Analytical Society that Melanie Klein and her followers should be expelled’ (1997 p 78).

Bergmann (1997) traces the establishment of psychoanalytic orthodoxy to Freud’s break with Adler and Jung in 1911 and 1913 respectively. He offers Adler’s case as ‘the prototype of disciple breaking with Freud’ (p 70). Considering a series of Freud’s texts at that time, especially *Wild Psycho-Analysis* and *On the History of the Psycho-Analytic Movement*, Bergmann posits that Freud was establishing the boundaries of psychoanalysis and asserting its core beliefs in terms of adherence to sexuality, and especially the Oedipus complex: 'with the progress of psycho-analytic studies the importance of the Oedipus complex has become more and more clearly evident; its recognition has become the shibboleth that distinguishes the adherents of psychoanalysis from its opponents' (Freud 1905 p 226, cited in Bergmann 1997 p 75). The establishment of an orthodoxy would also galvanise the psychoanalytic movement, providing it with a high degree of cohesion by 1914 (Bergmann 1997 p 76).

It is worth considering the connection between the Controversial Discussions and Freud's earlier breaks with disciples. Bergmann suggests that validation in psychoanalysis is a political act, with breaks, dissidences, and unity all related to political authority. While there is indeed an important political component to these situations, to characterise the outcome of the Controversies according to Anna Freud's incapacity to carry over her father's authority seems too narrow an analysis. Bergmann ignores both the metapsychological implications and the significant presence of terms like 'truth', 'science', and 'fact' as they regularly appeared at the Controversies. Are we to consider these terms merely in terms of politics? Furthermore, by choosing to emphasise the political aspects of Freud's break with Jung and Adler, Bergmann ignores the fact that, at this same time, Freud first articulated the notion of phylogenesis (Freud 1911a) in connection with the universality of the Oedipus complex, and according to the logic of both the genetic principle[14] and intergenerational transmission (e.g. *Totem and Taboo* (1912-1913)). In other words, the Oedipus complex, the central feature of psychoanalysis, was described within a biological rationale. Consequently, where Bergmann sees only political demarcation taking place in terms of orthodoxy, it is also possible to consider phylogenesis as a criterion for internal demarcation in psychoanalysis according to ANT. In Chapter 6 I will argue that phylogenesis, as articulated by Freud in relation to psychoanalysis, was carried over along with orthodoxy (Bergmann 1997) from the 1910s to the Controversial Discussions, with important consequences for the latter.

Further to these approaches privileging metapsychology and political orthodoxy, Rose (1993) considers the Controversial Discussions from an angle similar to Barratt's (1988). Just as the latter attempted to dislodge the prevailing notions of science, truth, and knowledge, Rose (1993) regards Klein's work as a challenge to the psychoanalytic status quo, which she deemed too attached to traditional scientific values in her view. According to Rose (1993), Klein offered a disturbing subjectivity, a psychic negativity ('a limit of what a society, of what a subject, can recognize of itself' (p 143)). In this respect, Rose considers the possibility of objective knowledge and scientific supremacy at the Controversial Discussions exclusively in Freudian terms (p 147) with Klein's work

[14] Further explanation provided previously on page 27. See also Section 5.3 of this Chapter.

opposing it. For example, Rose asserts that 'the concept of the death instinct or impulse is in no sense a biologistic concept in the work of Klein' (p 148). As a result, her reading of the Controversial Discussions is based on the participants' failure to grasp what Klein brought to the fore. That is, an intolerance to a fundamental negativity at the basis of subjectivity, a 'black-hole' that gives meaning to death (p 149) that blurs or short-circuits the psychoanalytic distinction between knowledge and phantasy, and even puts 'the status of psychoanalysis as scientific knowledge (…) at stake' (p 163).

It is true that Klein's ideas brought about disruption and novelty in the psychoanalytic scene. Hayman's and Bergmann's readings of the Controversial Discussions take into consideration the idea of disruption provoked by something new – a new metapsychology and new political power, respectively. However, Rose's (1993) articulation of Klein as a novelty can be challenged on different grounds. For example, the argument that biology was only on the side of Klein's opponents in the debate seems largely incorrect. As we shall see later, both sides of the dispute relied on biological arguments to support their views. Furthermore, Klein and her followers were as interested in considering their views to be objective and scientifically sound as her detractors were interested in rejecting them. During the Controversies, for instance, Klein defends her notion of the depressive position as 'based on the phylogenetic inheritance and […] ontogenetically the most fundamental of all human patterns' (p 757). Also countering Rose (1993 p 169), Klein and her followers provide a developmental framework, for example when Heimann and Isaacs account for the death drive in terms of regression and dates of fixation in early infancy and in connection with aggression (King and Steiner 1991 pp 687-709). It may be puzzling to the contemporary psychoanalytic reader, but Klein was indeed very concerned, in her work, to date phases in childhood (Spillius 1994 p 356). Elsewhere, in a critical stage of the Controversies, Isaacs was deeply concerned with providing evidence supporting their claims that unconscious phantasy is a fact of general development (King and Steiner 1991 pp 298-299).

Rose (1993) sees Klein's role in the Controversies as challenging the psychoanalytic status quo, but not simply by subverting established psychoanalytic views and standards or by opposing subjectivity against objectivity *tout court*. Rose (1993) has the merit of delving into details of the Controversy, focusing on what she understands as the possibility of objective knowledge, mostly in relation to biology, and Klein's idea of a death drive and the psychotic part of subjectivity (p 147). Accordingly, she argues that Klein's work disturbs 'the status of psychoanalysis as scientific knowledge' (p 163). However, I will argue that Klein and her followers subscribed to the same standards of validation as other psychoanalysts during the Controversies. In other words, Klein did not question the scientific standards of psychoanalysis, but subscribed to them and articulated her work according to these very same standards. In this respect, this thesis concurs with Rose's perspective on the importance of knowledge and the central role of biology during the Controversies. Nevertheless, I will argue that the strain on psychoanalytic knowledge should be regarded in terms of knowledge production in psychoanalysis, rather than Klein's triumph over an established and intolerant institution.

Steiner (1985; 2000) also provides a perspective on the Controversial Discussions, focusing on the history of psychoanalytic ideas and attempting to combine an internalist account with the socio-historic contexts in which these ideas were produced. On the one hand, Freud's sophisticated metapsychology belonged to a Viennese *fin-de-siècle* milieu; on the other side was a less coherent English way of working, reliant on the 'undogmatic empiricism of the pioneers of British psychoanalysis' (1985 p 40). Combining internalist with externalist explanations, for example, he writes that:

> Their leader [for the Viennese psychoanalysts] was Anna Freud, who in some ways seemed literally to incarnate her father's world. Their stronghold was the almost complete German edition of Freud's writings, published by Imago (…). Many of the Viennese knew these writings almost by heart, and they adopted (…) Freud's style [in discussion]: (…) its wealth of metapsychological terminology derives from the medical and philosophical, literary and psychological culture of that magic yet tragic era of *fin-de-siècle* Vienna' (p 30).

In this same text, Steiner adds a psychological explanation to the Controversial Discussions. For him:

> The focus of Klein's new theory of object relationships consists of the values implicit in the depressive position, and it was these values that did so much to prevent the CD [Controversial Discussions] from ending in catastrophe (…). Something of those values seems, however, to have found a positive echo in the unconscious of psychoanalysts who did not regard themselves as followers of Klein' (p 65).

With internalist, externalist, and psychological reasoning, Steiner (2000a) points to the complexity of the Controversies. In addition, he delves into relevant letters between Freud and Jones, Klein and Jones, and Anna Freud and Jones (Ibid.). He portrays the Controversies as a 'hermeneutic battle' over who could interpret Freud's work correctly (p 46), and also in terms of an orthodoxy led by Anna Freud in relation to Klein's ideas (p 30). His text spans so many different threads that it ends up glancing through all the aforementioned perspectives while failing to deepen our understanding of their influence over the Controversies. As such, Steiner (2000a) could be regarded as an essayist, offering entry points for investigation without providing in-depth engagement with the Controversial Discussions.

After working through several perspectives, Steiner invokes Kuhn's philosophy of science in the final part of his essay: 'obviously we cannot straightforwardly translate what I have said about the CD into terms of paradigms of normal science and scientific revolution. Kuhn applies his definitions to the development of what are generally known as the "hard" sciences: physics, chemistry, and astronomy' (p 71). Steiner aims to make sense of the Controversies' ultimate compromise by relying on the psychological argument of a worked-through depressive position ('the ability to balance one's destructive impulses with reparative impulses' (p 65)) among participants, such that they managed to prevent catastrophe and rupture. And he suggests a combination of psychology with paradigms.[15] Drawing on Kuhn, even if in passing, Steiner proposes that the Controversial Discussions be seen as a shift of psychoanalytic paradigms, wherein emotional anxiety was somehow worked through so that a gentlemen's agreement could be achieved and dissension averted (p 76). Yet, unless the notion of paradigm is properly articulated, it would be hard to substantiate the case for a shift of paradigms in psychoanalytic controversies. The mere mention of paradigms implies a scientific discussion, based on the existence of different perspectives, but without the notion of paradigm being properly articulated, it is hard to assess its implications any further than this (e.g. Fuller 2000).[16]

So far, the reviewed literature has framed the Controversial Discussions according to four main emphases: the metapsychological implications (Hayman 1994), the psychoanalytic orthodoxy (Bergmann 1997), the disruptive effect of negativity on the psychoanalytic institution (Rose 1993), and the necessary psychological working through in relation to paradigm shifts (Steiner 2000b). There are also some other contributions to the debate. Kohon (1986), for example, regards the Controversies as a complex situation, fought out by two women, each attempting to develop her own version of Freud; where Anna Freud emphasised developmental aspects of the libido – rather than *Nachtraglichkeit* – Klein was interested in positions (pp 42-3). Kohon ultimately regards these differences in terms of Klein creating 'a different metapsychology, a different model of the mind, based on different hypotheses from those that Freud had developed' (p 43). He proposes that Klein's notion of the death instinct, unconscious phantasy, and the pre-verbal stage, led to differences in theoretical beliefs as well as in clinical practice and technique, and affected the power structures of psychoanalysis in Britain. Thus, he is broadly aligned with Hayman's view on the metapsychological implications of the Controversies.

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[15] For a discussion on the problematic role of paradigms in STS, see Chapter 3, Section 1.

While the literature often focuses on Klein versus Anna Freud, Roazen (2000; 2002) provides a different take on the Controversial Discussions. With, by his own account, a revisionist historical perspective, he emphasises the key role played by Edward Glover while Ernest Jones 'conveniently shrank into the heart of the country, excusing his absence with a plea of ill health (…) from which he miraculously recovered by the end of the Controversial Discussions' (Grosskurth 1986 p 317). In an interview with Glover, Roazen asked 'who led the battle against Klein?' to which he answers that it was Glover himself and Melitta Schmideberg (Klein's daughter), while Anna Freud played a self-effacing role at that time (p 262). Based on this answer, Roazen adds complexity to the question of power and orthodoxy at the Controversies.

Klein's and Anna Freud's biographers both emphasise the role of the Controversies in helping debate through conceptual clarification, while both acknowledge the backdrop of disputes, animosity, and institutional conflicts in relation to the Controversial Discussions (Young-Bruehl 1988; Grosskurth 1986). While providing a description of the unfolding of the debate, Young-Bruehl (1988), noticed that 'to the tug-of-war over who was truly Freudian, the Controversial Discussions gave no respite. Both sides strained mightily, but the rope stood still. What did come from the discussions, however, was clarity about the territory in dispute' (p 267). Similarly, Grosskurth (1986) emphasises that 'it became apparent that on certain issues various members held mutually irreconcilable views; but at least there was an impressive attempt to clarify theoretical concepts, a welcome change from the backbiting of the Extraordinary Meetings the previous year' (p 319). In this respect, both biographers echo the call for a better debate of psychoanalytic concepts.

Taking stock of the available literature, the Controversies have been framed according to different perspectives such as personal rivalries, clashing of ideas, development of concepts, alternative metapsychology, Freud's legacy, psychological explanation, repudiation of a psychic negativity, and political orthodoxy. None of these themes are entirely clear cut in the literature, of course. They overlap and incorporate other elements in their accounts. Yet these different framing perspectives are helpful in understanding how the Controversial Discussions came to be understood and explained.

The main point of this literature review, though, is that the term 'science' has very often been evoked but seldom used as a central theme for explaining the Controversial Discussions, and fails to develop into a more robust framework. Where the term 'science' is concerned, the literature on the Controversial Discussions falls into three categories: some ignore its presence altogether (Grosskurth 1986; Kohon 1986; Young-Bruehl 1988; Hayman 1994; Bergmann 1997; Roazen 2002), some deal with it in a tentative and ambivalent way (Steiner 2000b), while some attempt to subvert it (Rose

1993). Therefore, there is no study that truly focuses on the role of 'science' and its implications for the Controversies in a systematic manner. This may be due to philosophy of science's negative response on the scientific status of psychoanalysis, rendering unnecessary or illogical any approach to the Controversial Discussions based on 'science'. However, Stengers (1997) has already argued that, even if psychoanalysis fails to achieve a scientific status, it should not be regarded as scientism, and it does not stop her from seriously considering the hard efforts on Freud's part to establish psychoanalysis as a science. From an ANT perspective, I follow a similar distinction: regardless of the scientific status of psychoanalysis, I still think it is relevant to consider the process of knowledge production in psychoanalysis, and consequently its internal demarcation criterion. Consequently, terms like 'internal demarcation', 'objective knowledge', 'truth', 'fact', 'science', and so on do not need to be relegated as scientism. These terms can also be regarded from an ANT perspective that privileges the serious effort to produce knowledge and its associated implications.[17] In this context, this gap on 'science' in the surveyed literature prepares the ground for an original contribution to the research question presented in the Introduction: how does psychoanalysis demarcate itself, internally, separating the concepts regarded as valid knowledge from those it rejects as invalid?

5. Psychoanalysis and the question of science

Having covered the literature on psychoanalytic controversy, this final section reviews relevant aspects of the literature on science and psychoanalysis. The focus of discussion here is the major argument for the scientific demarcation of psychoanalysis, mainly in terms of a philosophical debate between deductivism and inductivism. These poles of the discussion were championed by Karl Popper and Adolf Grunbaum (Popper 1959; Grunbaum 1984) respectively, and have their roots in Popper's formative years in Vienna in the 1910s. There have been many contributors to this debate, arising from both camps (e.g. Erwin 1996; Cioffi 1998; Boag et al. 2015), although a full survey of these developments would be beyond the scope of this review. Of primary interest for the purpose of this thesis is the different criteria of demarcation used to determine whether or not psychoanalysis is a science. It is against this background that I will return to the question of internal demarcation.

5.1 Popper and the criterion of falsification in psychoanalysis

[17] See glossary for terms such as 'truth', 'science', and 'fact'. See also the discussion in Chapter 7, Section 3.

In the beginning of *Conjectures and Refutations* (1969), Karl Popper reinstates a problem he had been grappling with since 1919: '*When should a theory be ranked as scientific?'* or '*Is there a criterion for the scientific character or status of a theory?' (p 33)*. He was aware of the widely accepted answer to this problem: 'that science is distinguished from pseudo-science – or from "metaphysics" – by its *empirical method,* which is essentially *inductive*, proceeding from observation or experiment' (p 33). In other words, Popper was faced with the traditional understanding that science could be distinguished on the basis of inductive methodology, but did not find this answer satisfying. He consciously engaged in a philosophical debate, pitting his views against a positivist interpretation of scientific knowledge, such as that taken by the Vienna Circle (Corvi 1997 p 22), while establishing dialogue with philosophers like Kant, Hume (p 24), and Bacon (p 32). Popper's own answer to the question of demarcating science from non-science was based on falsifiability, refutability, or testability (Popper 1969 p 37). His criterion for demarcating whether a theory should be ranked as scientific or not depends on the idea that 'every good scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is' (p 36). As a result, according to Popper's apparently simple logic of demarcation, 'a theory which is not refutable by any conceivable event is non-scientific' (Ibid.). With this approach, he criticises positivistic views that allow empirical validation of general theories to be proof of regularity in the world (Corvi 1997 p 21). Instead he emphasises the need for certifying tests in the first place: 'confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory' (Popper 1969 p 36). Thus:

> The problem which I tried to solve by proposing the criterion of falsifiability was neither a problem of meaningfulness or significance, nor a problem of truth or acceptability. It was the problem of drawing a line (as well as this can be done) between statements, or systems of statements, of the empirical sciences, and all other elements –whether they are of a religious or of a metaphysical character, or simply pseudo-scientific. (…) I called this first problem of mine the "*problem of demarcation"*. The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations' (Popper 1969 p 39).

Popper's criterion repudiates the sufficiency of induction as a validating method of scientific work, and replaces it with falsifiability. Empirical validation would still count, but Popper believed it should not be the sole guarantor of scientific status. For Popper, deducing universal theories from particular statements would count as a metaphysical belief in the world (Corvi 1997 p 21). So any claim to unrestricted generalisation, to scientific law, or to uniformity of nature cannot be sustained as scientific, even if provisionally, unless it is put to the test of falsification.

By his own account, psychoanalysis played a significant part in the development of Popper's notion of demarcation (Popper 1969 p 34 and p 164). Popper was particularly concerned with Marxism, psychoanalysis, and Adler's psychology since 'these theories appeared to be able to explain practically everything that happened within the field to which they referred' (p 34). As Popper remarked, 'once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it (…) it began to dawn on me that this apparent strength was in fact their weakness' (p 35). Because these three theories could claim an 'incessant stream of confirmation, of observations which "verified" the theories in question' (p 35), they could be deemed scientific by induction. On the other hand, Popper affirms that both psychoanalysis and Adler's psychology were 'simply non-testable, irrefutable. There was no conceivable human behaviour which could contradict them' (p 37). In this respect, these theories could contain insights, but only 'in the manner of myths. They contain most interesting psychological suggestions, but not in a testable form' (p 38). Consequently, Popper felt it would be hard to establish a line separating psychoanalytic statements from the confirmations astrologers find in their practice (Ibid.). From a philosophy of science perspective, then, psychoanalysis could be closer to astrology than astronomy.

Popper's categorisation of psychoanalysis above deserves some further explanation. As Hopkins (2014) warns, to simply discredit Freud – and Darwin for that matter – as pseudo-scientific is a misrepresentation of Popper's argument and intentions (p 3). In *Realism and the Aim of Science* (1985), Popper provided his most detailed account of Freud. There, for example, he deems Freud's *The Interpretation of Dreams* (1900) as being 'beyond any reasonable doubt, a great discovery. I at least feel convinced that there is a world of the unconscious, and that Freud's analyses of dreams given in his book are fundamentally correct, though no doubt incomplete.' (p 164). Later he also writes that 'I do not wish to be misunderstood. I think that Freud's *Interpretation of Dreams* is a great achievement. Yet it is more of the character of pre-Democritean atomism (…) than of a testable science. It certainly shows that even a metaphysical theory is infinitely better than no theory' (p 172). So although Popper did not see Freud's theories as scientific, he did not simply dismiss or discredit them.

5.2 Grunbaum's defence of induction

Adolf Grunbaum's *The Foundation of Psychoanalysis* (1984) has been 'widely received as the most substantial philosophical critique of Freud ever written' (Robinson 1993 p 180). For one thing, he certainly made a more detailed query on psychoanalysis than Popper did and, Robinson notes, that Grunbaum was 'a critic of Karl Popper before he became a critic of Freud' (p 198). In Grunbaum's own words:

> The first impetus for my inquiry into the intellectual merits of psychoanalytic enterprise came from my doubts concerning Karl Popper's philosophy of science, which I published in a series of four essays in 1976. As a corollary, my hunch was that his indictment of the Freudian corpus as inherently untenable had fundamentally misdiagnosed its very genuine epistemic defects, which are often quite subtle (1984 p xii).

With this opening statement to *The Foundation of Psychoanalysis,* Grunbaum engendered a debate within philosophy of science, while opposing Popper's views and understanding of psychoanalysis. This section reviews Grunbaum's critique and alternative assessment of the scientific credentials of psychoanalysis, structured according to the following sections: a repudiation of hermeneutic understandings of psychoanalysis, a critique of Popper's views on psychoanalysis and falsifiability, the development of Grunbaum's tally argument, and finally an assessment of psychoanalytic scientific status according to experimental-basis results.

In his critique of Popper's indictment of psychoanalysis, Grunbaum (1984) argues that psychoanalysis is indeed scientifically sound, at least where the falsifiability criterion is concerned. Robinson (1993) points to Grunbaum's two main arguments contra Popper: The first one, 'the classic example of Freud's changing his mind when the facts obliged him is, of course, his decision to abandon the seduction theory. Grunbaum frequently draws attention to the famous renunciation letter' (p 202). Along these lines, Grunbaum holds that 'Freud practiced what he preached: on several occasions Freud gave up ideas because they proved empirically insupportable' (p 202). Grunbaum's (1979) second main argument against Popper would be provided by instances where psychoanalytic ideas where proven false. Grunbaum (1979), for example, relies on empirical investigations that 'P-falsify' (i.e. meet Popper's standard of conceivable falsifiability), citing studies falsifying aspects of Freud's anal typology, Freud's doctrine of repression as inducing neurotic anxiety, and the nature of dreaming as wish-fulfilment (p 137). Thus Grunbaum concludes that 'by not having come to grips with details of the kind we illustrated, Popper failed to address adequately the issues of the *P-falsifiability* of Freud's psychogenetics. And I trust (…) [I will be able] to show that (…) the Freudian corpus can reasonably be held to be *P-falsifiable* and hence *P-scientific*' (Ibid.).

Having defended psychoanalysis' scientific credentials, Grunbaum develops his own argument: 'I shall say of a theory *T* that "*T* is *I*-scientific" if it qualifies as *inductively* scientific or *well*-supported by the neo-Baconian standards of controlled inquiry' (p 132). In other words, Grunbaum's opinion is that Freud's scientific credentials should be judged based on traditional inductivism. Grunbaum goes on to present his so-called Tally Argument, asserting that Freud makes five claims:

> (i) Denial of an irremediable epistemic contamination of clinical data by suggestion
>
> (ii) Affirmation of a crucial difference, with regard to the dynamics of therapy; between psychoanalytic treatment and all rival therapies that actually operate entirely by suggestion
>
> (iii) Assertion that the psychoanalytic method is able to validate its major causal claims – such as its specific sexual etiologies of the various psychoneuroses (…) without the burdens of prospective studies employing the controls of experimental enquiries
>
> (iv) Contention that favourable therapeutic outcome can be warrantedly attributed to psychoanalytic intervention without statistical comparisons (…)
>
> (v) Avowal that (…) credence can rightly be given to [the patient's] self-observations (…)
>
> (pp 127-128).

The logical implications of these five claims, according to the Tally Argument, are that 'only the psychoanalytic method of interpretation and treatment can yield or mediate to the patient correct insight into the unconscious pathogens of his psychoneurosis' (p 139). Furthermore, 'the analysand's correct insight into the etiology of his affliction and into the unconscious dynamics of his character is, in turn, *causally necessary* for the therapeutic conquest of his neurosis' (pp 139-140). For the purposes of this review, it is important to note that Grunbaum's Tally Argument provides an alternative criterion of scientific demarcation for psychoanalysis. His approach focuses on Freud's clinical theory of cure (Brakel 2015), based mostly on readings of Freud's texts such as his *Introductory Lectures on Psychoanalysis* (1916-1917), and *On the History of the Psycho-Analytic Movement* (1914). The Tally Argument is based on demarcation in tandem with inductivism, as it raises the issue of testing causal claims. Grunbaum eventually critiques psychoanalysis using the Tally Argument as the best measure of Freud's scientifically-oriented credentials. However, the psychoanalytic case for being scientific in Grunbaum's terms fails because, in his view, Freud initially complied with the Tally Argument only to abandon it later. According to Robinson (1993), 'Grunbaum read Freud's late therapeutic pessimism as an implicit disavowal of the Tally Argument. The argument posits a radical dependence of analytic ideas on therapeutic success, but Freud's growing doubts about his ability to achieve genuine and lasting cures effectively stripped the Argument of its essential premise' (p 222). Grunbaum (1984) concludes that 'unless analytic treatment is the paragon of the therapies as claimed in the Tally Argument, Freud himself acknowledged that he cannot be assured of the inherent scientific value of psychoanalysis' (p 172).

Grunbaum would also dismiss psychoanalysis' scientific credentials based on its incapacity to claim that psychoanalysis produces better results than its therapeutic rivals – of which, Grunbaum says, there are 'at least well over 125' (1984 p 161). As a result, he would cast the shadow of the placebo effect on the psychoanalytic processes of cure (p 161, p 165). In this context, Hopkins notes that Grunbaum's critique 'recycled the oldest but also the most influential grounds for rejecting psychoanalytic claims' (Hopkins 2014 p 5) when he argued that 'the kind of confirmatory clinical observations to which Freud refers may well be contained by the influence of the analyst, such as by unconscious suggestions with which the patient unwittingly complies' (Ibid.). Ultimately, psychoanalysis was unsuccessful in attaining scientific status because it complied with a falsificationist framework but failed at the same time to secure a firm causal explanatory framework and establish demarcating claims based on empirically controlled tests of clinical treatment.

5.3 Psychoanalysis and biology

So far, the scientific status of psychoanalysis has been approached *vis-à-vis* its dialogue with philosophy of science. However, the literature on the scientific status of psychoanalysis is not restricted to philosophy. Approaching the subject from a different angle, Sulloway (1979) for example considers psychoanalytic scientific credentials by privileging the history of ideas, through his comparison between Freud's ideas and the biological ideas of his time. In Robinson's comparison between Grunbaum's and Sulloway's discussions of Freud, he notes that:

> Grunbaum's judgement of Freud as a scientist contrasts interestingly with Sulloway's. On first blush, one might expect Grunbaum and Sulloway to adopt similar positions. After all, both approach Freud from a scientific perspective, Grunbaum as a philosopher of science, Sulloway as a historian of science. But instead they offer radically opposite views of Freud's scientific credentials. Sulloway's long book contains not so much as a word to suggest that Freud falls short of the empirical standards of modern science. On

the contrary, Sulloway speaks of Freud as a scientific genius of the first order. (…) the science against which Sulloway measures Freud is historical biology, which necessarily has a more indulgent conception of proof (…) than is generally tolerated in physics' (1993 pp 261-2).

The quote above illustrates Sulloway's approach – through which psychoanalysis emerges as scientific in relation to biology, rather than physics. Instead of a philosophical programme, then, Sulloway's approach relies on unearthing the proximity between Freud's work and biology via an intellectual biography. Sulloway (1979 p 21) admits that Freud being influenced by evolutionary biology (e.g. Litvo 1990) was not a new insight, but his contribution stems from his ability to relate psychoanalysis and biology: 'it is my contention that many, if not most, of Freud's fundamental conceptions were biological by *inspiration* as well as by *implication*' (p 5). According to this perspective, Freud was not the inventor of a pure psychology based on introspection but rather he applied biological ideas to the mind (Wollheim 1979). Therefore the scientific credentials of psychoanalysis would be granted with – and be dependent on – the key notion that Freud was highly indebted to Darwin, within a lineage of evolutionary biologists.

This view is echoed by Sulloway's explicit comparison of Freud to Darwin in his *Freud, Biologist of the Mind* (1979), an ambitious work purporting to be Freud's intellectual biography. Its major point of contention is that the 'Freudian Legend' established Freud's psychoanalysis as pure psychology, ignoring the prominence of biological thinking behind central psychoanalytic notions. In this respect, Freud was almost a biologist who passed as a psychologist. He was, in Sulloway's words, a 'crypto-biologist' (p 3). Supporting this notion, Sulloway initially focuses his attention on the 1890s – a watershed moment in Freud's career (Robinson 1993 p 61) in which he experimented with 'different patterns of explanation' (Wollheim 1979), gave up on his *Project for a Scientific Psychology* (1950 [1895]), broke up with his close friend Wilhelm Fliess, and arguably paved the way to his ground-breaking works of later years (i.e. *The Interpretation of Dreams* (1900) and *Three Essays on Sexuality* (1905)). Counter to the prevailing idea (or 'legend') that Freud transitioned from 19th century neuro-biology towards 20th century psychoanalytical notions of subjectivity, Sulloway asserts that the 1890s would mark 'not the end of Freud's commitment to scientific reductionism, but his conversion from one form of reductionism to another, namely, from neurophysiological reductionism to organic or evolutionary reductionism' (Robinson 1993 p 61). If anything, Freud's psychoanalytic theories became *more* biological, not less so, after the crucial years of discovery (1895-1900)' (Sulloway 1979 p 391).

While it is beyond the scope of this review to detail the development of Freud's intellectual trajectory, Wollheim (1979) notes that the biological argument is at the very heart of the matter in Sulloway's book, as in the chapter entitled *Evolutionary Biology Resolves Freud's Three Psychoanalytic Problems*. Here Sulloway (1979) discusses three problems that were deemed intractable to Freud's pre-1910 thinking: the nature of repression, the centrality of sex, and the choice of neurosis. Sulloway focuses on the first one, which is considered to be the most fundamental cornerstone in Freud's theory of psychoneurosis (p 368). According to Sulloway (1979), Freud 'conceptualized repression (or defence) in terms of the ego supressing certain "incompatible" and generally traumatic ideas' (p 368). He located this argument in relation to Freud's thinking between 1893 and 1897 – the notions of supressed affects and *abreaction* (Ibid.). This phase came to an end once Freud abandoned his seduction theory and questioned his own explanatory schema, 'now realizing that normal and neurotic individuals share similar childhood sexual experiences' (Ibid.), and became concerned with organic 'sexual repression'. According to Sulloway, in the period between 1897 and 1913, Freud considered repression in relation to the acquisition of disgust, shame, and morality (pp 368-9) while navigating tentatively through his theory of neurosis, which he saw as centrally dependent on repression. Sulloway quotes Freud as recorded in the *Minutes of the Vienna Psychoanalytic Society*: 'we assume that there is no repression that does not have an organic core (…). In this organic repression psychic factors as yet play no role (…) The entire theory of the neuroses is incomplete as long as no light has been shed on the organic core of repression' (Nunberg and Federn 1967 p 323 cited in Sulloway 1979 p 369). So Freud's complaint about the incompleteness of his theory of psychoneuroses is closely connected with organic repression (equated to primal repression according to Sulloway (p 369)). In order to rescue psychoanalysis from the apparent intractability of this challenge, he articulated an argument reliant on evolutionary biology. In *Totem and Taboo* (1912-1913), Freud 'greatly expanded the organic theory of repression that he had originally set forth in connection with olfaction and disgust. The ontogenetic acquisition of remorse, guilt, and moral sense now became conceivable to Freud as a phylogenetic precipitate' (Sulloway 1979 p 373). Sulloway establishes similar chronological paths of investigation for Freud's two other challenging questions – why sex, and choice of neuroses – while ending on a similar argument:

> In short, phylogeny was Freud's final answer to many of the difficulties that threatened to undermine his most basic psychoanalytic claims. From the problem of attributing neurosis to phantasies instead of real events, to the issue of just how universal were the psychosexual stages and neurotic complexes that Freud espoused, phylogenetic

> suppositions played a paramount role in legitimating his science of the mind' (p 388).

According to Sulloway, 'it was from his phylogenetic historical conception of sexuality that Freud drew the conviction that *his* system of psychology must be basic to all human societies' (p 391). And so 1913 becomes a central date for Sulloway as it was around that time that Freud formally introduced the notion of phylogenesis in his writings (e.g. Freud's paper on *Schreber* (1911a); *Totem and Taboo* (1912-1913)). Phylogenesis would be key in providing an explanatory framework for Freud's theory of psychoneuroses (Sulloway 1979 p 391). Furthermore, the articulation of phylogenesis consolidated Freud's adoption of an ultimate-causal theoretical framework (as in evolutionary biology) to replace the proximate-causal theory (as in neurophysiology) to explain psychoanalytical findings in scientific terms. Sulloway discusses this epistemological change (p 365) drawing on Mayr's (1961) distinction between different types of causalities in biology. He concludes that Freud's 'appeal to these ultimate causal solutions marks him as a shrewd thinker who fully understood the task of constructing a universal theory of human behaviour' (p 367). Therefore, leaving aside the validity of Freud's anthropological and Lamarckian assumptions in *Totem and Taboo*, Sulloway emphasises that phylogeny was fundamental to Freud (1979 p 391) as it provided an answer to his questions, an explanatory framework, and lent him scientific credentials from biology.

There have been responses to Sulloway's argument as outlined above, usually converging on the unsuccessful argumentation of *Freud, Biologist of the Mind* (1979). Gay (2006) dismisses Sulloway's contribution to the available Freudian scholarship, writing that *Freud, Biologist of the Mind* (1979) arrives 'presenting itself as a great unmasking document but bringing the essentially old news that Freud's theory had a biological background' (p 750). Robinson (1993) provides a similar response, adding that Sulloway's argument for evolutionary biology is a major distortion of Freud's thinking (p 80), over-reliant on phylogenesis (p 84). At the other end of the spectrum, Gould (2006) supports Sulloway's thesis while stating that 'Freud's writing gives no indication that he intended his phylogenetic speculation as anything but a potentially true account of actual events' (p 477). Wollheim (1979) concurs somewhat with Gould (2006), when noting that 'Sulloway is absolutely right to insist against a number of contemporary commentators that for Freud the importance of biology in the psyche was and remained a central tenet' (Wollheim 1979 p 7). Yet Wollheim also provides a fundamental caveat: 'but Sulloway has failed to perceive that the crucial question is, How does biology assert and maintain its importance? It is only if we have an answer to this question that the issue of the extent to which Freud's theory was biological and the extent to which it was psychological acquires an interest' (Ibid.).

Regardless of its faults, *Freud: Biologist of the Mind* has the merit of foregrounding the use of phylogenesis/ontogenesis and biology in Freud's work. So instead of testing psychoanalysis as Popper and Grunbaum did, Sulloway assesses Freud's arguments according to how psychoanalysis devises its own processes of validation, qua biology as a scientific discipline.[18]

5.4 Psychoanalysis in the laboratory

A late addition to the debate on psychoanalysis and science comes from sociological studies of laboratories and scientific activity. Knorr Cetina (1992) introduced the link between the laboratory and psychoanalysis to STS in a paper illustrating some aspects of laboratories and experiments, arguing that 'Freud went some length to turn psychoanalysis into a laboratory science' (p 130) since, in the analytic situation, like in the laboratory, a sense of placelessness is produced, with patients 'disengaging from everyday situations and […] sustaining a new system of self-others relationship with the analyst' (Ibid.). However, Knorr Cetina is more interested in highlighting 'the kind of activity performed in this setup rather than the setup itself' (p 130). She regards psychoanalysis as 'not processing material objects but processing signs; it is *reconstructing the meaning and origin of representations'* (Ibid.). She links this to aspects of laboratory settings – specifically the case of contemporary particle physics laboratories – in which physicists work experimentally with signs from laboratory benches (p 132). As a result, although the analytic setting as a physical site and in its material features (as an office, time bound, isolated place, and involving payment) have previously been discussed from a sociological perspective (e.g. De Swaan 1980), Knorr Cetina's specific contribution is to bring psychoanalysis closer to laboratory studies within STS.

Isabelle Stengers delves more deeply into Knorr Cetina's analogy between the psychoanalytic setting and the laboratory (Stengers 1997 p 95), focusing on the question of scientific demarcation of psychoanalysis. In her *Black Boxes; or, is Psychoanalysis a Science?* (1997), Stengers divides her essay into two main parts. In the first, she presents major aspects concerning her understanding of the

[18] His coinage of Freud as a biologist of the mind is provocative, but not concerned with answering whether psychoanalysis is a science. It is rather a question of how Freud thought scientifically at that time. Yet, simply equating can be very problematic, and reductive. A more productive question would be to ask for the relevance, from today's perspective, of this usage of outdated biology.

constitution of scientific activity. In the second, she discusses the scientific status of psychoanalysis according to her views on demarcation.

Stengers notes that 'one of the important characteristics of the activities that are called "scientific" is that they lead human beings to work "together" in a totally different way' (1997 p 79). From this initial observation, she posits that working together is a crucial achievement. It entails the recognition of scientific innovations as 'truly creations, which produce the criteria on the basis of which the accepted innovations will be described a posteriori as "obviously" scientific' (p 81). Stengers' understanding of 'working together' sanctions neither a realist nor a social constructivist reading of science, nor does she agree with imposing normative or general criteria of demarcation such as Popper's and Grunbaum's. Instead, she upholds the importance of scientific activity as a self-defining process, creating its own norms and statements (p 81). From this vantage point, Stengers looks further afield for defining features of sciences: 'the real issue is actually the invention and production of (…) reliable witnesses' (p 84). According to Stengers:

> All the phenomena that we know of are overloaded with multiple meaning, capable of authorizing an indefinite multiplicity of readings and interpretations, that is, of being utilized as evidence in the most diverse situations and thus also of being disqualified as evidence. The whole question is thus, for the scientist, to produce a testimony that cannot be disqualified by being attributed to his or her own "subjectivity", to his biased reading, as testimony that others must accept' (p 85).

So the reliable witness takes on the crucial role of providing 'testimony that others must accept', eliminating subjectivity, while articulating consensus and the capacity to work together. In other words, the witness produces a testimony that cannot be disqualified, and so constitutes scientific demarcation. In establishing a witness as reliable, Stengers borrows Latour's notion of the *black box*: 'a black box establishes a relation between what enters it and what leaves such that no one has, *practically*, the means to contest it' (Stengers 1997 p 85).[19] Put another way, science is the activity wherein new ways of working together can reliably be established through an original process. Scientific demarcation, then, takes place according to the success of this new way: the more black boxes that can be established in relation to the original manner of witnessing, the more convincing and undisputed a scientific activity becomes. Concomitantly, this novel way of working together becomes ever more influential and eventually forms a consensus within a whole community of

[19] This definition of black box is in tandem with Latour's general notion of black box, even though there is more to his definition than just this. See glossary, and also Section 5 in Chapter 3.

scientists. Therefore, demarcation relies on the capacity to impose such a process as uncontested, forcing acceptance by others.

With her definition of scientific demarcation, Stengers turns to psychoanalysis. She sees it as a challenge, since it moves away from 'hard sciences', but indicates the initial conditions for a potentially original path of psychoanalytic demarcation. She starts the second half of her essay positing that there exists in psychoanalysis 'a very curious "black box": the analytic scene itself' (p 91).[20] As a result, 'the analytic scene appears to create those who will have the right to speak about it' (p 91), and so leads to a 'profound singular state of affairs' (ibid) where the psychoanalytic capacity to work together is concerned. That is, in contrast with the 'technoexperimental devices' (p 91) of hard sciences, the problem of psychoanalysis as scientific entails 'learning to work together without this togetherness being centred on the production of objects, on the closing of black boxes; learning without being devoted to a faith in the guaranteed repetition of the event that opens up an intelligible world or in a method that is supposed to guarantee the agreement of the interested parties' (p 89). Therefore, demarcation of psychoanalysis entails the challenge of understanding how to work together without being centred on a capacity to see ('production of objects'), without the use of experimental devices (traditional black boxes), and without recourse to experimental replication ('guaranteed repetition') as they normally take place in hard sciences.[21]

However, Stengers does not exempt psychoanalysis from the challenge of producing its own reliable witness and black box. Where scientific demarcation is concerned, she is pointing instead to the way that psychoanalysis attempts to establish a science otherwise. In order to develop her argument, she turns to chemistry as the best analogy for the scientific demarcation of psychoanalysis. First, she invokes the chemical analysis-psychoanalysis analogy after Freud's own cue (p 93), writing that 'contrary to quantum mechanics or relativity, the reference to analytic chemistry cannot have bearing on a manner of description, a theoretical content, a lesson concerning the limit of our knowledge or their objectivity, but on an *operational technique'* (p.93). With this analogy to

[20] By calling the analytic scene a very curious 'black box', I understand that Stengers (1997) does not regard the question of scientific demarcation of psychoanalysis beyond the closing of black boxes because she indeed invokes the need for one, albeit 'curious'. In this sense, the fate of psychoanalytic demarcation would remain connected to the effective closure of such a black box.

[21] The preposition *without* as repeated in this quote does not lead to a psychoanalytic quest for demarcation beyond the closing of black boxes. *Without* should be understood as the need for psychoanalysis to relate to black boxes in a different way. That is, *without* the terms in which mainstream sciences establish themselves.

Lavoisier,[22] then, Stengers highlights that both activities follow a similar path of demarcation, centred on techno-practical innovation.[23]

Turning to Freud's papers on psychoanalytic technique, Stengers understands that he, with a remarkable clarity in terms of experimental control (p 104), devises a new technique centred on transference. As a result, Freud would claim for psychoanalysis a great deal of clinical control, since transference is dealt with inside the isolation of the setting, while also being isolated from external factors. 'Thus, transference enables Freud to substitute for the ordinary illness (…), a laboratory illness that refers only to the pure framework of the analytic scene' (p 95). Also, 'transference enables the substitution of the uncontrollable illness by an illness whose transformed symptoms convey, to the analyst's ear, reliable evidence about what they express' (ibid). Furthermore, in her comparison with 18th Century chemistry and Lavoisier, Stengers points to Freud's operational technique in analogy with the chemist's capacity to deal with purified primary material[24] (p 96). It would be through a comparable innovation – Lavoisier's and Freud's technique –that 'the analytic scene has in this way become the quite singular *laboratory*' (ibid) where transference-neurosis is dealt with.

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[22] Lavoisier was an 18th Century French chemist and is often referred to as father of modern chemistry. He is regarded by Chertok and Stengers (1989) as redefining chemistry while helping to demarcate it. Lavoisier would be the first chemist to observe a chemical phenomenon in isolation (p 10), in terms of 'pure' reactants, in contrast with previous chemists that only worked with raw materials (p 51). Unlike classical mechanics, chemistry elements could not 'be defined by intrinsic laws in the same way as falling bodies or the earth turning around the sun' (p 11). Furthermore Lavoisier did not know 'why a particular reaction took place, why a particular compound was possible, why a particular reagent decomposed it' (p 50). Differently from physics, then, Chertok and Stengers (1989) understand that 'the object of the new chemical science would need to be defined by *operational boundary conditions*, which would guarantee that nothing placed in the experimental enclosure would escape it, and that everything remaining after a reaction was the product only of that reaction' (p 11). So Lavoisier did not found chemistry on explanations of how chemical reactions work but rather on its capacity to isolate and purify materials (p 56). Consequently, it is also through his new technique that chemistry could establish reliable operational control (pp 48 – 9, pp 53 -4), that led to the demarcation of 'modern" chemistry'.

[23] In terms of analogy, the discussion on demarcation concerning Lavoisier and the transition from 'old' to 'modern' chemistry offers a backdrop in Stengers (1997). So it is building on her argument for a science founded on its operational techniques as chemistry that she discusses clinical technique as introduced by Freud in psychoanalysis. On this basis Stengers establishes a parallel: the passage from 'old' to 'modern' chemistry, to be compared with the changes from hypnosis to psychoanalysis. If Stengers' discussion focuses on the question of demarcation of psychoanalysis, her debate is heavily informed by Chertok's ongoing considerations on the hypnosis-psychoanalysis relationship (e.g. Chertok and Saussure 1979).

[24] With her focus on the analytic scene and the use of transference for therapeutic purposes, Stengers develops her analogy argument between chemistry and psychoanalysis. So, while Lavoisier deals with phenomena that are overloaded with multiple meaning in chemistry through control and production of 'pure' reactants (Chertok and Stengers 1989 p 50), Freud devises the analytic setting that no longer deals with real life neurosis, only an 'artificial illness' (Stengers 1997 p 96) that is produced and should be controlled in the analytic setting. Therefore Freud substitutes 'the ordinary illness (which clearly involves the analyst, but to the same extent as any other character in the real life of the patient)' (ibid) by a controllable and "pure" illness that is dealt with in the isolation of the analytic scene, and is analysed as a transference-neurosis' (ibid).

Stengers praises Freud's lucidity and ingeniousness. Yet, if he articulates a valid and original demarcation strategy for psychoanalysis, Freud inevitably failed to justify the scientific credentials of psychoanalysis. Stengers maintains two necessary conditions regarding the demarcation of psychoanalysis as a science: first, the operational technique behind transference connected with the successful creation of a reliable witness[25] in the setting and, second, the effective power of 'cure' stemming from psychoanalytic practice.[26] Psychoanalysis could only become successfully

[25] The reliable witness in psychoanalysis could be defined as the analytic patient (Stengers 1997 p 98), his (or her) human psyche (Chertok and Stengers 1989, p 227) or the unconscious in the service of knowledge (p 272), in a controlled environment.

[26] Chertok and Stengers (1989) discuss the scientific demarcation of psychoanalysis in relation to their analogy with chemistry. They read the demarcating transition from 'old' to 'modern' chemistry according to a new capacity to work together centred on technical and practical issues (see note 19). Furthermore 'modern' chemistry would be founded according to a double condition: a capacity to establish a new problem, a new object of study, new ways to see and articulate; and a capacity to identify the products of isolated chemical reactions (p 49), reproducing them in a controlled manner, and confirming 'proposed diagnoses about their identity and the rules of their compositions and decompositions' (p 54). In other words 'modern' chemistry would be founded according to a double condition: purification and control. Meeting these two requirements, 'modern' chemistry succeeds in breaking apart from 'old' chemistry by founding itself on its own terms, and unlike the mechanistic causality of physics (p 49).

In the case of psychoanalysis, the double condition for a successful scientific demarcation is specified right at the beginning of Chertok and Stengers (1989). It concerns the need to reconcile two requirements: 'that of "curing" the patient' (p xx), and 'to cure because, in one or another, the "reasons" for suffering become an object of knowledge' (ibid). In keeping with the analogy with Lavoisier and the technical-practical reading on 'modern' chemistry, psychoanalysis would have to be able to provide instrumental means to purify its object of study (see note 24) while also being able to control and sustain claims of therapeutic effectiveness in connection with its instruments. One condition is met, and is explored by Stengers (1997) in relation to Freud's capacity to devise the analytic setting and establish the transference as a central aspect of psychoanalytic technique innovation. As Chertok and Stengers write: 'Freud's stroke of genius was to make resistance and transference (…) into *generating principles* of the new technique, principles that would, by their very application, transform patients into purified, reliable subjects' (p 55). However, a second condition (that of 'curing' the patient) is also necessary but not met:

'In order for psychoanalysis to become a scientific technique capable of making research and therapy converge, it was necessary that the truthful decoding of mechanisms of the unconscious have effect on the human psyche that were distinguishable from those of suggestion. Then, and only then would psychoanalysis become a professional knowledge capable of taking its place with other scientific knowledge' (p 273).

As evidenced in the quote above, therapeutic effectiveness ('effect on the human psyche') is a necessary condition for psychoanalysis to become scientific. Meeting just one condition is not enough to, in the author's views, demarcate 'modern' from 'old', or psychoanalysis from hypnosis. In order to reach this conclusion, Chertok and Stengers (1989) follow Freud's papers on technique, mainly in relation to the effectiveness of the analytical transference in terms of therapeutic results ('cures'). They point out the triumphant tones upheld in 1910 by psychoanalysts, and quote Freud: 'when we *know* all that we now only *suspect* and when we have carried out all the improvements in technique (…), our medical procedures will reach a degree of precision and certainty of success which is not to be found in every specialized field of medicine' (Freud 1910a, *In:* Chertok and Stengers (1989) p 62). However, this initial optimism concerning the therapeutic effectiveness of psychoanalysis is short-lived. By 1918, Freud starts asking himself: 'is the analysis of the transference sufficient?' (Freud 1918, *In:* Chertok and Stengers (1989) p 62). Along these lines, Chertok and Stengers (1989) conclude that the technical-practical strategy of replacing ordinary neurosis with transference neurosis, 'which was supposed to put illness in the service of knowledge and make it accessible to analytic intervention, is not all-powerful' (p 66).

For Chertok and Stengers, without the therapeutic power of analytic intervention according to the original technical-practical arrangement centred on transference, psychoanalysis would lose its capacity to explain and control how 'cure' demarcated as scientific by satisfying both of these conditions. However, for Stengers, Freud only provides a satisfactory answer to the first condition, pointing to *Analysis Terminable and Interminable* (1937), where 'Freud draws up the list of reasons for the ineffectiveness of the analytic technique' (1997 p 98). Pointing to the psychoanalytic incapacity to uphold the effective power of "cure" stemming from psychoanalytic practice, she notes that 'the transference-neurosis is not sufficient, Freud recognizes in 1937, to put neurosis "at the service of knowledge", and to make it "accessible to the interventions" of the analyst' (ibid). Consequently, without the fulfilment of the second condition of Stengers' double requirement, psychoanalysis fails to demarcate itself scientifically because it does not 'have the hoped-for power necessary for constituting patients as reliable witnesses, as witnesses whose intelligible and calculable cure could confirm the validity of the theory' (p 98). In other words, for all Freud's lucidity and the promising analogy between chemistry, the laboratory, and the analytic setting, 'the cure in the original sense that Freud had given to it' (p 100), prior to 1937, would no longer be attainable. Therefore, from Stengers' techno-practical perspective, the lack of a clear curative power due to the limitations of transference constitute a breach, standing in the way of the successful demarcation of psychoanalysis as scientific.[27]

6. Conclusion

With the initial sections of this chapter, this review indicated several approaches to explaining psychoanalytic controversies, highlighting that none of them explore the term 'science' and its related notions to account for their role in controversies. Consequently, psychoanalytic controversies, their dynamics, and outcomes have been explained mainly in terms of personal rivalries, a clash of ideas, power struggles, ideological struggles, group dynamics, Freud's legacy,

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takes place. With *Analysis Terminable and Interminable* (Freud 1937) in 1937, 'Freud himself recognized the intrinsic limits of the instrument he had developed' (Stengers 1997 p 98), while drawing up 'the list of reasons for the ineffectiveness of the analytic technique' (ibid). Without the control on 'cure' along the lines of the original instruments as specified by Freud, he needs to introduce other variables explaining the relative failure of psychoanalysis in this respect. For Chertok and Stengers, this is a decisive step where their question on demarcation is concerned because psychoanalysis can no longer claim the capacity to produce 'cure' in a controlled manner (unlike Lavoisier who managed to claim and control in chemistry, for example, the law of conservation of mass) while other explanatory variables beyond the realm of transference need to be introduced (see for example Quinodoz (2006) pp 256-260 for a list). After 1937, then, 'all resemblance to the profession of chemist or the modern surgeon has disappeared. As incomplete as it might be, theory is given, but technique has become an art once more, or an artisanal technique, in the sense that it can no longer claim, even as a right (…), to control what it deals with (Chertok and Stengers 1989 p 118)'.

[27] To demarcate itself from hypnosis, psychoanalysis needs to meet the two conditions discussed in the previous note. Failing that, psychoanalysis can no longer make *tabula rasa* (Stengers 1997 p 94) of hypnosis, for example, and demarcate itself as scientific.

psychological explanations and re-readings. The available literature on the Controversial Discussions follows the same pattern, even though ‘science’ is repeatedly invoked during the Controversies. Furthermore, none of these approaches address the implications of stalemates and impasses in relation to a problem of internal demarcation, and how psychoanalytic notions are produced.

The four distinctive approaches discussed in the previous section have various implications for demarcation and, consequently, the scientific credentials of psychoanalysis. I have reviewed Popper’s criterion of falsification, Grunbaum’s criteria related to scientific causality, Sulloway’s philosophical basis for science through a history of ideas, and Stengers’ reliance on the successful creation of reliable witnesses and a capacity to control in clinical work.

With these four different approaches, psychoanalysis’ scientific status is either negative or inconclusive. Popper’s negative answer led to an ‘epistemic deadlock about the significance of clinical data’ (Hopkins 2014 p 6; see also Fisher and Greenberg 2002). Against the backdrop of this impasse and general pessimism, it is perhaps no wonder that a substantial approach engaging the term ‘science’ to psychoanalytic controversies has not been more foregrounded. At best, a discussion on science and psychoanalysis would cast doubts on psychoanalysis as therapeutically effective and, at worse, would deny its scientific status. However, Stengers (1997) has already argued that, even if psychoanalysis fails to achieve scientific status, it should not be regarded as scientism. From an ANT perspective, I follow a similar distinction: regardless of the scientific status of psychoanalysis, it is still possible to consider the process by which knowledge is produced in psychoanalysis, and to take seriously its aspirations to establish knowledge, truth, and facts.[28] This approach is clearly distinct from those in the existing literature on psychoanalytic controversies.

Popper’s, Grunbaum’s, Sulloway’s and Stengers’ demarcation criteria are mainly concerned with the general scientific status of psychoanalysis in relation to other scientific fields. They do not offer criteria for demarcation that takes place internally.[29] Popper’s criterion of falsification does not lend itself to differentiating valid from rejected claims, applying essentially to all psychoanalysis without allowing for differentiation among psychoanalytic claims. Grunbaum’s concern with therapeutic efficacy similarly does not allow for differentiation between Klein and Freud in the context of the Controversial Discussions. Sulloway’s approach to simply equate psychoanalysis with biology only transports the problem of scientific demarcation into the realm of biology, instead of addressing it. Finally, Stengers’ views on the transference as a criterion for demarcating psychoanalysis from suggestion cannot be applied to internal demarcation since both Klein and Freud posit the use of

[28] See note 23.
[29] See also Chapter 1, Section 2.

transference. If anything, Klein is accused of being more radically dependent on transference than Freud. So they cannot be fundamentally differentiated in Stengers' terms in this sense since her criterion is based on the isolation and use of transference that psychoanalysis does adopt and hypnosis does not. [30] Rather, I find the most effective and appropriate tools for discussing internal demarcation of psychoanalysis with STS and especially ANT.

My overarching question leads this research to address fragmentation, pluralism, and internal disputes in psychoanalysis rather than focussing on its scientific status. Yet, unlike existing approaches, I am interested in addressing this subject by systematically considering how knowledge, truth, 'science', and facts are used in this context of dispute. For all its idiosyncrasies, psychoanalysis is profoundly reliant on empirical observations – whether one calls it scientific or not – and this central issue somehow fails to be part of explanations of psychoanalytic controversies. I see my thesis as bridging these two aspects: a controversy being illuminated by the process of knowledge production in psychoanalysis (and in relation to facts and truth), while the term 'science' acquires meaning in relation to the process of knowledge production in the context of a controversy. My hope is to achieve a better understanding of the process of knowledge production in psychoanalysis by the end of this thesis, as well as its part in the dynamics of internal demarcation at the Controversial Discussions. Furthermore, this thesis should yield a systematic way to think about truth, fact, and the use of 'science', informing the process of fragmentation that has occurred in psychoanalysis since Freud's death.

[30] There are acknowledged differences in the usages of transference at the Controversies. However these were not used to demarcate differences between Klein and Freud. See Chapter 5, Section 5.

Chapter 3: Theoretical Framework

This chapter establishes a theoretical framework based mainly on ANT, focussing on the production of facts as the criterion of internal demarcation. Furthermore, this theoretical framework helps to account for the dynamics of demarcation at the Controversial Discussions according to the specific issues raised, and in relation to the process of fact production in psychoanalysis in general. I begin with a background in STS in order to provide a context for ANT; although the latter is a distinct approach, it developed out of the former.

1. The *Structure of Scientific Revolutions:* a background to Science and Technology Studies (STS)

Science and Technology Studies (STS) first took shape as a field of inquiry in the 1970s, offering a novel approach to studying sciences by formulating alternative questions to those provided by philosophy of science. Some commentators even regard STS, in contradistinction with traditional names such as Popper, Stuart Mill, and Francis Bacon, as the liveliest method of investigating science[31] in more recent years (Hacking 1999a p 186). While philosophy has mainly approached science according to a normative question on scientific status, Thomas Kuhn's *Structure of Scientific Revolutions* (1970) has been received within STS as both a 'dramatic break' (Turner 2012 p 474) and a fundamental inspiration (Golinski 2005). Indeed, *Social Studies of Science* – the main academic journal for STS – issued a special edition in 2012 celebrating the fifty-year anniversary of Kuhn's *Structure of Scientific Revolutions* (and the twenty-five year anniversary of Latour's *Science in Action* (Sismondo 2012)).

Kuhn's concepts have been very influential on STS, with his notions of paradigms (Sismondo 2012 p 415), incommensurability, scientific revolution (Collins 2012 p 421), and scientific communities (Golinski 2005 p 13). He provided the starting point for new agendas in studies of science, such as the one sparked by Bloor's 'Strong Programme' (Bloor 1976; Golinski 2005) and subsequently followed by other developments (e.g. Collins 1985; Latour and Woolgar 1986). But there are quite

[31] To the best of my knowledge, there is no significant attempt at defining what science is in the STS literature. In fact, it may not even be a point of concern (e.g. Hess 1997). Furthermore, there is no unifying theory in STS (Fuller 2006), and there is consequently no unified method with which to approach a scientific controversy. What is more, the piecemeal approach to each controversy according to its own terms and different approaches resists the idea of regarding scientific activity in terms of Science (with a capital letter). STS and ANT would arguably speak in the name of a multiplicity of sciences (e.g. Latour 2004b).

mixed views about Kuhn's perennial influence in STS. For example, it has been noticed that 'most of Kuhn's large-scale historiographical and theoretical claims have been abandoned by STS, or were never widely taken up' (Sismondo 2012 p 416). For example, Fuller (2000) investigates Kuhn's macro-historical approach, which draws on examples from a span of three hundred years within the history of natural sciences in Europe (p 23) and observes that 'it is impossible to find a historically extended episode that exemplifies [Kuhn's] entire cycle' (p 21), wherein normal science undergoes a crisis after which a new paradigm emerges. Fuller further argues that it is 'problematic to treat Kuhn's model as an ideal type (…) because *Structure's* syncretism combines aspects of different periods in the history of science that have sociologically excluded each other, thereby rendering his model empirically incoherent' (p 23). As a result, Kuhn 'never tells us the social processes by which members of an actual scientific community came to perceive the accumulation of anomalies in their paradigm as constitutive of a crisis' (p 21).

On one hand, Kuhn's account of the movement from normal science to revolution (1970) is regarded in STS as opening the way for attempts to explain 'scientific action' (Sismondo 2012 p 416),[32] crediting him with 'the founding proposition that science should be studied like other aspects of human culture' (Golinski 2005 p 5). On the other hand, STS has dismissed Kuhn's grand narrative while focusing on a significantly reduced scale of study (Fuller 2006 p 22). In eschewing Kuhn's grand narrative, sociology of science has not only skirted philosophy of science's normative stance but has also moved towards 'more empirically adequate accounts of what scientists do' (Fuller 2000 p 14).

As a result, a constitutive trademark of STS is its focus on 'micro' studies. These studies can be either historical (e.g. Schapin and Schaffer 1985; Latour 1988) or directly observed (e.g. Latour and Woolgar 1986; Collins 1985), but in both cases they are more situated in time and space and grounded in more defined situations than Kuhn's study. Accordingly, STS pursues scientific action closely in dialogue with, but also distant from, Kuhn's account that made it dangerously easy 'for all manner of inquiries to reinvent themselves as paradigms' (Fuller 2000 p 320). What I think is very relevant for this thesis is the fact that STS opens the way to a research agenda that is interested in scientific activity, while bypassing the more philosophical question of scientific demarcation and normativity. It is precisely this attention to situated sites of scientific disputes that provides an entry point to delve into the Controversial Discussions and its process of internal demarcation. Next, I will discuss ANT as containing a subset of STS tenets while also being a distinct development. In my view,

[32] Action is an important term in ANT, as it relates to a specific meaning of practice (Latour 1987).

this offers the best way to investigate the delicate question of how claims on the workings of the mind are validated and incorporated as knowledge in psychoanalysis.

2. Studies of scientific knowledge in controversies

Scientific knowledge in situations of controversy became an object of STS with Bloor's 'Strong Programme' in the late 1970s. Bloor's aim was to precisely investigate, within STS, how scientific knowledge was shaped, and 'how this shaping contributed to the dynamics of controversies' (Pinch and Leuenberger 2015 p 5). One of the key features of Bloor's programme is a methodological principle of symmetry (1976 p 5). Heavily influenced by Kuhn (Bloor 1976; Golinski 2005), Bloor's programme consisted in developing the idea that there is no such thing as a crucial evidence-based experiment that definitively resolves scientific controversies and makes scientists converge immediately (2015). With this idea, Bloor questions the internalist account of science because it relies on the existence of 'crucial experiments', in order to render scientific activity self-sufficient and autonomous, since 'an inductivist methodology would perhaps stress the emergence of theories out of an accumulation of observations' (1976 p 6). This view of science presupposes a teleological pursuit towards truth and knowledge[33] (p 8), limiting the role of sociology to explaining the wrong or irrational side of scientific disputes.

Indeed, the assumption behind the 'Strong Programme' is that scientists understand nature collectively, through society, not in spite of it (2015 p 593), and claims that both sociological and scientific aspects operate together within controversies (p 595). Bloor was in agreement when 'Kuhn argued that cognition within a group of scientists is coordinated by reference to a shared exemplar or paradigm, that is, a concrete achievement, recognized by the group' (p 593). In other words, in a situation of dispute, a claim needs inevitably to be negotiated within a scientific community and recognised socially in order to be validated as truth; knowledge and its process of production both need to be endorsed (p 3). Claims asserting truth or falsity are contingent on a social agreement, rather than being an independent and teleological pursuit of truth.

Within this framework, scientific controversies are a good field of investigation because, 'with each side in a dispute claiming to have "truth on its side", and disparaging the efforts of the other, and with the very outcome unknown' (Ibid.), it would be very difficult to apply a sociology of error or an internalist account of truth. By its very nature, a controversy is a situation of uncertainty and an

[33] The terms 'truth', 'validated claim', 'knowledge', 'fact', 'objective knowledge' are used synonymously here, within an ANT framework. Of course, they may have different meanings when used in other frameworks. See glossary.

attempt at the internal demarcation based on the process wherein truth (or a fact) eventually becomes established. The status of uncertainty that defines scientific controversies is a fundamental insight incorporated by ANT, which I will apply to the Controversial Discussions.

Given that uncertainty is an essential aspect of scientific controversy, one important development of Bloor's programme is his methodological principle of symmetry (1976 p 5), which requires sociologists to 'use the same explanatory resources to explain both successful and unsuccessful knowledge claims' (Pinch 2001 p 13721). So the principle of symmetry establishes a methodology that examines the very processes wherein claims become differentiated as true or false, without allusion to crucial experiments and simply relying on evidence-based control.[34] In other words, sociology of science would no longer leave scientific activity to science alone. From this perspective, scientific controversies become a focal point of attention because they represent situations wherein 'the symmetry principle can be applied to good effect' (Ibid.).

Whereas Bloor's theoretical programme has been applied mostly from a historical perspective (e.g. Bloor 1976; Schapin and Schafer 1985), Collins (1985) approaches more contemporary controversies. A synthesis of several case studies reveals that one of Collins' major findings is what he calls the 'experimenter's regress'. For example, he studied a debate around gravitational waves – a controversy at the research frontiers of knowledge (Pinch 2001 p 13721). The physicist Joseph Weber claimed to have detected gravitational waves through his experiment with a gravity wave antenna (p 79) – a claim which was universally disbelieved (p 81). But at the time of his assertion, it became a controversy in need of confirmation. According to Collins (1985), scientists can decide this matter based on potential repeatability, or experiments replicated by other scientists, which test assertions in practice and 'act as a demarcation criterion for objective knowledge' (p 19). However, scientists had difficulty corroborating the original findings, and given the complexity of building an apparatus and all the practical aspects surrounding it, there was always room to question whether a proper replication had taken place or not. The 'experimenter's regress' attempts to capture exactly this: the impossibility of 'specifying in advance all of the conditions that must be met for an experiment to be conducted successfully' (Golinski 2005 p 29). In other words, the regress describes a loop wherein '"facts" can only be generated by "good" instruments, while "good instruments" can only be recognized as such if they produce "facts"' (Godin and Gingras 2002 pp 137-8). Consequently, it becomes impossible to break this cycle if there are no criteria, independent from the experiment outcome, with which to decide the disagreement. According to Collins (1985): 'we

[34] The problem is not to isolate one from the other, but to show that processes rely on context, social factors, etc. See Bloor's comments on this matter (Bloor 2015).

won't know if we have built a good detector until we have tried it and obtained the correct outcome! But we don't know what the correct outcome is until… and so on *ad infinitum.'* (p 84). Considered from this perspective, the establishment of a scientific fact (together with the status of 'good' experiments and good practice) is the main aim of a controversy, not a teleological presence, with prior existence, and just waiting to be found.

A major implication of Collins' notion of 'experimenter's regress' is that experimentation alone is an insufficient criterion for internal demarcation in scientific activity because it does not automatically lead to the establishment of a fact. So his studies that closely followed scientific action rebuffed the internalist understanding of scientific matters as self-sufficient. One of Collins' main points is that, given the complexities and practicalities involved in setting up an experiment (as with Weber's gravitational antenna), any subsequent experiment will 'always differ in some respects from the original' (Golinski 2005 p 29), and therefore 'there is always room to argue that it differs in some relevant aspect, which makes it not a fair comparison' (Ibid.). In other words, Collins (1985) understands that replication lies at the heart of internal demarcation. Yet, once scientists turn to their replicating experiments, it will always be possible to question the validity of repetition (ibid). Consequently, the closure of a controversy and the validation of scientific knowledge depends not just on experiments but also on social means negotiated within the network of scientists (Collins 1985). Furthermore, scientists need ultimately to decide, as a group, which experimental results to trust, and to which to lend their authority (Collins 1985 p 151-9; Golinski 2005 p 29). They somehow need to agree on what is considered a validated experiment, specifying its outcome and facts. In this respect, internal demarcation relies on scientific activity, but also on, and always together with, social activity.

Bloor and Collins share the understanding that controversies are situations of openness, where scientific action is taking place within uncertainty. In other words, facts are not established at the beginning of controversies, but are the desired outcome of controversies. Furthermore, Bloor and Collins both focused on the idea that controversies are not concluded by way of experiments, independent facts and truth, but emphasise the role of social activity – and the need to reach consensus – as a fundamental and necessary aspect of scientific activity. As a result, they agree that a fact is what ends a controversy, providing internal demarcation, with their accounts concluding that scientific and social activity are intertwined, and both are necessary for scientific knowledge to be established.

The STS tradition also views controversies within the context of a knowledge frontier, where truth cannot be the arbiter since the truth is itself being disputed. At the same time, a controversy can be regarded within a continuum of scientific activities, and in close connection with the laboratory (e.g. Collins 1985; Schapin and Schafer 1985; Latour and Woolgar 1986). In this respect, although controversy and laboratory studies emphasise different aspects of scientific activity, they are not separate entities. These notions are useful for framing my understanding of the Controversial Discussions, especially using insights from Latour, who develops his own understanding of controversies through an ANT perspective.

3. **ANT and STS**

Since STS focusses on scientific activity, observing what scientists do, STS has been applied to controversies that raise relatively few issues in terms of their scientific credentials, as in the cases of physics (gravitational waves, quantum physics) and molecular biology (DNA models, peptides). Yet, without subscribing to a prescriptive account of science (Zammito 2004 p 123), STS's foundations depend on making case studies out of controversies that are implicitly assumed to be scientific.[35] One cannot do the same with psychoanalysis.

STS's emphasis on social elements, in contrast with internalist accounts of science, pose significant challenges for this study. Bloor's and Collins' investigations on controversy are in their origins based on countering internalist understandings of science via the introduction of a social dimension to scientific activity (1985). However, in the context of this thesis, how could the pursuit of facts be applied to psychoanalysis, considering that its status as scientific is, at best, problematic? Without the 'solid' and 'pre-established' scientific side upon which social explanation is added to explain the production of knowledge in science, there is the risk of reducing the Controversial Discussions simply to social matters.[36] As discussed in the Literature Review, the Controversial Discussions have already

[35] There needs to be an implicit understanding of what science is, otherwise it would be impossible to even select a case study at all. Yet, STS bypasses this question in order to focus on knowledge production, as does ANT.

[36] The STS strategy is to question internalist accounts of science (Merton 1957) by adding an externalist layer to it. So STS takes as a point of departure the existence of scientific activity and argues that there is a need to add social aspects to the production of scientific knowledge (e.g. Collins 1985). Since the STS emphasis is on adding a social layer of explanation to scientific activity, this becomes a very complicated move where psychoanalysis is concerned. On one hand, this dual-layer approach cannot be applied to psychoanalysis because its grounds as scientific are highly-questionable (see Chapter 2) and needs to be addressed previously and beyond an implicit understanding of what science is (see note above). On the other hand, there is already available in the current literature on the Controversial Discussions a number of approaches exploring the social variables at stake (see Chapter 2). What remains missing is an account addressing the presence of the term 'science' (see Introduction Chapter) and the processes involved in the production of psychoanalytic knowledge that are ignored once the explanation of psychoanalytic controversies relies on social factors (see Chapter 2; also, this is the Latourian general argument for leaving what he calls as the 'sociology of the social' aside and using instead his 'sociology of associations' (Latour 2005 p 95)).

been analysed according to a range of social factors, such as group dynamics, psychological influence, paradigms, power struggle, and institutional interests addressing the Controversies. Of course, it is possible to expand on the social understandings of the Controversial Discussions, and also to argue that the aforementioned approaches do not overlap with social explanations in STS. However, as identified in the Introduction and Chapter 3, the novelty of the argument in this thesis is accounting precisely for the term 'science' in the psychoanalytic literature on controversies and the Controversial Discussions. Along these lines, STS can help to emphasise the social explanation of psychoanalysis. But its emphasis on the social does not address how to take up 'science' at the Controversies, and justify its approach in the case of psychoanalysis. Unlike STS, I think ANT addresses these points in relation to the Controversial Discussions. Furthermore, ANT was developed in response to a sense of theoretical crisis in STS (Zammitto 2004; Law 1986).[37] With its sociology of associations, ANT deemphasises the role of traditional 'social agents', and allows me instead to focus on the full range of actors taking part in the process of psychoanalytic knowledge production.

The question of psychoanalysis as a 'scientific activity' is not a problem in ANT because Latour's approach does not depend on adding 'society to science' (or externalist to internalist account of science, as in STS). ANT is regarded as part of what Rouse (1992) calls the cultural studies of science. In this context of challenge of dualisms, Latour's approach tries to blur the very science-society division (Latour 1993), where scientific asymmetry is taken as an outcome rather than an initial condition (Latour 2004b)[38]. At the same time, psychoanalysis can be investigated here in the name of a multiplicity of different types of truth production (Schmidgen 2014 121), in the name of sciences (in the plural and without capital letters), and blurred divisions that psychoanalysis can be investigated here – regardless of its status as scientific or not – as one field of inquiry struggling in its own right to produce facts and stabilise its claims according to its own particularities. Unlike with STS, an ANT approach has no pre-requirement of a scientific-social duality as a starting point. Categories are much looser (science, religion, politics, legal system), and often mixed (Tresch 2013).

4. ANT's specific approach to scientific controversies

ANT borrows STS's understanding of controversy is a moment of openness, but provides an alternative theory (Pinch 2015 p 284). Latour discusses scientific controversies in great detail in

[37] The main issue of this crisis is described in Zammito (2004, p 166).
[38] As a result of a purification (Latour 1993).

Science in Action (1987), asserting that 'uncertainty, people at work, decisions, competition, controversies are what one gets when making a flashback from certain, cold unproblematic black boxes to their recent past' (p 4). Unlike STS, Latour starts contrasting controversies with moments of cold, unproblematic science, while introducing his notion of the *black box*.[39] He goes on with this distinction: 'if you take two pictures, one of the black boxes and the other of the open controversies, they are utterly different (…). "Science in the making" on the right side, "all made science" or "ready-made science" on the other' (Ibid.). With this contrast, Latour establishes that controversies are the very moment when a fact is potentially being produced. He also indicates that successful fact production ends the controversy, produces a black box, which then turns controversy into a normalised and 'cold' science.

Black box is a notion drawn from cybernetics. It is used in its original context whenever 'a piece of machinery or a set of commands is too complex. In its place they draw a little box about which they [*scientists]* need to know nothing' (1987 pp 2-3). Latour's use of the cybernetics black box in scientific activity depicts the closure of uncertainty. In this sense black box is simply the replacement in time of a complex process and open controversy with a sense of unproblematic certainty and simplicity in science. Latour goes as far as to ascertain that such temporal reversal, from controversial uncertainty to unproblematic certainty in science, 'is one of the most puzzling phenomena we encounter when following [scientist's] trails' (p 98).[40]

In order to substantiate his notions of controversy and the black box, Latour draws on the example of DNA's status at two moments in time: one from 1985 in Paris, another from 1951 in Cambridge (p 2). In 1985, John Whittaker displays pictures of DNA as part of his research project on a data bank for molecular biologists at the Institute Pasteur. The double helix shape of the DNA displayed on his computer screen is taken as unproblematic, 'a basic dogma' (Ibid.), with no risk involved since to believe in the three-dimensional shape of the double helix is, at that point, a routine choice (p 3). In other words, the shape of the DNA is seen in this context as a cold fact. With this status, Latour indicates that the shape of DNA has been black boxed, that a scientific fact had already been produced. This was not yet the case in 1951. There was no 'basic dogma' then, when 'Watson and Crick are struggling to define a shape for DNA' (p 2). At that moment, the shape of DNA was far from certain, surrounded by controversy, and any proposed explanation was tentative or had already been dismissed. Within such uncertainty, there was controversy. There was competition and dispute surrounding DNA, with Rosalind Franklin claiming that DNA might be a three-stranded helix, with

[39] See glossary.
[40] This effect of temporality seems to operate similarly to après-coup in psychoanalysis. (Perelberg 2006)

Lawrence Bragg ordering Watson and Crick to give up their work, and with Linus Pauling presenting a spiral protein structure triple model (pp 3-5). At that moment, all claims regarding DNA were examined and foregrounded, debated and questioned. Latour connects these two situations, thirty years distant from one another, to draw attention to the importance of temporality in science, wherein the same subject can be uncertain and contested at one stage, and cease to be problematic, regarded as a fact, at another. 1985 and 1951 relate to the same issue, from these two very different perspectives, where the 'facticity' of the DNA is concerned. Latour brings this temporal phenomenon, and the black box as producing scientific facts to the heart of ANT.

From an ANT vantage point, the very criterion of internal demarcation for scientific controversies depends on the closure of black boxes, since such an event marks the change of complex processes in science into simple and undisputed facts. Controversies are a special situation in scientific activity because once a black box is closed, it is very hard to open it again and painstakingly follow the process of construction (p 69). Ultimately in ANT, then, a fact is a collective consensus, involving human and non-human actors. From this perspective, scientists are deemed fact-builders, competing in controversies to 'turn one another's claims into subjective opinion' (1987 p 83) while struggling to make their own claims an objective fact[41] (p 30).

Zammito (2004) provides an important account of what drives ANT to differentiate itself from STS. Drawing on different authors such as Lynch, Law, Latour and Callon, he views STS as undergoing a crisis in the 1980s, brought about by the very same project that defined it. This discipline was initially interested in adding social elements to questioning the internalist account that leaves science to scientists by adding social elements as fundamental elements to explain scientific activity (p 145). However, since scientific processes were examined and questioned, what also came under scrutiny was 'the idea that there is a backcloth of relatively stable social interests which direct knowledge or ideology' (Law 1986 p 11). In other words, if sciences operate under uncertainty, the stability of 'social explanation' and social factors should also be questioned and be similarly regarded as negotiated (Latour 1993). 'Since society is no more obvious or less controversial than nature, sociological explanation can find no solid foundation' (Callon 1986 p 3). Zammito quotes Lynch here:

> Sociology's general concepts and methodological strategies are simply overwhelmed by the heterogeneity and technical density of the languages, equipment, and skills through which mathematicians, scientists, and practitioners in many other fields of activity make their affairs

[41] In ANT, to be 'objective' is simply the very opposite of being subjective. This is an achievement. See glossary.

> accountable. It is not that their practices are asocial, but that they are more thoroughly and locally social than sociology is prepared to handle' (Lynch 1992, *In:* Zammito (2004) p 166).

In response to this impasse, ANT gives up on the idea of social context or structure to explain the workings of science, as much as it abandons any idea of Nature granting scientific knowledge. While STS and ANT both argue for a collective consensus underpinning the production of facts, STS relies on more traditional social explanations – Collins' group consensus, Bloor's idea of scientists' belief as social organisations – while ANT offers an alternative solution. This is based on the relationality of networks, comprising actors such as machines, instruments, texts, desks, scientists, and so on. I appropriate this vantage point in this thesis to account for the Controversial Discussions. Instead of focusing only on participants (though never radically excluding their presence as I think ANT tends to do to make its point), their group dynamics, institutional context, their psychology and different perspectives, I am interested in trying to understand the dynamics of internal demarcation at the Controversies according to the articulation of actors and their associations, that contribute to the stabilization of facts in psychoanalysis. From an ANT perspective, to reduce the Controversial Discussions to an issue of 'social context' disregards all the processes (technique, singular cases, inscription of sessions, use of biology, and changes of scale from the individual to the general) that affect its very dynamics.

5. Constructivism of ANT

With his studies on science, Latour ultimately argues that facts are a collective construction (1987 p 41), according to a sociology of associations (and in contrast with the more 'traditional' sociology of the social (Latour 2005)). Similarly to others in STS, Latour is concerned with science in the making (p 4), focusing, like Bloor's 'Strong Programme' and Collins' 'experimenter's regress', on controversies at the research frontier of science. Yet, in emphasising the temporality of scientific activity, the network of actors, and the process of translations and associations, Latour endeavours to provide an answer to the crisis in STS as depicted above. Rather than a focus on scientists as a social group playing a distinct part in the closure of scientific indeterminacy (e.g. Bloor 1976; Collins 1985), Latour's emphasis on the black box follows the messy processes that lead from uncertainty to the production of stable certainty in sciences.

Latour does not speak of experiments on one hand, and social cognition on the other. Neither does he speak of science and social. In Schmidgen's intellectual biography of Latour's work (2014), he notes that the writing desk is at the centre of Latour's attention on the work of scientists. It is where ‘different types of literature – published journal articles, computer printouts with columns of figures, diagrams, tables, manuscripts, and so on – are collected before being transformed into scientific publications’ (p 45). So the production of literary inscriptions is a crucial moment in the scientific work of the laboratory. For example, Latour notices that laboratory observations on endorphin and naxalon injections allow the scientist to produce a visual graph (1987 p 65), which relates to a text. At the same time, ‘it has been *extracted* from the instruments in this room, *cleaned*, *redrawn*, and *displayed*’ (Ibid.), such that ‘the images were the last layer in the text, are the *end result* of a long process’ (Ibid.). In other words, inscriptions are a crucial aspect of knowledge production because they are the moment of transformation, where a series of observations is ‘cleaned’ and ‘redrawn’ – translated, in ANT terms – into traces, spots, points, histograms, which help establish the very properties of objects (Schmidgen 2014 pp 45-9).

Inscriptions are one major example of how Latour explores ‘processes of translation and displacement that can be observed beyond and beneath purely social interactions and beyond and beneath spoken as well as written languages’ (Schmidgen 2014 p 4). Therefore, inscriptions are one type of associations, located in a hybrid space, form an interface between ‘a paper world (…) and one of instruments’ (1987 p 65). At the same time, the notion of inscription suggests that there is a long process between setting up a space of observation, arranging or devising instruments, making observations, agreeing on what needs to be observed, extracting regularities from what is observed, redrawing them through images, and displaying this information in texts (e.g. Latour and Woolgar 1986). These are all aspects of the process of producing facts and knowledge in scientific activity.

Once inscriptions are obtained, they become part of texts, articles, and journals. At the same time, Latour defines the production of facts in sciences as the attempt to get rid of ‘any trace of ownership, construction, time and place’ (1987 p 23). In other words, the process of construction involves black boxing complexity so that what remains is an indisputable, simple, timeless assertion ‘because no more has to be said about it’ (Ibid.). A fact can simply be taken for granted and used as a departure point. Yet, ‘*by itself a given sentence is neither a fact nor a fiction; it is made so by others, later on*’ (Latour 1987 p 25). With inscriptions, it is possible to capture the initial moments where visual displays, graphs, or tables define their objects. Within this process of associations and transformations, sentences are put forward, published and circulated through journals where they become liable to be disputed, such that the very processes that led to these inscriptions, the traces of ownership, construction, time and place, are brought under examination (pp 23-4). So, ‘the fate of the statement, that is the decision about whether it is a fact or a fiction, depends on a sequence of debates later' (p 27). In this quote, Latour alludes once more to the temporal character of facts, collectively constructed through a multi-stage process, both in and beyond the laboratory (p 29).

Latour draws on several examples from the sciences to illustrate his constructivist perspective on controversy as the moment where dispute takes place, complexity is out in the open, and different actors become mobilised in order to produce a black box. In addition to the DNA example, he offers the example of Schally's assertion of the primary structure of the growth hormone-releasing hormone (GHRH). Guillemin questioned this on physiological grounds (pp 22-7), proposing an alternative growth hormone-releasing factor (GRF) and a controversy ensued, with different authors and papers joining the technical endocrinology debate; Schally's inscriptions are minutely scrutinised (pp 33-35), counter-experiments are offered, until Schally's claims are eventually dismissed as fabrication in the literature, while Guillemin's GRF gains support via several resources (experiments, articles, followers) and is ultimately black boxed and regarded as fact (pp 52-65). Additionally, Latour argues that the network of associations and actors involved in controversy and fact production extends beyond the community of scientists and their laboratories. In *The Pasteurization of France* (1988), for instance, he shows how Pasteur's assertions of microorganisms as the agents of anthrax had to gain traction and mobilise several actors within France before it could win out against Pouchet's spontaneous generation theory (1988 Chapter 4). These actors included scientific journals (*Revue Scientifique, Concours Medical*), hygienists, laboratories, farms, cows, dogs, vaccines, the Agricultural Society, and medical doctors. It is only after this long process of articulation and mobilisation through associations, involving a change of scale from one laboratory site to farms, that the process behind Pasteur's work could be black boxed, his activities in the laboratory sanctioned, and the properties of microorganisms established as fact. So, if the successful production of a black box can be considered as the very criterion of internal demarcation in sciences, much of it depends on the strong associations taking place throughout the process of knowledge production. In this sense, Latour's constructivism is about temporality (black box as an event) as much as it is about effort (construction of facts as process).[42]

One final aspect I would like to highlight is that ANT's constructivism entails the notion that both the social and the natural are constructed (Latour 1993). So ANT's constructivism is relevant here because the question of what is natural and social in ANT can be transposed into a tension more specific to psychoanalysis – that is, into how the question of what is contingent, subjective and

[42] ANT does not provide a causal explanation *stricto sensu*. It never establishes how exactly a fact comes about. ANT instead studies the construction of elements (stronger associations) that surrounds and leads to a situation where the mysterious temporal inversion, the black box, takes place as an event. See (Latour 1987).

personal ('social') versus what is taken as certain and general ('natural') is played out in psychoanalysis.[43]

6. Theoretical Context

Fourteen years after the publication of his famous ethnographic study at the Salk Laboratory (Latour & Woolgar 1986; Hacking 1999a), Latour published *We Have Never Been Modern* (1993). According to Harman (2009), his point of departure is that moderns 'purify the world by dissecting it into two utterly opposed realms. On one side we have the human sphere, comprised of transparent freedom and ruled by arbitrary and incommensurable perspectives. On the other side we have nature or the external world, made up of hard matters of fact and acting with objective, mechanical precision' (Harman 2009 p 57). In some ways, Latour's views on modern thought are no novelty. They sit within Rouse's melting pot of cultural studies of scientific knowledge. In addition, Descola (2013) identifies other critical minds similarly objecting to the human-external world dichotomy throughout history.[44] He points, for example, to 16th century philosopher Michel de Montaigne as one 'among those who have criticized the attribution of an absolute singularity to humans thanks to their inner faculties' (p 174). Yet Latour's thesis gains in distinctiveness when he proposes that the world is not just divided into two opposed zones, but rather that dichotomy is the end result – the outcome produced by a western cosmology separating Nature from Culture. To this cosmology, Latour posits that there is always a concomitant ongoing process, which he explains according to a set of practices comprising the works of *purification* and *translation* (1993 p 11). In his view, purification establishes 'a partition between a natural world that has always been there, [and] a society with predictable and stable interests and stakes' (p 12). Latour then really challenges the modern dichotomy, describing the natural world and considering translation and purification together.

Descola (2013) notes that:

> Ever since the mechanistic revolution of the seventeenth century, scientific and technical activity has never ceased to create mixtures of nature and culture in networks of increasingly complex structures in which objects and humans, and material effects and social conventions, coexist in a situation of mutual 'translation' (p 86).

[43] This question was similarly presented as a challenge by Forrester (1996) as psychoanalysis would aim to be both science and personal investigation.
[44] On the role of Descola's anthropological work in Latour's modern thinking, see (Blok and Jensen 2011, p 154).

So Latour's *translation* involves heterogeneous elements and the creation of mixtures of nature and culture in networks that, after a process of negotiation, eventually gain ontological strength through the work of *purification*. The laboratory in itself, for example, is a hybrid arena containing a mix of scientists, instruments, inscriptions, texts, diagrams, objects, colleagues, other scientific fields, computers, bioassays, and so on (Latour and Woolgar 1986). These elements are all part of complex processes, an assemblage without clear boundaries in terms of human intervention, the influence of techniques, tools, and the circulation of data (Callon 1980, 1986, 1990). And while Latour claims that mixed activities of heterogeneous elements remain ever operative, he also notes that mutual *translation* is somehow kept invisible and brushed aside by modern eyes (1993 p 11). The original and fertile features of Latour's thinking lie precisely in making visible the concomitant work of *purification* and *translation*:[45]

> So long as we consider these two practices of translation and purification separately, we are truly modern (…). As soon as we direct our attention simultaneously to the work of purification and the work of hybridization, we immediately stop being wholly modern, and our future begins to change (Latour 1993 p 11).

At the same time, Latour defines the co-existing works of translation and purification as follows:

> The word 'modern' designates two sets of entirely different practices (…). The first set of practices, by 'translation', creates mixtures between entirely new types of beings, hybrids of nature and culture. The second, by 'purification' creates two entirely distinct ontological zones: that of human beings on the one hand; that of nonhumans on the other. Without the first set, the practices of purification would be fruitless or pointless. Without the second the work of translation would be slowed down, limited, or even ruled out. The first set corresponds to what I have called networks; the second to what I shall call the modern critical stance (Latour 1993 pp 10-11).

Latour's more general task, then, is to render visible the fact that moderns are simultaneously creating ever more hybrids through the work of translation, while attempting at the same time to purify the world according to their dichotomous cosmology. Therefore, a focus on processes and practices leads ANT to understand that 'moderns neither do what they say nor say what they do' (Descola 2013 p 86), because they try to fit the world into an invented dualistic cosmology, while actually creating mixtures of nature and culture in practice, which eventually become purified into ontological zones. This is precisely Latour's conclusion, stemming directly from his account of

[45] See glossary.

scientific activity, controversies, and his notion of the black box. Expanding on the omnipresence of translation, Latour argues that the more artificial and sophisticatedly constructed the network of actors (of scientists, instruments, ideas, texts, laboratory, etc.), the more likely it is that modern scientists will strengthen and articulate their ontological claims to natural facts (Latour 1993). From a modern perspective, this presents a paradox: the more translations and articulations among actors, the more moderns will believe in a fact's independence, as given by a Nature of primary qualities (Latour 2004b; 2010). Put another way, it is '*because* [a fact] is constructed that it is so very real, so autonomous' (Latour 1999b p 275), such that 'construction' and 'autonomous reality' become synonymous (Ibid.) within a process of co-production of facts and fabrication.

7. Critical dialogue

Latour's 'approach is notoriously difficult to capture in a few simple characteristics' (Blok and Jensen 2012 p vi). He is a controversial figure, hovering 'on the edges of critical theory in the humanities, but has never quite been subsumed into that generic French "theory" that Anglo-American academies tend to construct' (Luckhurst 2006 p 4). Latour indeed does not 'easily fit into traditional disciplinary categories' (Crawford 1993 p 1), even when there is enough time and room for debate.

Since his early works, Latour has been engaged in dialogues and critiques of his own work in different forms. Latour engages in debates within subject areas that include the humanities, field and laboratory sciences, sociology of science, and sociology. This section organises these various threads into two central and sensitive focal points of Latour's formulations: the primal character of relations and the hybrid reality of symmetrical actors. My aim is to take stock of the limitations of my investigation, based on ANT, while also highlighting that my analysis is supported by a cogent and robust relational theory

The primal character of relations places Latour's thinking within a philosophical tradition known as occasionalism (Harman 2009 p 112). Latour's insistence on networks and relations clashed with a longstanding tradition of realists, going as far back as Aristotle, for whom there was always something more, subsistent, beyond relations:

> The repeated shouts against Latour that reality exists 'whether we like it or not' can immediately be dismissed when they are meant to endorse the human-world split that his democracy of actors destroys. But there is a more defensible side to 'reality whether we like it or not' – namely, the reality of a thing apart from its relations whether we like it or not. Since Latour is an absolute relationist in his theory of actors, he cannot look

> fondly upon this point. But the grain of truth in the physicist's scream is that an actor must already exist if other actors are to negotiate with it in the first place (Harman 2009 p 111).

In the quote above, Harman points to the strengths of ANT in relation to realists, especially with regard to the human-world split. At the same time, he acknowledges the reality of actors, which cannot be fully encompassed by relations. In this sense, Latour's model 'pays the following price: while the gaps between entities are rightly multiplied to infinity, he leaves no gap at all between a thing's inherent reality and its effects on other things' (Harman 2009 p 112). Harman regards this price as far smaller than do traditional realists (Ibid.), but points out that different arguments for realism should be appreciated on different terms. Bloor (1999), for example, denies the value of ANT in the society-nature debate, rejecting the idea of a co-production of nature and culture. Bloor arguably keeps realism intact when he writes that his aim 'isn't to explain nature, but to explain shared beliefs about nature' (p 87), which situates him in the 'reality exists whether we like it or not' camp, leading to an impasse and Bloor's wholesale rejection of ANT. In contrast, Hacking (1999b) and Shaviro (2004) take a more nuanced perspective that stresses discomfort with Latour's ontological flattening between subjects and objects. At the far end of the realist spectrum, Sokal and Bricmont charge Latour with the fundamental sin of misconceiving Einstein's theory of relativity (Guillory 2002); it does not matter to Sokal and Bricmont who or where Latour is, as long as he is kept away from 'real' science. Interestingly enough, Schinkel (2007 p 720) suggests that Bourdieu agreed with, and assumed a similar stance to, Sokal and Bricmont where realism is concerned, and as it affects discussions on sociology; he suggests that Bourdieu's critique of Latour was based on reinforcing a subject-object divide which ultimately leaves scientific and objective matters unavailable to sociological debate. It is beyond the scope of this thesis to delve into the realm of human and non-human agency that underpins the debate above (see for example Barad 2003, 2007) on the subject-object divide. However, the point to be taken here is that ANT offers tools to consider psychoanalysis as it transitions from subjective accounts of patient-analyst interactions to claims to objective knowledge. I am interested precisely in this negotiation that breaches the division between subject and object.

The other sensitive point in Latour's formulations is the hybrid nature of objects. For Harman (2009), Latour's work on hybridity is one of his greatest achievements since it 'denies' the modern oscillation between human and world as the wellspring of all enlightenment' (p 112). At the same time, something might be lost without these modern oscillations. Subjects and objects are all taken by ANT as actors, and evaluated according to symmetrical views that ignore a priori differentiations or given boundaries. So much so that Latour replaces subject-object terminology with human and nonhuman actors (1999b), highlighting his wish to deemphasise oscillating differences and focus on combinations and hybridity within an egalitarian stance. Evaluating this blurring of boundaries, Pels (1996) shows discomfort with the idea of symmetry: 'Michel Callon and Bruno Latour, in the boldest move in S&ST so far, have pleaded the symmetrical inclusion of nonhuman actors or "quasi-objects" into a general anthropology of hybrid networks or "culture-natures"' (p 278). Pels' major point is that symmetry should not simply be associated with an idea of impartiality, as if symmetry were outside politics. He questions the notion of neutrality, as associated with symmetry and with Latour's democracy of quasi-objects, while warning that there is no such disinterested position. Similarly, Elam (1999) questioned Latour as the privileged observer, 'occupying a "breathing space" independent of the networks of power-knowledge he charts' (p 2). Elam notes that symmetry 'is not only a way of establishing equality but also a means of setting the scale of difference at zero' (p 13). In this elimination of difference, sexual difference, for example, was silenced in Latour's text. Like Elam, Fuller (2000) relates ANT to its 'social circumstances', correlating Latour's thought with 'specific features of French philosophical and political culture' (p 5). Lastly, Lee and Brown (1994) problematise Latour's totalising impulse, placing ANT 'on the throne as narrator-in-chief and the final arbiter of fairness' (p 783). In this respect, Lee and Brown draw attention to the impossibility of, or great difficulty in, thinking in terms of the sociological 'other'.

Latour's hybrid world of symmetric actors drew more critiques in a similar vein. For example, Strathern (1996) argues that the power of ANT's analytical networks is also their problem. She agrees that 'the rhetorical power of the hybrid rests on its critique of pure form, of which the archetype is the critique of the separation of technology from society, culture from nature, and human from nonhuman' (p 521). But, while understanding the concept of hybridity as supplemented by the notion of a network, she argues that networks have limits, lengths, and can, therefore, be subject to 'cuttings' (p 522). She then employs an anthropological account and the idea of ownership to illustrate a network being interrupted. For Strathern, the problem of such analytical networks is that 'theoretically they are without limit (…). Yet analysis, like interpretation, must have a point; it must be enacted as a stopping place' (p 523). Star (1991) adds further comments on networks, noticing that Latour's analyses rely on heterogeneities, but that their focus leaves aside other limiting aspects of networks, for example the observer evading the question of differences – as in gender, split 'selves', and complex identities (pp 52-53) – as engendering a complex interaction within networks. Bearing these issues in mind, then, 'we enter a high tension zone which may illuminate the properties of the more conventionalized, standardized aspects of those networks which are stabilized for many' (p 53) but not for minorities that offer different accounts of networks.

I think the points above illustrate the trade-off between the use of ANT and other frameworks. ANT's riposte is that the crucial starting point for Latour's intellectual project is to think outside of modernism: 'is there an alternative to modernism? If so, how can one find it? In 1973 in Abidjan, that was not a useless question. In 2001, in Paris, it is not useless either' (Latour 2001 p 138 – my translation). Latour mobilised his relationality and symmetric views in relation to this question. In other words, Latour does not dismiss the issues of differences, subjectivities, and identities, but deemphasises them (Latour 1996), considering it a reasonable price to pay for an alternative framework to modern thinking, with special attention to objectivity. By his own account, this imbalance begins to be corrected in time with what he calls a second wave of ANT, wherein more recent studies explore the issue of differences from within the network (Latour 1999a p 23). Yet, I do not think Latour's argument is robust enough here. It would be better to simply acknowledge ANT's limitations, and that the adoption of ANT implies a choice. In my case, I leave aside the theoretical frameworks and notions that form part of this modern divide. For instance, I am not relying on social structure (e.g. the educational-political question of training, the institutional context of the Controversies), or questions of identity or discourse to underpin my investigation. These are certainly useful tools, and have already been deployed in relation to the Controversies, as shown in Chapter 2. Still, in the case of the Controversial Discussions, I also think that ANT limitations are compensated for, in the main, by the original entry points it offers to consider the dynamics of internal demarcation while highlighting the work of translations – often left aside by these modern categories. In a sense, the best example I can think of is the handling of the term 'science' at the Controversies. Are we to leave such a central term in that context as a mere matter of participants' opinion? A false belief? Is it possible to take up this term more consistently as influencing that debate? ANT's constructionism avoids the reduction of the debate around 'science' in psychoanalysis as merely a 'social' phenomenon, while allowing us to explore how the 'natural' is also produced in it. This is the fundamental reason for my choice of ANT as favourable to other approaches, including STS.

Latour is aware that relationality 'is the most counter-intuitive aspect of ANT. Literally there is nothing but networks, there is nothing in between them' (1997). Yet, his use of networks as a fundamental relational stance helps ANT to 'dissolve the micro-macro distinction that has plagued social theory from its inception' (Ibid.), while blurring the notions of inside-outside, for example. So, while there are valid critiques of ANT's 'breathing space', independent from power relations, ruling out a sociological 'other', they are arguably couched in anthropocentric and sociocentric perspectives, which are precisely the partitioned, modern stances that Latour seeks to avoid. Latour's main goal is to dissolve pre-given boundaries and allow much more porosity between realms: ‘a network notion is ideally suited to follow the change of scales since it does not require the analyst to partition her world with any a priori scale’ (Ibid.). In so doing, Latour radically flattens any ontological distinction between humans and nonhumans, making a mockery – as he does – of the critique of ANT ‘as if it talked of a few superhumans longing for power and stopping at nothing to achieve their ruthless goals’ (1996 p 7). Certainly, ANT does not disqualify anthropocentrism and sociocentrism. It acknowledges the strengths and fertile ground provided by modern thinking (Descola 2013; Latour 1993; 1999b; 2004b), but sees these disciplines as taking very different theoretical perspectives, leading to an impasse. In other words, many critiques might fail to appreciate that ANT’s departure point for analysis is to stop giving humans an ontological priority, and to stop thinking about the social with the material world taken for granted. There seems to be a short circuit or a circularity of arguments whereby critics argue: ‘you fail to account for power, social relations and the “other”’, with ANT responding ‘but I am precisely trying to avoid these sort of limiting analyses’. Facing such a situation, it is important to remember that Latour seeks to foster what he calls a sociology of associations, tracing a historical lineage descending from Tarde, and contrasting with classical sociology (Schinkel 2007) or a sociology of ‘the social’ as associated with Durkheim (Latour 2005).

However, Latour is aware that ‘if a criticism can be levelled at ANT it is (…) its complete indifference for providing a model of human competence’ (Latour 1996 p 7); he is aware that ANT is not a humanist theory:

> There is no model of (human) actor in ANT nor any basic list of competences that have to be set at the beginning because the human, the self, and the social actor of traditional social theory is not on its agenda.
>
> So what is on its agenda? The attribution of human, unhuman, nonhuman, inhuman characteristics; the distribution of properties among these entities; the connections established between them; the circulation entailed by these attributions, distributions and connections (Latour 1996 p 7).

The lack of emphasis on humans is one price to be paid by Latour’s quest to shake up spheres and domains, and to regain a sense of heterogeneity. The flipside to this loss is the gain in terms of new agendas regarding material agency, science, and ecology (Bennett 2010; Barad 2003, 2007). Latour acknowledges that ANT ‘is an extremely bad tool for differentiating associations. It gives a black and white picture not a coloured and contrasted one’ (1997). So, the very trajectories that are obtained in ANT need to be specified and criticised in terms of what is lost – or can be further added – in the process of network-tracing activity. It is here that Strathern's and Star's observations gain salience, in that power relations and differences bring distortions and hidden limitations to networking, and therefore need to be factored in and made explicit. It may seem strange at first to deemphasise the 'human' side in psychoanalysis since it is suffused with it. Yet, one of my contending points with the Controversial Discussions is that there is more to it than just human interaction. There are uses of biology, attempts to produce facts, changes of scale and attempts to generalise and define knowledge that go beyond a simple matter of opinion and involve a great number of observations and articulations. There is a price to be paid, as these critiques indicate. But I also consider the gains of using this approach, wagering that ANT will help to illuminate the Controversies in an original manner.

I think the most important point raised here is the incompatibility between ANT and several critiques coming from the humanities. Behind this issue, there is a question of choice between two different philosophical positions. Latour acknowledges that ANT is 'very good at giving freedom of movement but very bad at defining differences' (Tresch 2013 p 304), which provides a good summary of the critiques raised in relation to ANT. Yet, if gender, sexual difference, power relations, the 'other' and other central notions for critical enquiry are omitted, I agree with Latour's point that these issues have so far broadly avoided the very question of how facts are produced in sciences.[46] And I regard the issue of fact production as central since it is closely connected with internal demarcation at the Controversial Discussions. It is certainly possible to study the Controversies from different angles, which has been done before as indicated in the previous chapter. Yet, studies relying on approaches based on gender, sexual difference, power relations and others have arguably not opened many vistas to properly address the central presence of 'science' at the Controversial Discussions. This is one major reason why I have chosen ANT as my framework, and I think it reflects the strengths of ANT.

8. Conclusion

This chapter presents the theoretical framework for this study. It firstly provides a context for STS and some of its main developments concerning scientific controversies. It secondly locates ANT as a response to a sense of crisis and limitations in STS. ANT departs from STS with the development of a theory that accounts for the production of facts via all sorts of processes, involving heterogeneous actors that all play an integral part in fact production. These are investigated according to a

[46] Broadly but certainly not totally. See for example Barad (2003, 2007).

sociology of associations. With this move, explanations resorting to Nature or the Social are excluded. What is left is a relationality, where actors define themselves through their associations.

A fact is regarded in ANT as an event. There is no causal necessity behind it, only the possibility of the event as a result of strengthened associations. So ANT does not speak in the name of Science but only in terms of sciences, in the plural, to indicate the specificities and contingencies associated with situated sites of knowledge production. The production of a fact from an ANT vantage point remains a mysterious event that undergoes an inversion. In its mystery, it is not explained but followed according to supporting networks of associations. Latour approaches the inversion from complementary angles: as a black box, as the shutting down of any questioning, according to the strengthening of a network of associations behind a claim, and as an objective knowledge that eventually ceases to be associated with the subjectivity or opinion of a person. These angles flesh out the process, via the efforts working behind fact production. In this sense, a fact is the successful consolidation of these efforts, which ANT calls associations.

So, while ANT emphasises both the uncertainty and the struggle behind scientific dispute, it also provides a specific model to understand the settling of a scientific controversy because, once and if a fact is produced, such an event ends controversy and becomes the very criterion of internal demarcation for a scientific controversy (Latour 1987 p.97). Unlike the terms provided mainly by philosophy of science, I regard ANT as a suitable framework to address the issue of internal demarcation in psychoanalysis. At the same time, it has its own limitations, and choosing ANT involves a trade-off. I find it a reasonable one since it will allow me to address the term ‘science’ as a central issue at the Controversial Discussions, without reducing the role played by ‘science’ to a question of discourse, power relations or social context. I think there is fundamentally more to the notion of fact in psychoanalysis than what other frameworks in the humanities have so far been able to provide. And it is according to this perspective, considering the processes behind claims, that I am progressing my analysis here.

Chapter 4: A Method for Following Actors

If earnest scholars do not find it dignifying to compare an introduction on science to a travel guide, be they kindly reminded that 'where to travel' and 'what is worth seeing there' is nothing but a way of saying in plain English what is usually said under the pompous Greek name of 'method' or, even worse, 'methodology'. (Latour 2005 p 17)

Having arrived at a more complex understanding of the research question, with a discussion centred on scientific controversy, fact, and internal demarcation, the aim of this chapter is to specify how I will deploy the tools provided by my theoretical framework. I structure my discussion on methodology according to ANT's injunction of 'following the actors'. Thus I break down this general methodological principle into four parts to help me to outline my approach.

1. Focusing on the scene

Latour is highly indebted to insights from ethnomethodology. By following the actor (Latour 2005), we find the researcher assuming a specific methodological attitude towards investigation, which I think deeply connects with Garfinkel's influence on ANT.[47] Ethnomethodology can be defined as a study of social agents' own methods to produce social order (Rawls 2002). At the same time, 'Garfinkel's focus on patterned orderliness places emphasis on the scene and away from the population. From this perspective, the variables are in the scene and not in the population' (p 129). So, for Garfinkel, 'formal institutions, and collective concepts and beliefs, have little to do with the production of order and intelligibility' (p 130). Furthermore, 'because these orders are collective enactments, a focus on individual subjectivity would obscure them' (p 146). What is central to ethnomethodology, then, is the enactment of observable practices in actors' interactions, witnessed in specific scenes, and according to descriptive methods (p 149).[48] This is also a core methodological principle of ANT.

The option for a scene also introduces the idea of a choice or methodological cut. Considering the ANT literature, Mol (2002) for example studies the process of multiple enactments of leg

[47] For Garfinkel's influence on Latour's thinking, see for example Blok and Jensen (2012 pp 161-2) and Latour (2005 p 54).

[48] I see great relevance in the fact that Garfinkel provides a specific way 'to travel', to guide one through social activity with an approach that rejects or at least deemphasises traditional tenets of 'classic' sociological methodology (the reliance on formal institutions, collective concepts, beliefs) as much as individual subjectivity. Latour follows the same line, and very clearly so, for example, in Latour (2005).

atherosclerosis. She establishes as a scene the very boundaries of a Dutch hospital. In this sense, the actor-network arena is 'cut' by the very physical limits of the hospital. Consequently, Mol's choice for what happens inside a hospital provides a sort of structure to her investigation. The same can be said about *Laboratory Life* (1986). There, without making it explicit, Latour and Woolgar choose as their scene the boundaries provided by a scientific laboratory. However, the 'cut' in the network need not necessarily obey physical delimitations: with *The Pasteurization of France* (1988) the scene for inquiry emphasises instead negotiations *outside* Pasteur's laboratory (Schmidgen 2014).

In my opinion, it is only in theory that ANT encourages the investigation of an endless network. In practice, as Strathern (1996) notices, tracing actors in a network entails having to face a 'cut', which I have connected in this thesis to the idea of a scene. Of course, the Controversial Discussions is an event in the history of psychoanalysis that is preceded by a series of events and is full of psychological motivations (e.g. Klein's arrival in England, Freud's family's immigration to the UK, a hotly contested debate on child analysis between Anna Freud and Klein, growing concerns over institutional matters and candidates' training). Yet, the 'cut' in the network established here delimits this study to the rooms at the British Psycho-Analytical Society where a series of extraordinary and scientific meetings took place, for the specific purpose of discussing Melanie Klein's propositions, between 1941 and 1945.

Bounded by these specifications, I argue that a scene took place there and use this as the basis for selecting my sources of study. As part of its research design, then, this study is not particularly interested in individual subjectivity, personal memories, and personal letters. Accordingly, I use sources accounting for what was publicly debated in that situation and exclude any additional texts concerning personal views, personal experiences, and correspondences. I have chosen the *Freud-Klein Controversies* then, because it shows the negotiations that took place collectively during meetings at the Controversial Discussions. Along these lines, the 'cut' for the scene provides the basis for choosing my sources of study. As a result, I am using in this investigation the sources accounting for what was publicly debated in that situation. On a par with the idea of studying a scene, I have excluded from analysis here additional texts concerning matters of personal views, personal experiences, and correspondences. As part of its research design, then, this study is not particularly interested in individual subjectivity, personal memories, and personal letters. With the notion of a scene, there is a clear differentiation between what one puts forward and is recorded at a public debate, and for example what one feels and remembers. This is also the reason why I restrict my analysis to the contents of *Freud-Klein Controversies* since what this book does is to mainly compile, binding together the original documents or 'the reports of the actual discussions that took place in the British Psycho-Analytical Society between 1941 and 1945' (King and Steiner 1991 p xxvii).[49]

2. Using the text

Like *The Pasteurization of France*, the Controversial Discussions is a controversy of the past that can only be approached through texts. I now delve into how I use this book to 'follow the actors' and their network of associations. Latour's use of texts does not follow the premise of a discourse analysis, since he believes that language should not be isolated or regarded as an autonomous and independent instance of analysis (1993 62-5). A focus on discourse, he argues, isolates it from referents, context, and the need to establish truth and proofs (ibid). In a world understood as hybrid (1988 p 184) and made up of associations (2005 p 127), texts should always be regarded in connection with other actors. So texts, accounts, reports, and conversations should be regarded as just other types of mediators within the network (pp 122-8) that help to produce facts. Through texts, then, Latour tries to observe 'processes of translation and displacement that can be observed beyond and beneath purely social interactions and beyond and beneath spoken as well as written languages' (Schmidgen 2014 p 4), while being part of and helping reveal a 'complex structure made up of instruments, data, theories, and scholarly publications' (p 50), neither in isolation (Latour 1993) as 'text *qua* text' (Latour 2005 p 122) nor as a matter of language games (1988 p 204).

Take *inscription* for example. For Latour, inscriptions are located in a hybrid space, forming an interface between 'a paper world (…) and one of instruments' (1987 p 65) and only matter in a controversy to the extent to which they carry the weight of associations behind them. Otherwise, without the complex structure behind them, they are void words, incapable of influencing the production of a black box. Therefore, in the absence of a focus on institutional contexts, power disputes,[50] and psychological accounts, I look to *The Freud-Klein Controversies 1941-1945* not in search of a textual generativity. On the contrary, I am using the primary data contained in the book

[49] The *Freud-Klein Controversies* is a compilation of material stemming from the archives from the Institute of Psychoanalysis and Wellcome libraries – I have consulted these libraries but have been unable to locate any additional source material on these meetings other than personal letters, non-related reports and minutes of meetings that are not formally part of the Controversies. So, what distinguishes the *Freud-Klein Controversies* is that the specific scene takes place through them: through the exposition of articles (the four texts, mentioned in Chapter 3) read and used during the discussions; through the verbatim reports of ensuing debates where participants address one another publicly; and through memoranda on psychoanalytic technique also used for collective debate. In other words, I am approaching the *Freud-Klein Controversies* as the resource that exhibits the collective attempt to produce order through the demarcation of Melanie Klein's propositions.

[50] There is of course a power dispute. In ANT, a specific form of power is taken up as enmeshed with the black boxing of facts and creation of reality. See Latour (2004b), and also the conclusion of Chapter 9.

to consider how an array of associations operate behind the text. It is also these associations that give support to, and help spur, claims concerning the demarcation of Klein's notions. What needs to be emphasised is that a statement *qua* text (or discourse) is irrelevant in ANT. From this perspective, if it is through texts that associations in processes of knowledge production are revealed, it is also the case that texts only become relevant in knowledge production according to the array of the associations underpinning them.

My general reading strategy reflects the points above. One of my first tasks was to structure my approach to the text of *The Freud-Klein Controversies 1941-1945*. This is a mammoth book, with almost one thousand pages. In essence, it is a compilation of material (minutes, memoranda, essays, verbatim debates) from the time[51] that was later bound together in chronological order.[52] The order of appearance in the book reflects the order in which events took place at the Controversial Discussions. I first read *The Freud-Klein Controversies 1941-1945* once, from beginning to end, in order to familiarise myself with the text and have a sense of its structure. In this initial reading, I noticed the central role of the term 'science' and related notions at the Controversies. With my theoretical framework, I returned to the text, searching for every reference to the word 'science', its variations (e.g. 'scientific'), and associated terms (fact, empirical, truth, evidence, etc.). While I regard the scene as being delimited[53] by *The Freud-Klein Controversies 1941-1945*, I emphasise the second part of the volume, which starts on page 264. While the first part of the book is mostly concerned with the aims and format of the discussion, the second part sees the beginning of a

[51] Consulting the Institute of Psychoanalysis' archives, I can verify that any original relevant source available and related to the Controversial Discussions is covered by *The Freud-Klein Controversies 1941-1945*. From the same period, I could find no other extensive document regarding other institutional matters that did not relate to the Controversies. Usually, these other documents were very brief minutes of meetings. In general, the documents concerning the Controversial Discussions stand out for their length, details, and numerous accounts. The same careful treatment was not applied to any other subject in the consulted files for the 1941-1945 period.

[52] One of the few exceptions is Strachey's final report on technique, in section three of *The Freud-Klein Controversies 1941-1945.*

[53] There are important differences behind the comparison between scene and the institutional context. With the former, the point of departure is indeterminacy, with actors negotiating to create order locally and among themselves. This is an understanding derived from ethnomethodology and borrowed by Latour, which arguably contrasts with the idea of individuals struggling against institutional constraints in specific contexts (Rawls 2002, p 130). For Garfinkel, 'formal institutions, and collective concepts and beliefs, have little to do with the production of order and intelligibility' (ibid). Institutional constraints do play a part but are regarded as secondary to the order produced at a specific scene. Latour goes even further in his views on the primacy of local negotiations. For him, an ANT 'main tenet is that actors themselves make everything, including their own frames, their own theories, their own contexts, their own metaphysics, even their own ontologies' (Latour 2005, p 147). In other words, Latour inverts the order of social accounts: instead of contexts informing individuals, it would instead be the actors eventually making their own context through associations. In ANT, social and institutional contexts, facts and reality, would all be the output of negotiations within a network of actors establishing associations, and not the other way around. In fact, this 'inverted' order runs through Latour's work (1993, 1999b, 2004b, 2005). Therefore, it would not make sense in ANT to ask for institutional context as an explanatory variable to the Controversial Discussions, since its role would be vague, distant, and secondary to the emphasis on observing how actors enact order among themselves.

focused debate on Klein's notions, marked by Isaacs' opening presentation on unconscious phantasy. I refer to the first part of the Controversies, but focus my analysis on associations based on the second part since this is the arena of dispute proper, with the bulk of associations and strategies related to knowledge production. In summary, the first part is about preparation, and the second about negotiation.

Having divided the book into two main parts, and having located the term 'science' and related notions in their specific contexts in the text, I returned once more to *The Freud-Klein Controversies 1941-1945*. Given the ANT notion that facts emerge out of a struggle, comprising different aspects in the chain of knowledge production, my first task was to identify what parts of this chain assumed prominence at the Controversies. I was aware that potential criteria of internal demarcation could relate to any particular point within the chain of knowledge production in psychoanalysis, and to any array of associations within the chain, such as clinical cases (Forrester 1996), technique (Stengers 1997), or biology (Sulloway 1979). They could also relate to what were simply regarded as facts, evidence, data, and observation at the Controversies. These considerations gave me a specific kind of reading attention.

My re-readings of *The Freud-Klein Controversies 1941-1945* have an iterative aspect. Informed by the above, for example, I returned to the text, reading all passages containing the term 'science' and related notions. But I also began searching for terms related to clinical cases, to technique (e.g. transference), or to biology (e.g. Mother Nature, laws of nature, phylogenesis). I mapped out these parts in the chain of knowledge production in psychoanalysis, while also beginning to investigate how and what associations, actors, were being mobilized to underpin claims to factual knowledge at the Controversial Discussions. With the mobilization of the terms above, I discuss through my following of the negotiations at public debate and identify four potential criteria for internal demarcation: 'claiming the truth', 'clinical evidence', 'technique', and 'biology'. In Chapter 5, I conclude that only the last criterion becomes the eventual criterion for internal demarcation at the Controversial Discussions.

On a final note, Latour's prototypical textual source is the academic article. While *Laboratory Life* and *The Pasteurization of France* differ in their scope of analysis, they have in common the centrality of the academic text. With the former, Schmidgen (2014) notices that Latour and Woolgar focus their attention on 'the writing desks upon which very different types of literature (…) are collected before being transformed into scientific publications' (p 45). With the latter, it is stated from the beginning that it relies on three academic periodicals as the basic data upon which a study following actors and tracing associations takes place. With *Science in Action,* Latour once more emphasises the importance of texts and academic publication in scientific controversy. In adapting ANT's approach to texts for this thesis, I have to consider that the Controversies were shaped by a mix of long verbal interventions and presentations instead of the typical format of scientific articles, as in Latour's accounts. Although he makes some use of dialogues as primary data*,* reporting conversations between scientists for example (Latour and Woolgar 1986), Latour does not regard them as a personal matter, a question of personal agency or opinion. He is interested in the collective process of negotiations among heterogeneous actors,[54] and so uses conversations to observe these processes. While academic articles and dialogues have uneven treatments in Latour's work, there is no theoretical principle differentiating them – they all take part in the agonistic battle to produce facts. Dialogues at the Controversies are therefore treated similarly, although one needs to be more careful to keep in mind the distinction between a dialogue revealing processes of translation and displacements, and a dialogue as a personal matter.

3. Understanding the grounds for dispute

With *Science in Action,* ANT has established that the construction of facts takes place in controversies according to an agonistic logic (Latour 1987 p 237). An agonistic dimension privileges a scene of combativeness, where scientists fight all the time, since 'at any rate, day in, day out, [scientific] talk revolved around strategy, occupation of positions, infiltration of ideas, destruction of reputations, defeating opponents, and even of guerrilla warfare' (Schmidgen 2014 p 36). From this perspective, scientific controversy is arguably a collective enactment of winning (or losing) credibility around the construction of facts, in scientific contentions where many strategies can be invoked (Zammito 2004 pp 152-3). At the same time, and as already mentioned in the introductory chapter, ANT regards controversies as erupting 'hot spots', with their clashes compared to 'punching' the system (Pinch 2015). So scientific controversies can be regarded in ANT as a specific kind of scene: they are characterised by friction and collision between contending sides, and by the explicit manoeuvres supporting the facticity of claims that are overtly made during debate.

If the association between psychoanalysis and biology is the central criterion for internal demarcation at the Controversial Discussions, Chapter 5 also identifies what I regard as the main actors in the agonistic dispute. On the one hand, there is the notion of the Oedipus complex, taken as the exemplary fact in psychoanalysis; on the other, Klein's notion of the depressive position. The question is whether or not her notion should acquire the same status as that granted to Freud's.

[54] From this ANT perspective, the role of a participant becomes limited to that of a spokesperson: he or she, who reveals and speaks on behalf of associations. See glossary.

Since the depressive position is compared against the Oedipus complex, with the latter taken as a biological fact, I need to understand the role of biology, according to what associations and which rationale. This is why I turn in Chapter 6 to the emergence of phylogenesis in Freud's work, in *Totem and Taboo*, which articulates the association of biology with the establishment of Freud's notion of the Oedipus complex as a fact. In other words, if I aim to think about the associations underpinning the depressive position in relation to biology, I also need to understand the equivalent array of associations that led the Oedipus complex to be regarded as a fact. Here I investigate associations relating to phylogenesis through 'historical' references, even though I refrain from relying on archival research. In ANT, the work of a historian is described by Latour (1988) in a specific manner:

> I use history as a brain scientist uses a rat, cutting through it in order to follow the mechanisms that may allow me to understand at once the *content* of a science and its *context*. For this reason the presentation of the documentary materials does not follow the historical path but rather the network of associations that slowly make up the Pasteurian world. (Latour 1988 p 12)

The method above relates to history, but does not relate to the use of historical archival material along a traditional path. Latour makes it very clear right from the beginning of his book that 'I cannot claim [the honour] of being a historian. This undertaking does not purport to add anything to the history of science, still less to the history of the nineteenth century' (Latour 1999b p 12). That is, his use of documentary material is arguably not used for providing new details, original information, according to unearthed sources. It searches not for thoroughness of information and therefore cannot contribute to the literature of history because Latour is interested instead in understanding the workings of network of associations. With my reading of *Totem and Taboo,* I am also 'cutting through history'[55] because I am not respecting the chronological time or 'the historical path'. I am instead reading *Totem and Taboo* to investigate what associations are being invoked behind the Oedipus complex taken as a fact at the Controversial Discussions. In this sense, *Totem and Taboo* may be distant in time from the Controversies, but in close continuity where associations are concerned. Paraphrasing Latour, I do not follow the historical path but rather the network of associations that slowly make up the biology-psychoanalysis world.

My reading approach to *Totem and Taboo* is informed by the relationship between biology and fact production in psychoanalysis. I read the whole text in order to unearth its associations, mapping out how phylogenesis is articulated as a main association, playing a role within the chain of knowledge

[55] The priority is not to follow history, but rather to view the associations that are displayed, established, and imported in its outflow throughout time, as I argue is the case of phylogenesis. See also Serres' influence on Latour (Serres 1995).

production in efforts to elevate the Oedipus complex to the status of an innate fact of development –a universal claim. After this I return to another re-reading of *The Freud-Klein Controversies 1941-1945.* In an iterative fashion, I can now read the text with a kind of attention that is informed by the terms mentioned above (science, fact, truth, biology, depressive position, Oedipus complex) and what I have learned in Chapters 5, 6, and 7. That is, I now have even more key terms through which to look for associations, to show how biology becomes the specific criterion of internal demarcation, how it entrenches the Oedipus complex as a fact, and how it plays out in supporting or negating Klein's claim to the fact of a depressive position.

With the text of *The Freud-Klein Controversies 1941-1945,* I apply the injunction 'to follow the actors' to investigate the presence of a network of associations as they are brought together in the context of the debate. These are, for example, biological notions, phylogenesis, ontogenesis (or psychic development), Mother Nature, clinical cases, totemic societies, 'primitive men', the genetic principle, infant observation, child analysis, the Oedipus complex, Freud and so on. It is through them that the agonistic dispute takes place, according to the alignment of interests, translations leading to changes of scale, efforts to support Klein's claim on the depressive position via associations, and their transformation into a fact. The stronger these networks, the stronger the depressive position becomes in its claim as a fact in phylogenetic terms.

4. Further comments

I am aware of some important limitations of my research design. By tracing associations of actors, the researcher relying on ANT should avoid '"society", "power", "structure", and "context"' (Latour 2005 p 22) as explanatory variables to the social. For all its difficulties in establishing a formal and well-defined methodology, ANT also attempts to be very clear what it is *not* about. As Latour makes the case for associations and the process of negotiations among actors, he refrains from seeking either structural or psychological explanations, as he makes clear in *Reassembling the Social* (2005).

I have tried to be as clear as possible regarding my approach to the Controversial Discussions by teasing out my method in relation to the injunction to 'follow the actors'. With my study, I argue that Melanie Klein was not simply providing a perspective.[56] With her claims, she wanted her

[56] The distinction I make between theoretical perspective and fact also relates to what ANT is *not* about. In this case, with its emphasis on *translations* and *associations*, ANT does not focus on an epistemological discussion that would emphasise and delve into the novelties provided by Klein's perspective. It is beyond the scope of this thesis to provide a full account of the relation and tensions between epistemology and ANT. The point stressed here is that an epistemological break is never enough to establish a scientific fact in ANT. As Schmidgen (2014) puts it, ANT theory shifts instead to the 'social processes whereby knowledge is generated, established, and above all disseminated in a particular society' (p 63). In other words,

findings to be regarded as discoveries, as truth, as facts. The status of these claims, in turn, would affect the outcome of the Controversies, according to the very criterion of internal demarcation that was negotiated in relation to phylogenesis. So I am interested in the network of associations behind the constellation of concepts around the depressive position that were used for this purpose. In line with ANT, then, it is by research design that I do not delve into the ramifications of Klein's concepts, as already mentioned in the introduction. Consequently, this thesis does not focus on the nuances and new epistemological perspectives provided by Klein's views. Instead, the aim of this study is to understand how Klein's notions were articulated as a whole in order to gain facticity, according to the array of articulations underpinning her claims, within an agonistic battle revealed through the text of *The Freud-Klein Controversies 1941-1945.*

ANT, through its constructivism, is more interested in investigating scientists as fact builders, while attempting to evaluate the network of associations underpinning any claim.

Chapter 5: The Good, the Bad, and the Neutral: in search of the path for internal demarcation

The previous chapters have established that internal demarcation evolves in controversy according to the struggle to produce a fact. This chapter investigates how this struggle takes a shape specifically at the Controversies. The process of knowledge production in psychoanalysis has its own specificities, some of them already identified or discussed in the literature – the couch (Krause and Guggenheim 2013), clinical cases (Forrester 1996), technique (Stengers 1997), and biology (Sulloway 1979). It is in relation to these and other arrays of associations within the psychoanalytic chain of knowledge that a strategy for fact production emerged at the scene. My task here is to follow negotiations through a systematic reading of public interventions at the Controversial Discussions. More specifically, I intend to identify which associations are taken as central in efforts to black box Klein's claims around the depressive position, as these associations are revealed through participants' – or spokespersons' – interventions. Furthermore, I qualify the potential demarcating moves from an ANT perspective, considering that there is no such a thing as an a priori true or false statement, but only good or bad paths involving associations, which may strengthen claims or otherwise, and eventually lead to fact production. I distinguish four different moves as good, bad or neutral[57] in their capacities to affect and establish a path for internal demarcation at the Controversies.

As mentioned in the previous chapter, I am mainly focusing my investigation on the period between January 1943 and May 1944, comprising what King and Steiner, in their editorial work, call the second, third, and fourth sections of *The Freud-Klein Controversies 1941-1945*. The second section begins at the first Scientific Meeting with Isaacs' opening paper on unconscious phantasy and the ensuing discussion. Five discussion meetings took place after Isaacs' presentation. Then, Paula Heiman presented her paper on introjection and projection in early development, followed by another two rounds of discussions. Therefore, the second section comprises two presented papers, and seven discussion meetings, spanning January 1943 to November 1943. In keeping with a chronological order of documented events, King and Steiner regard section three as comprising mainly a series of memoranda, extracts of minutes for Training Committee meetings, a draft report, and a final report. The fourth section resumes debate within the format already established in section two, beginning with a paper on regression by Heimann and Isaacs, followed by two rounds of

[57] Good, bad, and neutral, refer to the quality of associations in ANT, in their capacity to produce facts.

discussion between December 1943 and February 1944. Melanie Klein then presents her paper on the infant emotional life in relation to the depressive position, which was followed by another two discussions, the last one taking place in May 1944.

Although I occasionally refer to sections one and five, my analysis concentrates upon sections two, three, and four, where debate was mainly directed in response to Klein's claims, ideas, and technique. With this, my intention is to survey how potential criteria of internal demarcation emerge.

1. Claiming the truth: a bad demarcating move

With her paper on unconscious phantasy, at the very outset of the Scientific Meetings, Isaacs already invokes Klein's notions as facts. In the opening pages of her paper, for example, Isaacs writes that an expansion on the meaning of the term phantasy should take place 'for a good reason – because the facts, and the theoretical formulations they necessitate, require it. It is *the relationships between facts* which need to be looked at more closely and clarified in our thought' (King and Steiner 1991 p 268). With these claims, then, Isaacs posits a factual underpinning to Klein´s propositions to give them an unquestionable status at the outset of the Controversies. So the truth in psychoanalytic knowledge would be, by necessity and from the beginning, on Isaacs' side. She reinforces her stance a few paragraphs later, saying 'our knowledge of early mental life has developed (…). It is the facts which have compelled this extension' (p 271). She reprises this strategy in her conclusion: 'the *concept of phantasy* has been gradually widened in psychoanalytical thought. It now requires further expansion and clarification, if it is to integrate all the relevant facts' (p 313). There, Isaacs also adds that 'Melanie Klein's views as to the early phantasy life of the infant, and the concept of the "depressive position" take full account of all these facts' (p 309) – these facts being a series of observations by Merrell Middlemore (1941) in her study of the new-born and young infant. So, when the debate at the Controversies had hardly started, in Isaacs' view, not only does the factual speak for itself, but other sources of data provide 'full confirmation of Melanie Klein's hypothesis as to (…) early wishes and phantasies' (King and Steiner 1991 p 304).

Claiming to have the truth on one's side is one of the strategies adopted to create internal demarcation at the Controversial Discussions. This may work, as long as we sustain a modern understanding of reality, where truth or facts are a priori givens. However, claiming the truth on one's side is hardly a solution from a constructivist perspective, because it precludes discussion and argumentation – the very process of wherein associations operates, and by which truth is constructed in controversies. For this reason, I consider Isaacs' internal demarcation strategy to be a bad one because such a strategy does nothing but simply invoke an a priori truth, in a tautological strategy, akin to an ineffective statement such as 'I am right because I am right in advance'. Put differently, these kinds of assertions incur in ineffectively precluding dispute before one has even properly started. A bad demarcating move also has a specific meaning in ANT: it designates weak claims underpinned by a poor chain of associations, which hardly offers the means to attract and align other actors' interests. Simply attributing a factual character to one's statement hardly does anything to effectively black box the claim. As a result, Isaacs' assertion of facticity is a bad move in internal demarcation because we are offered hardly any glimpses of the array of articulated actors that would otherwise support the factual status of the depressive position beneath such a statement. Isaacs's assertion does not display any array of associations that could strengthen the case of her statements. Simply adding the term 'truth' to Klein's views does not contribute at all to turn opposition into allies, and so it does not help to strengthen Klein's side in the agonistic battle for knowledge production at the Controversial Discussions.

I single out Isaacs as the central spokesperson at the beginning of the actual debate to highlight the ineffectiveness of this strategy. However, she was not alone in employing this strategy of 'claiming the truth' since it is clear from the unfolding of sections two, three, and four of *The Freud-Klein Controversies 1941-1945* that other participants also offered comments pursuing this line of validation of Klein's claims. In what Jones regarded as the pre-existence of facts, he comments on Isaacs' paper in the first discussion of the Scientific Meetings that 'clearly no a priori arguments (…) can settle the question. It is a matter, as elsewhere in science, of inference from the data' (King and Steiner 1991 p 324). Here, Jones echoes her invocation of facts speaking for themselves, since it would be only a matter of observing data.[58] Along similar lines, in the same discussion, Glover disagrees with the validity of Isaacs's assertions while reinforcing the same demarcating strategy, noting that 'we must decide how much of Mrs Isaacs' argument is based on facts and how much on her own (that is to say Mrs Klein's) favoured interpretations of unconscious contents' (p 325). Furthermore, while the terms truth, data, and facts were invoked from the outset of the debate as uncontroversial and belonging to an a priori reality (and therefore not needing to be produced, only found),[59] the same ineffective demarcating stance persisted. For example, following Jones' intervention mentioned above, Isaacs responds, now in the second round of discussions, that: 'only

[58] ANT reverses this logic: it is not a priori data that defines controversy, it is controversy that defines a posteriori what data is. In more theoretical terms, this is a consequence of how to think about reality, whether in modern or non-modern terms. See Chapter 3.

[59] ANT opposes the mode of finding or discovering reality, and replaces it with producing reality.

close observation and discussion of empirical data can carry us further' (p 368). References to Freud were also quoted in order to invoke an a priori truth, as when Isaacs notes in the fifth round of discussions that 'it is true that in all our minds theories have a way of becoming rigid and blurring our vision of new facts (…). We should struggle against such tendencies in our scientific thinking. Freud has always emphasised the empirical character of psychoanalysis. E.g. in the Encyclopaedia article [(Freud 1923)]' (p 445). Heimann reinforces this strategy of 'claiming the truth', by also invoking the a priori nature of facts, in the seventh round of discussions: 'Freud in *Beyond the Pleasure Principle* starts from clinical observations' (p 573). In a similar fashion, Heimann's paper (second within the Controversies) characterises Klein's notions of projection and introjection 'as having such very great significance; it is their derivation from inherent biological facts that leads us to attribute such essential effect and functions to them' (p 507). We can also see the same line of demarcating strategy persisting right until the end of debate, when Klein presented the final paper for discussion. There, she accorded priority to observation and invoked truth as stemming from an independent and shared reality: 'we return once more to facts. A number of workers have been able through the analyses of young children (…) to establish beyond doubt the operations at this stage' (p 762), so that 'in the long run, all of us should be able to check them' (p 788).

My intention here is not to exhaust the occasions in which 'claiming the truth' emerges in *The Freud-Klein Controversies 1941-1945.* Rather, I consider these examples, in their specific context at the Controversies, to show that such a demarcating strategy is highly ineffective in the beginning and, as such, any persisting claims along a similar logic are mere noise, signifying nothing where the work of associations and the agonistic struggle are concerned.

2. Neutral demarcating move

There is hardly any direct reference to clinical material, at any stage of the Controversies. This absence has already been noted (e.g. King and Steiner 1991; Roazen 2000), and is as surprising as it is a distinctive feature of the Controversial Discussions. Roazen (2002), for example, notices that 'virtually no clinical material whatever appears [during the Controversial Discussions]. One rare clinical example has to do with a Kleinian mentioning a girl of sixteen months who played a favourite game with her parents' (p 76). This absence of clinical material does not mean that Klein's assertions refrained from relying on clinical evidence. On the contrary: in the first text presented during the Scientific Meetings, Isaacs puts forward two sources of evidence based on clinical work. First, there was evidence 'gained from the actual analysis (…) of young children, particularly of the ages two to three years' (King and Steiner 1991 p 298). Second, Isaacs notes that there was 'confirmatory experience in the analysis of adults, by the various analysts who have for some time made use in their work of the various facts and hypotheses regarding early development, first formulated in the analysis of young children' (Ibid.). However, although this evidence was presented, it was barely discussed, and the meagre material mentioned by Roazen (2002) only arises at the very end of the Controversies, in the fourth section of *The Freud-Klein Controversies 1941-1945*.

I identify the lack of clinical discussions as a neutral demarcating move because it fails to affect the dynamics of internal demarcation in spite of an array of associations connected with clinical cases. Given the laboratory-couch analogy developed within the STS literature, and the importance of clinical activity for psychoanalytic knowledge, the participants in the Controversial Discussions would have been expected to use their work with patients, and associations related to it. Yet, at the Controversies as a scene, participants did not steer internal demarcation towards their 'laboratories'. Several references are made to clinical cases, but only ever in passing. Clinical material never becomes the focus. Right at the outset of the second section of *The Freud-Klein Controversies 1941-1945*, Isaacs for example writes in her opening paper that, where clinical evidence and facts are concerned, '[it] has been constantly provided in published papers and books. (…) one has only to turn to Melanie Klein's book *The Psychoanalysis of Children*' (p 266). She later concludes in the same paper that 'it is not possible for me to offer case material evidence in this paper, from the analysis of young children. Nor is it necessary. I may and shall assume that my readers have some familiarity with the many papers which have been published by Melanie Klein and her co-workers' (p 299). So, clinical cases were summarily dismissed as unnecessary for the purposes of discussion at the Controversial Discussions, or at least characterised as secondary for internal demarcation.

To reinforce this pattern of absence, other participants did express the need to turn to clinical material, and still none was provided. For example, Jones responds to Isaacs' presentation saying that concerning 'the central problem of whether phantasy does or does not occur in the first year of life (…) I would suggest that the most profitable form the discussion can take place would be to concentrate on two or three examples, either analytical or observational, and compare alternative explanations of them with the aim of ascertaining which of these most closely and comprehensively cover the facts' (King and Steiner 1991 p 324). Yet his suggestion was not taken up. As the lack of clinical material persisted, there being no clinical material used more than halfway through the Controversies, Glover notes at a much later stage in the Controversies that 'as far as the central issues are concerned, viz., the validity of Mrs Klein's theories and practice, and of the clinical evidence on which these are based, I need only remind you that so far we have not touched on these issues at all' (p 710). This is Glover's intervention at the sixth round of discussions, in the fourth section of *The Freud-Klein Controversies 1941-1945.* His comment is made in reaction to there being no clinical material used up to that point in the debate, more than halfway through the Controversies. This absence of clinical material in centre stage is clear throughout *The Freud-Klein Controversies 1941-1945*. So, even if participants such as Jones or Glover proposed the need for clinical material, offering to steer debate on internal demarcation towards this path, no clinical material was put forward other than in passing.

While refraining from bringing it into the discussion, Klein's supporters insisted on the existence of previous clinical material as evidence. In the eighth round of discussions, for example, Heimann says that 'when we were asked (…) to present our views for the purpose of these discussions, we were not requested to prepare clinical papers. We were expected to give a systematic account of our theoretical standpoint on particular issues (…). In such papers clinical data can only be used to illustrate (…). To present clinical data fully would occupy a far greater time and space than the framework of those discussions could possibly allow' (pp 732-3). The same pattern of alluding to clinical material but failing to articulate it as a criterion of internal demarcation takes place with one of Klein's interventions, when she makes an indirect reference to clinical material at the very last round of discussions. But once more it is only summarily presented: 'a number of workers have been able through the analyses of young children from about two years onwards to establish beyond doubt the operation (…) of feelings of anxiety and guilt' (p 762).

Considering the above, there is consensus among participants that no clinical case was brought forward to substantiate Klein's claims. But understandings of that absence were conflicting, in line with the opposing stances in the debate. Klein and her followers assumed that the necessary clinical case data had already been demonstrated through previous publications, and established according to cases worked upon by 'a number of workers'. So there was, in their view, no need for clinical work to constitute a relevant stage in the process of knowledge production in psychoanalysis. Corroborating this perspective, Jones for example comments in the first Scientific Meeting that 'the presence of such phantasies in older subjects had long ago led me to deduce the actual existence of them in young infants, and this inference has in my opinion been amply confirmed by the analysis of them carried out by Mrs Klein and others' (p 324). But while some participants took the clinical work for granted, others framed the lack of clinical evidence as a challenge. Such was Glover's perspective, when he insisted on the problem of the absence of clinical material up until his departure from the debate. Anna Freud held a similar view when she noted that 'as regards the aims of the series, as set out on page [216], (a), I cannot see that the conditions of the discussion provide any opportunities for establishing either the validity or the importance of any given views' (p 328). With (a), Anna Freud made reference to the question raised during the setting up of the Scientific Meetings (p 216), concerning the type of evidence supporting Klein's views.

Exemplary cases (Forrester 1996) could have been used at the Controversial Discussions and developed into the internal demarcation of Klein's claims. Yet, in its absence, and for all its array of associations, clinical material simply failed to become relevant throughout the Controversies. So it remained neutral, as a non-factor concerning the dynamics of internal demarcation. From an ANT perspective, then, no chain of associations, translations, and network of actors were explicitly brought forward. Agonistic negotiation upon the validation or rejection of Klein's views rather took place couched in biology, and in its related associations in the process of knowledge production in psychoanalysis.

3. **The emergence of a path for internal demarcation**

With the Controversies at an impasse following the first round of discussions, Anna Freud proceeds by introducing an alternative pathway to internal demarcation: 'I therefore concentrate on the point of compatibility or incompatibility or, to put it in different words, I shall try to show, more clearly than has been done in the paper before us, what consequences the acceptance of Mrs Klein's views has for the theory of psychoanalysis as a whole' (p 328). In this sense, Anna Freud proposes to steer the issue of validating Klein's notions according to 'the point of compatibility or incompatibility' between Klein's formulations and Freud's established views. In my view, this helps to negotiate order at the scene. More than this, I identify Anna Freud's intervention as a fundamental one, from which the criterion of internal demarcation begins to emerge, and which will organise the rest of the Controversies.

In Anna Freud's proposal to use theory to highlight the problem of compatibility between Freudian and Kleinian views (pp 328-9), she also enumerates the problematic consequences of subscribing to Klein's views on unconscious phantasy as presented by Susan Isaacs. She, for example, notices that the term 'phantasy' widens the gulf between conscious and unconscious processes as established in Freud, obscures the usage of instinctual processes in mental activity as in Freud's libido theory, and shifts from a sensorial corollary based on pain-pleasure to an imaginal experience, thereby moving away from Freud's pleasure principle (King and Steiner 1991 pp 328-9). Others then took up this strand of argumentation on theoretical incompatibility. For example, in the first round of discussions, Glover states that, with her paper, 'Mrs Isaacs' main concern is to build up a new metapsychology (…). Anyhow, if Mrs Isaacs' new metapsychology is right, then Freud's metapsychology must be wrong, for the two are incompatible in a number of respects' (p 325). Glover insisted on this point adding at another occasion that the term 'phantasy' 'fails to distinguish between the terms "imaginary", cathexis of memory-traces, and presentations' (p 398), and as a result would make a confusion of several other Freudian distinctions such as those involving 'images, ideational representations, end-presentations of instincts, reality thinking and phantasy (…) [and] cathected image memory from a phantasy' (Ibid.). In other words, the theoretical implications proposed by Klein were to be compared with Freud's views: at best, they could add to the psychoanalytic corpus as a compatible extension; at worse, they should be rejected as incompatible. Freud's daughter cements this path of validation when she recasts the issues mentioned above in the fourth round of discussions. Seeking to reinforce the same question of compatibility, she states that there is '[an] outstanding difference between Mrs Klein's theories and psychoanalytic theory as I understand it' (p 421). In so doing, she lays the ground for validation in theoretical terms, while also stating that Klein is not compatible with Freud. Yet, if theoretical (or metapsychological) incompatibilities were raised and discussed, I see this as a strategy for internal demarcation, concerned with the production of facts, emerging in relation to developmental (i.e. ontogenetic) issues.

With the issue of theoretical compatibility, Anna Freud also launches her criticism that 'the nature of the unconscious phantasies described as existent in the first year of life completely alters the conception of unconscious life as such' (p 329). Connecting theoretical compatibility with the first year of life in psychic development, she notes that unconscious phantasy and associated terms such as the depressive position, 'shift all emphasis from the later stages of development to the earliest ones' (Ibid.). So, the question of metapsychological compatibility is not simply cast as a theoretical one. Within Anna Freud's very central intervention, she is also offering a path of negotiation of internal demarcation that relies on, while moving beyond, theoretical compatibility between Klein and Freud. The key question here, then, becomes what necessarily takes place in the first year of life as a stage of development. As a result, I think that the debate on theoretical compatibility subtly hinges on a developmental argument, since so many metapsychological[60] issues were organised,

[60] With my discussion above, I am not trying to exhaust the subject of metapsychology. There are indeed several further moments during the Controversial Discussions where the implications of different aspects of Klein's theoretical – or metapsychological – work came to the fore. Brierley, for example, was mostly concerned with the equation between interpretation and psychic contents (King and Steiner 1991, p 331). Fairbairn similarly had his own take on notions such as internal objects and inner reality, discrediting Klein's reliance on phantasy in object relations (p 359). Another example of metapsychological implications at the Controversies is discussed, for instance, by Perelberg (2006). Revisiting the Controversies, she is mostly concerned with the question of temporality, and expands on what she sees as a metapsychological argument on temporality provided by different analysts during the debate. However, I focus on metapsychology in its role in the collective negotiation for internal demarcation at the Controversial Discussions as a scene.

justified, and understood according to a developmental timeline. Indeed, Anna Freud makes a full case on the issue of developmental compatibility throughout a long intervention at the fourth round of discussions. There, she links Klein's notions of object relations, love and hate in the new-born infant and early unconscious phantasies, and other concepts, with Freud's views on development in terms of narcissism, auto-erotic phase, and so on (pp 417-424). Therefore, for the purposes of internal demarcation in psychoanalysis, the debate becomes couched in whether or not Klein's views on development could be established as an extension of Freud's notions of libidinal development that lead to the development of the Oedipus complex (p 423).

Following Anna Freud's lead, Friedlander refers to this logic of development underpinning the debate saying that 'Mrs Klein's view of the "early mental development" (…) has given up Freud's theory of the development of libido' (p.343). Friedlander adds that Freud's conception of libidinal development is 'based on the biological tendency of the instincts to develop' (p 346), whereas 'we can only perceive [early phantasies] as either being inborn or universally formed in the first months of life (…) [so that] the process of development is seen in a more static form' (p 346). Others also joined the debate on internal demarcation in developmental terms, indicating at the same time what array of associations within knowledge production were most relevant in this context. Isaacs, for example, notices that Klein was interested in understanding the steps and 'all the varied paths of development from the earlier to the later phases' (p 370), between unconscious phantasies and Freud's Oedipus complex. Isaacs maintained the compatibility between Klein's views on early development and Freud's, saying: 'I do most definitely claim that Melanie Klein's views are derived from Freud's (…). They are in large part identical with his' (p 377). Furthermore, she added that 'Mrs Klein makes it quite clear that she adheres to the concept of successive biological stages of the libido, but that she considers these phases to be less spread out' (p 379), while also connecting libidinal development with early sadistic phantasies and specific anxiety situations (p 380). In terms of associations, we can already identify here a series of actors such as the Oedipus complex, early development, Freud, biological stages, libido, and so on. In terms of internal demarcation, we can identify a dispute taking place, in a to-and-fro dynamic, with supporters and opponents fighting around the developmental issue.

In hindsight, the developmental perspective of the debate – with its implicit ontogenetic and phylogenetic basis – had been proposed beforehand. In section one of *The Freud-Klein Controversies 1941-1945,* Sylvia Payne (then future president of the British Psycho-Analytical Society) argued in the

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Hence, my use of Anna Freud's quote indicating the exact moment wherein participants turned towards theory and away from clinical work and the couch, and the connection between 'theoretical compatibility' and developmental grounds.

first Business Meeting (before the series of Scientific Meetings where actual debate took place) that psychological research had difficulties in establishing objective evidence in relation to natural sciences (King and Steiner 1991 p 53). However, she also asserted that:

> I cannot see why people with a true scientific outlook should not work in the same society on different aspects of mental functioning, some relating to the most primitive phases of mental development, and others on more differentiated parts of the mind, conscious and unconscious (…). In my view the new work [from Klein] could and ought to be brought about into a balanced relation with the old and if it is not it will be owing to undue claims and emotional factors in all of us (p 54).

Payne makes reference to a frontier of knowledge that should be compatibly expanded ('ought to be brought about into a balanced relation with the old') as if psychoanalytic knowledge of mental functioning should be uncovered on a cumulative basis within a developmental perspective. In other words, Payne seems to suggest that different psychoanalytic explorations should cohere on this basis, within a developmental perspective, where claims override clinical discussion and facts can be produced in terms of both biological progression ('phases of mental development' or ontogenetic perspective) and universal transmission (innate, inborn, or phylogenetic perspective). This is what I think Anna Freud's intervention ultimately unleashed after she began with the question of theoretical compatibility.

So, having ruled out strategies for internal demarcation based on 'claiming the truth' and clinical cases I identify the emergence of a criterion for internal demarcation that focuses on a specific array of associations, and becomes central to the struggle for the fact production of Klein's claims. While this criterion stems from a more general debate on theoretical compatibility, it evolves centrally in relation to questions of innate developmental aspects of the mind in the first year of life. The theoretical compatibility discussed above would depend on the 'truth' about the progression of psychic development at this early stage, which needed to be validated. That is, theoretical expansions and compatibility would hinge on the validation of the depressive position, prior to the Oedipus complex. I now turn to how this criterion of demarcation evolved to take centre stage at the Controversial Discussions.

4. A good demarcating move

From an ANT point of view, it does not fundamentally matter whether or not the developmental perspective coheres with mainstream biology. This is only important inasmuch as the literature concerning Freud and biology focuses on whether or not his views subscribed to Lamarck's theory instead of Darwin's (e.g. Litvo 1990; Young 2006). What is more relevant in understanding the dynamics of the Controversial Discussions is simply identifying the use of biological notions, such as ontogenesis and phylogenesis, as part of a major collectively negotiated strategy of internal demarcation. From an ANT perspective, 'following the actors' in this case means identifying a preference for a demarcating path, within the process of knowledge production and towards the articulation of a fact that could end controversy.

I have shown in the previous section how the criterion of internal demarcation emerges out of Anna Freud's suggestion for theoretical compatibility, leading to a framework of biological progression within which facts and knowledge were taken for granted (Freud's Oedipus complex within the libidinal development, for example) or disputed (Klein's depressive position, unconscious phantasy, at the first year of life). This pattern emerged in the first rounds of discussion in section two of *The Freud-Klein Controversies 1941-1945.* Now, I will show how the question of development as a biological progression unfolded and was consolidated as the criterion of demarcation of the debate throughout the rest of the Controversies. Here, Isaacs' intervention encapsulates and sets the tone for the debate:

> We know that every child passes through the Oedipus complex. We find it determining the character and history and neurosis of every adult patient. (…) Exactly the same applies to the "depressive position" and early psychotic phantasies (…). On Melanie Klein's view, every infant and young child experiences this phase of development (King and Steiner 1991 pp 458-59).

This intervention took place in the fifth round of discussions, that is, four rounds or four months after Anna Freud's major intervention. In her quote above, Isaacs proclaims the Oedipus complex to be universal ('every child passes through'), a law and definite form of relation (Oedipus complex determines the ensuing character and 'neurosis of every adult patient'), and a given enforced independently by human contingency. Furthermore, Isaacs explicitly puts forward the demarcating path, once she posits a factual status by proxy to the Oedipus complex for Klein's depressive position ('exactly the same applies to the "depressive position"'), in developmental terms. Although I am quoting one of Issacs' interventions, my analysis is not based on 'discourse' or on Isaacs' subjective opinion. Rather, I am using her statement to tease out associations, from an ANT perspective, as they connect with the issue of internal demarcation. Through Isaacs' words, we have some access to the associations that allow the Oedipus complex to become a black box, and to the reasoning as to how these same associations could also be applied to the depressive position. So it is in this very moment, with Isaacs' quote, that I am applying the ANT method of using controversies as 'hotspots', as special situations where associations can be studied. In this respect, unlike 'claiming the truth', there is no simple tautological statement in Isaacs' intervention. Threads of associations within knowledge production are being invoked, in terms of development, while also inviting us to consider what relevant associations underpin the black boxing of the Oedipus complex as innate, as a fact of development. Therefore, we have here a roadmap, with a fact (Oedipus complex), a contending claim (depressive position), a hint of the grounds upon which the fact was produced (the array of associations centred on biological development), and the demarcating aim of obtaining the same status for Klein's claim on similar grounds. I do not take this roadmap as simply a discourse or a matter of opinion since beneath Isaacs' intervention it is possible to start identifying all these aspects, from associations to black boxes. Therefore, with Isaacs' intervention in the fifth round of discussions, we are offered a whole internal demarcating programme, and glimpses of associations within the process of knowledge production in psychoanalysis.

As discussed in Chapter 2, the subject of child development puts forth the question of the genetic principle, according to a search for knowledge based on the modularity of basic mental operations. One of these basic operations, consensually agreed upon and taken as a black boxed developmental fact, was the Oedipus complex as indicated above. Through a collective negotiation at a scene, it was now the depressive position that was to be designated as coherent or otherwise, and accepted or not as a fact in the same way as the Oedipus complex – according to a developmental logic couched in biological terms. Where internal demarcation is concerned, other participants reinforced the role of biology. For example, Brierley stated during the Controversies that 'I accept the principle of genetic continuity in mental life, our old friend the law of psychological determinism in modern dress' (p 331). In so doing, Brierley reinforced the importance of associations that connect psychoanalysis with psychological laws, and hints of biological determinism. Further on in her same intervention, she stated 'indeed I can conceive that there may be phylogenetic determinants at work in the baby's mind' (p 331). I think it is within this blended arena of associations, comprising psychology and biology, that a quest for producing a fact concerning Klein's notions becomes most pressing. For example, Isaacs proposes the 'fundamental fact of general development (…) namely, the fact of genetic continuity' (p 285). She adds that 'probably no psychoanalyst would doubt the fact of genetic continuity (…). It is a concrete instrument of knowledge, enabling us to trace threads of development backwards and forwards' (Ibid.). Similarly to Brierley, then, Isaacs equated natural sciences with laws in psychology through the blending of psychological and biological notions. Furthermore, while discussing her paper on phantasy, Isaacs reinforces the association of psychology and biology as the primary ground for demarcation, stating that Dr Friedlander's contribution was absurd, since ‘it overlooks the phylogenetic sources of knowledge, the fact that such knowledge is *inherent* in bodily impulses as a vehicle of instinct’ (pp 451-52). As Scott asserted in this same context, ‘the attempt to seek the chronology of the states of phases of development in their genetic continuity should always be before us’ (p 353).

Another good example in the Controversial Discussions displaying efforts towards producing a fact along these lines is provided by Paula Heimann’s intervention towards the end of the second section of *The Freud-Klein Controversies 1941-1945*. In her paper on introjection and projection – the second text presented at the Controversies – she writes that:

> Our understanding of the complicated processes and conditions of the mind has been made easier by Freud’s comparison of it with the simplest organism, the amoeba, (in, Freud, S. 1914:33, SE 14:74) and by him showing us that the mind is to be regarded as a living organism subject to the same general biological laws (King and Steiner 1991 p 507).
>
> It is useful to remember genetic development and to apply the laws of life, which are so clear in a simple organism, to the highest form of life (Ibid.).
>
> The patterns Nature uses seem to be few but she is inexhaustible in their variation. It is on the basis of these considerations that Melanie Klein and her co-workers regard these processes [*posited by her]* as having such very great significance; it is their derivation from inherent biological facts (Ibid.).

The quotes above find resonance with Isaacs’ demarcating programme. They also indicate how her proposal of comparing the depressive position to the Oedipus complex in developmental terms persisted throughout the debate. At the same time, the more these examples reinforce this criterion of demarcation, the more they reveal an array of associations sustaining fact production; in this case, in terms of how the Oedipus complex is – and the depressive position should be – articulated for black boxing purposes in relation to actors such as Nature, laws, phylogenesis, genetic continuity, psychology, development, and other associations. This has a double effect: On one hand, more of the network of actors is revealed. On the other, there is more evidence to make the case that internal demarcation took place in biological terms. So my understanding of the role of biology relies on the clarity of terms proposed by Isaacs, on Anna Freud’s comments on compatibility, and also on the recurrence of examples using this logic of biological progression to measure Klein’s claims. Many, if not the majority, of these examples took place after Isaacs’ unofficial announcement of a demarcating programme, as in Heimann’s intervention at the seventh round of discussions in section four of *The Freud-Klein Controversies 1941-1945*:

> We follow Freud in using a biological approach to the psychological formulations required by the observed clinical data, and in his attempts to relate psychological phenomena to the wider field of biological processes. (…) His whole approach to the mental life is based on the view that psychological phenomena are *genetically and functionally* related to biological facts (p 579).

To the list of actors presented above, here we can add further associations relating to clinical data, psychological phenomena, and biological process, as newly revealed or variations of previously displayed actors. Klein also reinforces this trend of revealing while reinforcing biology as a criterion of demarcation, when she described human patterns in terms of biological laws, such as phylogenetic inheritance and ontogenetic development. At the end of the debate, presenting the final paper of the Controversial Discussions, she notes that 'I shall endeavour to show the part which early depressive feelings play in the whole of the emotional development during the first year of life' (p 753). So, in order to assert the validity of her views, she puts forward her findings as facts stemming from biology, rather than focusing on clinical cases. She posits for instance that 'the relation of the infant to his mother [*'s breast*] is based on phylogenetic inheritance and is ontogenetically the most fundamental of all human patterns' (p 757), applying the same framework to her depressive position, consistently referring to it as 'facts' (p 762), a 'universal phenomenon' (p 768), 'the early stages of development' (p 770), and applicable to the 'infant as a living organism – which means psychologically as well as biologically' (Ibid.).

Together, these papers and interventions consolidate a pattern, organising the debate around arguments on internal demarcation related to biology. This began with Isaacs' unofficial programme at the fifth (out of ten) round of discussions, and persisting until the end of the Controversies as in the case of Klein's interventions, quoted above. Another example is when Heimann replied (in the seventh round) to comments on her text on introjection and projection saying that 'we follow Freud in using a biological approach to the psychological formulations required by the observed clinical data, and in his attempts to relate psychological phenomena to the wider field of biological processes' (p 579). Also, the subsequent text on regression (third of four papers) asserts that aggression, orality, and anxiety are compatible and evolve alongside Freud's libidinal developmental lines. There are many other examples of how biology is negotiated as a demarcating criterion at the Controversies, indicating how an acceptance of biological facts and laws can be identified in *The Freud-Klein Controversies 1941-1945* as shared by others, at different stages of the debate, as with Hoffer (p 427, p 550), Friedlander (pp 539-44), and Glover (p 379, p 588). For or against Klein's claims, these numerous examples spread out at the Controversies in what I see as a veritable collective arrangement, with biology consolidating itself as the main criterion of internal demarcation as the Controversial Discussions unfolded.

Unlike the dynamics of 'claiming the truth', a bad demarcating move, I consider the comparison between the depressive position and the Oedipus complex in developmental terms to be a good demarcating move. Since to be good, in ANT, means having the capacity to invoke articulations and therefore add weight to one's position within the struggle to produce a fact, the move towards biology is regarded as a good one because it indicates a path towards the eventual production of a fact, while also relying on an array of associations such as biological grounds, Freud's texts, the Oedipus complex, phylogeny, the genetic principle, metapsychology, psychic development, and so on. It is also in this context that I argue that the depressive position is the main actor to be followed here, in relation to the Oedipus complex. With this good demarcating move, participants identify the existence of the Oedipus complex as the established fact in psychoanalysis against which a new claim needs to be measured. Such is the case of Klein's notion of a depressive position, whose status at the Controversies is uncertain and disputable – unlike the black boxed Oedipus complex.

5. Another neutral demarcating move

During the Controversial Discussions, an important debate at the Training Committee took place alongside the series of paper presentations and discussions at the Scientific Meetings. The stated aim of this parallel debate was, as King puts it, to consider 'the effect of the scientific differences on the training of Candidates' (p 593) or, as Strachey writes, 'to consider the ways in which current theoretical controversies affect matters of training' (p 602). Formally instigated by Glover and organised by Strachey, a discussion on the training of candidates finally took place through memoranda on psychoanalytic technique provided by members of the Training Committee. This specific debate is recorded in section three of the book, with presentations by Brierley, Anna Freud, Klein, Sharpe, and Payne, with an additional draft final report provided soon after by Strachey.

As a guideline for those writing memoranda on technique, Strachey suggests that members should focus on 'the essentials of a valid psychoanalytic technique (…), in what respect the parties to our current controversies diverge from those essentials' (p 609). With a focus on technique, participants offered different angles and glimpses of how they work clinically, and therefore on aspects concerning knowledge production in psychoanalysis. However, I do not think these insights were transformed into a criterion capable of establishing a valid and essentially normative technique, or of influencing the struggle to black box Klein's findings.

The subject of clinical technique is indeed a sensitive matter connected with several issues. It concerns, for example, institutional questions such as the training programme, the training analysis of candidates, and those who should be allowed positions to teach. However, for all the broad implications related to the debate, instead of linking technique with the production of facts, the debate turned mainly into a discussion on subjective[61] perspectives of technique and the issue of education concerning training candidates. Brierley, for example, points to the aim of her clinical work in terms of 'ego-reintegration on a reality basis' (p 618), and freeing oneself from 'id and/or superego tyranny' (ibid). She also provides comments on counter-transference and her misgivings in relation to Klein's technique: Brierley has 'impressions that her technique and that of her close colleagues may tend to stereotype, to selective emphasis and various forms of transference suggestion' (p 624). In other words, Brierley was concerned with the idealisation in the application of technique informed by Klein's work. In her memorandum, Anna Freud points to different emphases led by contrasting techniques. For her, Klein would almost exclusively emphasise transference material, 'attributing the greatest pathogenic importance to mechanisms of introjection and projection' (p 631). Klein, in her turn, writes about relying on transference interpretation from the beginning of analysis (p 635). She places emphasis on analysing according to differentiating 'phantasy products and the perception of reality' (p 636), while giving attention to destructive impulses, guilt, and anxiety (pp 637-8). Sharpe focuses on the idea that 'it is an illusion that a valid technique can be taught in the way that facts are taught' (p 644), since for her technique is above all a sort of craft, influenced by specific situations, clinical experience, and individual development. As for Payne, she also speaks of differences in technique in terms of individual perspectives: 'therapy cannot be an exact science' (p 652).

It is hard to see how a debate centred on technique could directly influence the dynamics of internal demarcation of Klein's notions, seeing as it considered technique from a personal perspective. So technique is associated with: attention to specific emotions such as guilt, anxiety, and aggressiveness; counter-transference in the analyst, and the risk of idealisation; technique as an individual process of learning; and the handling and use of transference. While it is understandable that technique was taken up along these lines in psychoanalysis, emphasising the particular and the emotional aspects, it is hard to observe matters of associations and the work of translation strengthening factual claims, because no thoughts were offered on how the subjective experience could lead to the production of facts. In other words, the discussion on technique reveals associations (e.g. transference, setting, counter-transference) but fails to connect them with an

[61] See glossary on ANT use of subjective versus objective, where associations are concerned.

account of how the depressive position could be produced as a fact. This is exactly what Strachey, who oversaw and formally led the discussion on technique at the Controversial Discussions, does when he segregates technique from Klein's claims: 'a valid technique is not necessarily or even chiefly *the product* of our scientific findings and theories, (…) it is rather the most efficient instrument by which they can be reached' (p 607). Furthermore, Strachey also writes, in his final report, that:

> The Committee is thus not *directly* concerned with the truth or falsity of the views upon theory and practice held by a Member of the Society. It is concerned only to judge which of those views most fully represent the general opinion of the Society and how best to incorporate them into the educational scheme. Its functions, in short, are executive rather than scientific (p 654).

With no concern for validity and findings, his statement reinforces the neutrality in which I regard the debate on clinical issues in Section 2 above, because the discussion on technique did not lead to matters where translations could align interests, convince actors, or shut down opposition to claims. Technique remains instead, for all its array of associations, a matter of personal perspective, through which hardly any work underpinning the process of knowledge production of facts in psychoanalysis could be more consistently followed, and where different takes on technique were accepted as valid ('not *directly* concerned with the truth or falsity of the views'). In relation to the dynamics of demarcation at the Controversies, Strachey arguably distances himself from such a debate, segregating the 'educational scheme' from a 'scientific' one. As a result, Strachey's final report recommends only general principles, and is concerned with bureaucratic changes related to the Training Committee (p 678). If anything, the question of internal demarcation was taken as a prior and separate issue, that once sorted out could then inform more profoundly and decisively the debate on technique. In this context, Brierley writes that 'I am grimly determined not to pre-judge (…). On the contrary I propose to insist upon my right to make up my mind on the evidence I hope Mrs Klein will be good enough to provide, both in this inquiry and in her forthcoming paper' (pp 627-8). Along similar lines, Strachey adds that 'if our Society were a purely scientific one, we could cheerfully leave the settlement of such problems [of additions to knowledge] to the passage of time and to the amassing of more facts; but the educational side of our work (…) requires more than a merely expectant policy' (p 654). In other words, the issue of technique was segregated as an educational matter, rather than becoming an instrumental part in the debate of internal demarcation and knowledge production. For all associations that could be followed through this thread of technique, they failed to be negotiated as central to demarcation.

In the absence of connecting Klein's claims as true or false with technique, another potential demarcating move would be to rule out Klein's way of working as non-psychoanalytic or faulty. Consequently, all of her claims would be invalidated for what would be seen as a faulty technique. However, this hypothesis was not even contemplated in either of Strachey's reports. His recommendation in these documents does not say anything about invalidating Klein's manner of working with patients (p 659, p 678). Clearly, the whole issue of technique was very relevant for the development of the Controversial Discussions, since Strachey's draft report prompted Glover to resign from the British Society, and Anna Freud from the Training Committee. Both, with some other participants, did not attend in person any further round of discussions. However, where the issue of internal demarcation is concerned, it continued to take place through biology before and after the debates recorded in the third section of *The Freud-Klein Controversies 1941-1945*, as discussed above. In this respect, I see the question of technique as neutral in relation to the question of demarcation: the debate on technique was important but remained as an isolated pocket. In addition to my own readings, another way to indicate this isolation and lack of influence on demarcation is for example to notice how the key clinical term 'transference' remained pretty much restricted to the ninety pages of section three, and failed to become an important actor, unlike biology and associated terms, throughout the rest of the debate. The word 'transference' appears 171 times in *The Freud-Klein Controversies 1941-1945;* 24 of these appear in the preliminary debates in section one, and prior to Isaacs' paper presentation. It only appears 16 times in the round of discussions in section two. We then have 119 instances in section three, which was devoted to technique, but it fails to appear during section four as the debate unfolds. 'Transference' does not appear for the rest of the actual debate on the validation of Klein's views (the final 12 instances appearing in the final section of the book, after the round of discussions of Scientific Meetings). While my argument on the neutrality of technique is not merely a numerical exercise, it is a good indicator of what I have observed, and argued above.[62]

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[62] Stengers' criterion could be potentially relevant in the context of the debate at the Controversial Discussions, where technique was taken up, as long as issues related to transference, therapeutic effectiveness, and a reliable witness become central and influence the internal demarcation of Klein's claims. However, Klein makes sure to state that she relies on – and therefore follows - transference as a central part of her clinical practice. She writes for example: 'I found that with children the transference (positive or negative) is active from the beginning of analysis' (King and Steiner 1991, p 635). She also adds that 'in my experience, the transference situation permeates the whole actual life of the patient during the analysis (ibid). In this respect, Klein arguably cannot be demarcated from Freud in Stengers' terms because both shared the use of transference as an instrument for clinical practice. If anything, Klein could be accused of excessive use of transference as in Anna Freud's report (p 631), and not of insufficiency. However, considering the recommendations from Strachey's report, and technique being limited to a pocket during the debate, I conclude that no real attempt to demarcate Klein via technique was meaningfully pursued during the course of debates at the Controversies.

6. Conclusion

The aim of this chapter is to identify in *The Freud-Klein Controversies 1941-1945* the different types of associations in psychoanalytic knowledge production that could lead internal demarcation in relation to Klein's views. Using participants' interventions, I look for the array of associations that led to criteria of internal demarcation. I first identify 'claiming the truth' as one such criterion. However, while truth was invoked as unproblematic, these statements offer virtually no underpinning associations and are regarded as tautological and devoid of significance from an ANT perspective. After this bad demarcating move, the next section notes that the Controversial Discussions skirted the issue of clinical observations, with Kleinians asserting the superfluity of evaluating clinical data while Freudians insisted on clinical data as fundamental and sorely missed. This absence, which I understand as a neutral demarcating move, raises questions regarding the process of knowledge production in psychoanalysis in relation to the couch-laboratory analogy, and the absence of Collins' 'experimenter's regress' as a typical solution for scientific controversies.

In the absence of clinical data, I then show that the Controversial Discussions takes an alternative path with a criterion of internal demarcation emerging in terms of theoretical compatibility with Freud's established views, and according to coherence within a developmental perspective. Thus, the logic of demarcation behind metapsychology became ever more dependent on whether or not Klein's views on early development were coherent with (Freud's) established psychoanalytic views.

The developmental terms that came to shape the debate foregrounds a main argument of this chapter. From an ANT perspective, the internal demarcation of psychoanalytic knowledge was put forward through what I identify as a good demarcating move, and mainly couched in terms of biology. In the absence of clinical work as a potential criterion, attention was turned to a dynamic of asserting natural laws of development, and to the genetic principle that posits knowledge based on the modularity of basic mental operations. With the established factual truth of both the libidinal development and the universal transmission of the Oedipus complex on biological grounds, the antagonism in the debate becomes based on whether psychoanalytic knowledge could be expanded on similar grounds, with Klein's supporters asserting the biological credentials of her notions while her opponents denied them. This tug-of-war came to dominate the debate, with biology covertly represented by metapsychology where demarcation was concerned, and discussion of ontogenesis and phylogenesis came to overtly frame the debate. This suggests that biology emerges as a strong criterion of demarcation because it is embraced by both camps, negotiated collectively, dominating the debate from the second paper (Heimann's *Some Aspects of the Role of Introjection and Projection in Early Development)* through to the end of the Controversial Discussions.

Finally, while issues on technique were raised, displaying another array of associations, they failed to become a criterion for internal demarcation because they remained at a subjective or contingent level, and were not connected with the overall struggle to validate or invalidate Melanie Klein's claims.

In summary: 'claiming the truth' is posited as a potential criterion of demarcation but it provides no array of associations to underpin it; in their absence, clinical cases fail to become a criterion since they (and their related associations) are not invoked; technique, for all its associations, remains dissociated from the production of a fact and relatively isolated in a pocket within the Controversies; and it is biology that emerges as a consistent criterion of internal demarcation for Klein's notion of the depressive position.

The next chapter accounts for the array of associations around biology –mainly phylogenesis –as they were originally articulated in psychoanalysis, so that we can understand their role in knowledge production and the means by which they were imported into the Controversial Discussions in the first place.

Chapter 6: Underpinning the Oedipus Complex with Associations

1. Introduction

The particular use of history in ANT has been discussed in the Methodology Chapter as a way to cut through – 'not following the historical path but rather the network of associations' (Latour 1988 p 12). I use this method to expand on 'the good' demarcating move identified above, pointing to Latour's assertion in *The Pasteurization of France*: 'the sociologist of science (…) looks for what *former* scientific professions have already done to create this vast reservoir of energy' (p 19). Soon after, he adds that 'we do not have a "long durée" that would act as a cause to push or pull the Pasteurians, but we have Villermé[63] and his friends constituting, through the new profession of scientific hygiene and through the elaboration of national statistics, a link between mortality and degree of wealth' (ibid). Finally, Latour concludes: 'the social context of a science is rarely made up of a context; it is most of the time made up of a *previous* science' (ibid).

The quotes above portray the use of history in ANT in a specific situation – that is, according to the identification of a build-up in the networks of associations, throughout time, helping and informing Pasteurians' own networks in the making. In this case, Latour shows how previous actors established a link between health and wealth, which later helped to support Pasteur's case for microbes. So, instead of 'context', Latour points to a previous chain of associations, and in particular to a link.[64] Along these lines, I discuss in this chapter 'the good' move on the basis of a previously established network, and in terms of how the link between phylogenesis and a psychoanalytic notions was first made – not from a previous 'science' but mainly through previous efforts of associations. I argue that this forged network of associations was imported, and that it vastly informed the dynamics of internal demarcation at the Controversial Discussions. The main argument of this chapter, then, is that phylogenesis was instrumental in changing Freud's Oedipus complex from a personal, uncertain notion into a black boxed and uncontroversial fact in psychoanalysis. Thus, focusing on the role of phylogenesis in knowledge production in psychoanalysis casts light on the Controversies.

2. A background to *Totem and Taboo* and the Oedipus complex

Totem and Taboo comprises four essays, originally published in instalments between 1912 and 1913. According to Freud, this four-essay text was an attempt to bridge the gap (a paraphrase of

[63] Louis-Rene Villermé, 19th century French socio-epidemiologist.
[64] This is a good example of why ANT is not a 'history', as it is concerned with associations instead of archival research.

‘translation’) between distinct fields of investigation such as social anthropology, philology, folklore and psychoanalysis (p xiii). Furthermore, Freud writes in the preface that ‘an attempt is made in this volume to deduce the original meaning of totemism from the vestiges remaining of it in childhood’ (p xiv). In addition, Gay (2006) notices that the book’s subtitle – *Several Congruences in the Mental Life of Savages and Neurotics* – is both revealing (p 328) and relevant for the development of Freud’s argument. Both the subtitle and preface of *Totem and Taboo* outlined Freud’s argument strategy, and according to Gay, ‘adopting the comparative strategy typical of his theorizing, Freud linked these unsubstantiated, quite insecure guesses [from Robert Smith’s biblical studies, and Darwin’s hypothesis of a prehistoric horde] to the animal phobias of neurotic children and then ushered the Oedipus complex, which had been hovering in the wings, to centre stage’ (2006 p 329). It is possible to read *Totem and Taboo* from an ANT perspective precisely because of the way Freud articulates connections between previously unrelated actors.

The first essay of *Totem and Taboo* discusses ‘primitive man’s horror of incest in relation to accounts of totemic societies and their dynamics of kinship and sexual intercourse regulation’ (pp 1-17). Freud centred his account on totemic sexual regulation between a son and his mother-in-law or mother, while concluding that ‘all that I have been able to add to our understanding of [this regulation] is to emphasise the fact that it is essentially an *infantile* feature and that it reveals a striking agreement with the mental life of neurotic patients’ (p 17). And, ‘we have arrived at the point of regarding a child’s relation to his parents, dominated as it is by incestuous longings, as the nuclear complex of neuroses’ (Ibid.). The second essay focuses on the Polynesian word ‘taboo’ (p 18) and its associated idea of prohibition in relation to neurotics’ obsessive behaviour (p 26). While entertaining the existence of an ‘organised’ inherited psychical endowment of innate ideas (p 31), Freud also attempts to create a network of associations among the notions of ambivalence, taboo, and obsession with amnesia, prohibition, and repression. In other words, from an ANT perspective, Freud sought to establish a link between ‘savages’ and neurotics, while also making associations between a prehistoric past and clinical work in the present. Along similar lines, the third essay seeks to establish the convergence between a primitive belief in animism and magical thinking with the development of sexuality in a child’s mind and obsessional neuroses (pp 75-99).

Many regard the fourth essay as the most important. In his editorial text to *Totem and Taboo,* James Strachey notes that this ‘book remained a favourite all through [Freud’s] life and he constantly recurred to it’ (p xi), with Freud having a ‘very high opinion of this last essay both as regards its content and its form’ (Ibid.). Similarly, Jones (1956) notes that this essay was as esteemed by Freud as was his last chapter of *The Interpretation of Dreams* (1900) and his essay on *The Unconscious*

(1915b). It is in the fourth essay that Freud finally articulates a link between the fundamental taboos of totemism, a sense of guilt, the assumption of a collective mind, and the Oedipus complex.

Although the stated aim of *Totem and Taboo* was to bridge the gap between social anthropology, philology, folklore and psychoanalysis, the underlying mechanism uniting these fields is arguably that of intergenerational transmission – or phylogenesis – based on acquired characteristics. So while phylogenesis is not overtly named in *Totem and Taboo*, the more Freud creates links associating totemism and Polynesian taboo with childhood, neurotics, and the sense of guilt as oedipal threads, the more he aligns these heterogeneous actors according to an emphasis on phylogenesis and its role in psychoanalysis. Freud's notion of phylogenesis finally emerges out of *Totem and Taboo* when he argues that 'without the assumption of a collective mind, which makes it possible to neglect the interruptions of mental acts caused by the extinction of the individual, social psychology in general cannot exist. Unless psychical processes were continued from one generation to another, if each generation were obliged to acquire its attitude to life anew, there would be no progress in this field and next to no development' (p 158). In other words, *Totem and Taboo* attempted to articulate a 'collective mind' and intergenerational transmission throughout the text and with the support of all established connections in there.

Freud first uses the term 'phylogenesis' in his *Schreber* case history (1911a p 82) and *The Claims of Psycho-Analysis to Scientific Interest* (1913 p 184). This coincides with the timing of *Totem and Taboo,* with Freud originally borrowing the term from Abraham, Spielrein and Jung (Ibid.), who discussed this subject as early as 1909 (Makari 2008 p 313). Furthermore, there is a certain consolidation of phylogenesis in his work subsequent to *Totem and Taboo*, such as in the third preface to Freud's *Three Essays on the Theory of Sexuality* (1905) in 1914, in *Instinct and their Vicissitudes* (1915a), in his introductory lectures (1916-1917), and in his draft of a metapsychological paper on phylogenesis (1987), the latter not being published during Freud's lifetime.

While the oedipal theme intertwines with phylogeny in *Totem and Taboo,* the Oedipus complex underwent a laborious and gradual process of consolidation in Freud's work, without him ever providing a systematic account of it (Laplanche and Pontalis 1973). The theme of *Oedipus Rex* appeared early on in Freud's writings, both in his correspondence with Fliess in 1897 and in *The Interpretation of Dreams* in 1900 (Laplanche and Pontalis 1973). Yet, the theme of parricide in Freud's work was far from integrated at that stage. For example, there was no mention of Oedipus in the first edition of *Three Essays on Sexuality* in 1905 (Quinodoz 2006), appearing only in a footnote to the 1920 edition (Freud 1905 p 206). And it was only in 1910 that the term 'Oedipus complex' proper debuted in print (Perron 2005 p 1183; Quinodoz 2006), with the term 'complex' being borrowed from Jung's work in 1906 (Freud 1914 p 29). Forrester (1980) provides an in-depth account of the development of the Oedipus complex arguing that it was only during the 1908-10 period that it indeed became the nucleus of neurosis (p 95, p 232), adding that *Totem and Taboo* provided 'the first full-scale argument concerning the Oedipus complex' (p 232). So it was only during this period – during the 1910 Nuremberg congress and when *Totem and Taboo* was written – that the Oedipus complex was consolidated into 'a form in which opposition *and* identification [with the parents] were both *necessarily* involved' (p 91). That is also when it becomes associated with the notion of *Nachtraglichkeit* (Ibid.) and forms the unified core of all neuroses (p 92).

The brief summary above suggests that the fundamental development of the Oedipus complex took place concomitantly with the consolidation of the psychoanalytic movement and the publication of *Totem and Taboo*. As the unifying concept explaining neuroses, the Oedipus complex took shape relating family romance with sexuality, symptoms, neuroses, and a hierarchy of unconscious material within a triangular structure (Forrester 1980 pp 84-97). Furthermore, this complex was arguably the fact of life organising the whole of *Totem and Taboo* (Gay 2006 p 332): given his personal conviction of the universality and importance of the Oedipus complex (Freud 1905 p 226), Freud did not simply invoke it as true but worked instead on his assertion. From an ANT perspective, he made great efforts to translate the psychological consequences of an Oedipus complex into a problem converging with the early days of human history. In this respect, the Oedipus complex was not only concomitant with *Totem and Taboo*, but also a major part of it.

3. ***Totem and Taboo* and the articulation of the Oedipus complex**

This section follows the process of translation and network formation in *Totem and Taboo*, specifically the constitution of phylogenesis as a relevant actor in psychoanalysis. I argue that psychoanalytic observation of a limited pool of clinical cases, a thinking in cases (Forrester 1996) based on the subjective experience of long treatments, undergoes transformations which eventually enable psychoanalysis to assert 'facts' about the human psyche on phylogenetic grounds. In other words, this section uses an ANT framework to discuss a step in the chain of knowledge production in psychoanalysis, involving both phylogenesis and the first full, large scale formation of the Oedipus complex (Forrester 1980 p 232). This specific rationale works in tandem with ANT's assertion that the production of a fact involves rendering it devoid of subjectivity, contingency, or traces of constructions.

Latour borrows his notion of translation from the work of Michel Serres (Blok and Jensen 2012 p 37). It is originally understood as forging passages and establishing connections between heterogeneous elements (Brown 2002) which can create 'convergences and homologies by relating things that were previously different' (Callon 1980 p 211). In the case of *Totem and Taboo*, forging the very connections of previously unrelated terms meant that the past (biologically given) could be established by the present (oedipal notions, clinical material). According to the logic underpinning the work of translation, *Totem and Taboo* articulates a phylogenetic biology within psychoanalysis in relation to what was first established by subjective conviction and psychological clinical work. In this respect, Freud is very clear in *Totem and Taboo* that psychology is his starting point: only 'after we have completed our psychoanalytic work we shall have to find a point of contact with biology' (1913 p 182). Therefore, he makes it clear that the first step in knowledge production of psychoanalysis is to think in cases (as in Forrester 1996), and only then in terms of biology. It is the present that gives shape to the past.

Considering that clinical psychology sits at the beginning of knowledge production, present and past could relate 'in an extraordinarily complex, unexpected, complicated way' (Serres 1995 p 58). From this framework, Freud's attempt to link psychological clinical work with biology is not simply a way to explain the oedipal theme running from biology (past) to psychology (present); instead, it indicates how a complex array of elements in clinical work is translated into, while also introducing a notion of biology in, psychoanalysis. Freud's intention to connect his psychological theory of the Oedipus complex to phylogenetic biology is clear, for example, in his correspondence with Ferenczi:

> The work on totem is a mess. I am reading fat books without any real interest, since I know the conclusions already; my instinct tells me so. But all the material has to be ground through, and in the meantime the insights become clouded. There are many things that don't want to make sense, and yet shouldn't be forced (Brabant et al. 1993 pp 316-317).

Freud hereby admitted that he was already personally convinced of what he was supposed to achieve ('my instinct tells me so') in his research for *Totem and Taboo.* Gay (2006) notes that 'in important respects, he [Freud] had leapt before he looked' (p 324).

ANT provides a specific way to view Freud's personal convictions within psychoanalytic knowledge production*.* Latour regards scientists as fact-builders, competing in controversies to 'turn one another's claims into subjective opinion' (1987 p 83) while struggling to make their own claims objective or a fact (p 30). Put another way, the very struggle of a scientist is to transform his or her personal convictions, claims, and theories – and in this case Freud's reading of Greek Tragedy, Shakespeare, and personal analysis (Anzieu 1986) – into independent facts, stemming from reality and regarded as certain. So, even if his personal convictions anticipated his own writing, Freud's task still remained ahead: to transform a matter of opinion into a black-boxed fact, independent from his personal experience, beliefs, and authority.

Discussing taboo and emotional ambivalence in his second essay in *Totem and Taboo,* Freud writes that 'both the clinical and psychical mechanism of obsessional neurosis have become known to us through psychoanalysis' (p 29), and thereby reasserts the psychological origins of psychoanalytic knowledge, drawing on his clinical work to support his notions of repression, concealed desire, prohibition substitutes, neurosis, and obsessive acts (p 30). In other words, Freud departs from clinical observations and the challenge of turning subjective experiences into objective knowledge,[65] and now invites his reader to 'make the experiment of treating taboo as though it were of the same nature as an obsessional prohibition in one of our patients' (pp 30-31). 'As though it were' is the connective part of his statement flagging his intention to forge an association between psychological experience in clinical work and the past of humankind. It is through this simple connector that the converging work of translation takes place and a change of scale begins. The association between clinical work and human past is only reinforced when Freud also adds that 'we can reconstruct (…) the history of taboo as it follows on the model of obsessional prohibitions' (p 31). Later on he writes that 'what we shall rather endeavour to confirm, therefore, are the psychological determinants of taboo, which we have learnt to know from obsessional neurosis' (p 35).

Having articulated the convergence of previously unrelated actors in *Totem and Taboo*, Freud provides homologies between neurotic features of contemporary patients and the primeval human. He eloquently announced, for example, that 'anyone approaching the problem of taboo from the angle of psycho-analysis, that is to say, of the investigation of the unconscious portion of the individual mind, will recognize, after a moment's reflection, that these phenomena are far from unfamiliar to him' (p 26). In forging such associations, Freud drew on what he had observed first in the consulting room in searching for a convergence or homology between oedipal obsessional symptoms and taboo usages. He consequently casts light on 'the horror of incest displayed by savages' (p 17) as it 'reveals a striking agreement with the mental life of neurotic patients' (Ibid.) whose infantile mental features would form a 'nuclear complex of neuroses' (Ibid.).

[65] In ANT, objective knowledge only means the capacity to rule out accusations of being subjective in relation to certain claims. Disputes about fact are dynamics to 'turn one another's claims into subjective opinion' (1987 p 83) while struggling to make their own claims objective or a fact (p 30).

Building these convergences, Freud foregrounded other oedipal features to reinforce his case: 'the most ancient and important taboo prohibitions are the two basic laws of totemism: not to kill the totem animal and to avoid sexual intercourse with members of the totem clan of the opposite sex' (p 32). In other words, Freud adds more and more associations to strengthen his case for the Oedipus complex being established as a 'fact'. Soon after, Freud formulated the conclusion he already personally believed: 'the multiplicity of the manifestations of taboo (…) are reduced to a single unity by our thesis: the basis of taboo is a prohibited action, for performing which a strong inclination exists in the unconscious' (Ibid.). Continuing with his second essay in *Totem and Taboo* and by adding more associations to the network, Freud further enhances the connection of 'savages' of the past with the present reality of his clinical work: 'the strongest support for our effort to equate taboo prohibitions with neurotic symptoms is to be found in the taboo ceremonials themselves (…). Here, then, we have an exact counterpart of the obsessional act in the neurosis' (p 50).

Freud's translation of psychoanalytical insights into the primitive 'savage' helps him to deal with a dual challenge of thinking in cases (see Chapter 5), and in relation to knowledge production. Firstly, with the convergence brought about by translation, Freud makes the case that subjective experience in clinical work can be turned into inherent experience. Secondly, Freud argued that clinical accounts can support generalisation and certainty for his claims.

In the fourth and final essay of *Totem and Taboo*, Freud turns his attention to 'the resemblance between the relations of children and of primitive men towards animals' (p 126). Based on an extremely limited number of clinical cases (or exemplars, in Forrester's terminology), Freud establishes his understanding of totemism according to the clinical picture of animal phobia. In his own words, 'no detailed analytic examination has yet been made of children's animal phobias (…). But a few cases of phobia of this kind directed towards the large animals have proved accessible to analysis' (p 127). Therefore, establishing convergence between three clinical cases – one from the psychoanalyst Karl Abraham (Ibid.), another from Ferenczi's study of Arpad (p 130), and his own study of 'Little Hans' (1909) – Freud connected the theory of the Oedipus complex with the displacement of children's feelings 'from their father on to an animal' (p 129). Based on the presence of this feature in the sample of three clinical cases, the generalisation of the Oedipus complex is pursued through linking present with past:

> If the totem animal is the father, then the two principal ordinances of totemism, the two taboo prohibitions which constitute its core not to kill the totem and not to have sexual relations with a woman of the same totem – coincide in their content with the two

> crimes of Oedipus, who killed his father and married his mother, as well as with the two primal wishes of children (…). If this equation is anything more than a misleading trick of chance, it must enable us to make it probable that the totemic system – like little Hans' animal phobia and littler Arpad's poultry perversion – was a product of the conditions involved in the Oedipus complex (p 132).

Very importantly, the quote above signals for the first time in *Totem and Taboo* a transformation in the convergence and homology from the present (of Freud's conviction, clinical work) with the past. I identify this moment as the beginning of an inversion (as identified by Latour and discussed in Chapter 3), as part of an attempt to black box the Oedipus complex. This subtle yet ingenious inversion is there, with Freud making 'it is probable that the totemic system (…) was a product of the conditions involved in the Oedipus complex'. As mentioned previously in this study, the black box is simply the replacement in time of a complex process and open controversy by a sense of unproblematic certainty and simplicity in science. Latour goes so far as to ascertain that such temporal reversal, from controversial uncertainty to unproblematic certainty in science, 'is one of the most puzzling phenomena we encounter when following [scientist's] trails' (p 98). In the quote above, as with all the work of associations provided previously, there is the attempt to reverse the temporality of construction of the totemic system, from being dependent on the Oedipus complex into what grants certainty to this very notion. This inversion is no apparent contradiction with what has been established so far. Puzzling as it is, such a reversal is exactly Freud's attempt to black box, replacing the complexity of associations between present and past with an unproblematic certainty of Oedipus complex in every time frame.

In order to carry out the black box inversion and construct the origins of the Oedipus complex in a distant past, the work of translation continues, with *Totem and Taboo* now articulating emotional features of the festival of the totem meal (pp 142-143), Freud writes 'We need only suppose that the tumultuous mob of brothers were filled with the same contradictory feelings which we can see at work in the ambivalent father complexes of our children and of our neurotic patients' (p 143). Accordingly, the 'savages' would have hated and admired their father, rid themselves of him, felt remorse, and 'a sense of guilt made its appearance, which in this instance coincided with the remorse felt by the whole group' (Ibid.). Consequently, the primitive sons:

> Thus created out of their filial sense of guilt the two fundamental taboos of totemism, which for that very reason inevitably corresponded to the two repressed wishes of the Oedipus complex (p 143).

The quote above is also very important where knowledge production is concerned. It portrays the moment in *Totem and Taboo* when the work of translation gives way to the work of purification – the moment when the present ceases to shape the past and the past comes to inform the present. Put differently, this inversion allows the Oedipus complex to be black boxed and regarded as bearing the certainty of a fact, rather than simply being contingent (out of a limited number of clinical cases, Freud's conviction, and his personal analysis). There is now necessity and universality of a fact associated with the Oedipus complex. So, if the articulated associations[66] are strong enough, Freud can then claim that the Oedipus complex originates independently in the past and inexorably informs the present, as a fact of phylogenetic biology and human nature.

After stating his notion of the Oedipus complex in black boxed terms, Freud ends *Totem and Taboo* with more explicit reference to the inevitable inheritance of psychical dispositions throughout generations: 'the sense of guilt for an action has persisted for many thousands of years and has remained operative in generations which can have had no knowledge of that action' (p 158). Such transmission was based on his assumption of a 'collective mind', 'which makes it possible to neglect the interruptions of mental acts caused by the extinction of the individual' (Ibid.). Here, Freud hints at phylogenesis as the necessary mechanism of psychic determinism and the presence of the Oedipus complex in neurotic patients. In other words, Freud posits that the Oedipus complex must be transmitted phylogenetically, and in turn this transmission mechanism implies that the Oedipus complex should be deemed as a fact in psychoanalysis.

4. Considerations for the Controversial Discussions

With *Totem and Taboo,* I have provided an ANT account of the emergence of phylogenesis as a relevant actor in psychoanalysis. I have also displayed the array of associations, the convergence through translations, and the establishment of associations between previously unrelated actors. It is only in the end, after so much effort is put into establishing this network, that the Oedipus complex can be inverted and put forward as a phylogenetic fact. Of course, the status of the Oedipus complex as a fact does not depend solely on *Totem and Taboo,* phylogenesis, or Freud. It also relies on whether or not it will later be taken by others as black boxed, and according to other associations underpinning Freud's claims . I have chosen to focus on *Totem and Taboo* because it articulates phylogenesis in psychoanalysis while also displaying the whole arc of changes, through the process

[66] I am not claiming that all associations stem from *Totem and Taboo*. I am only identifying associations in relation to this text.

of translation, that transforms contingency to posited necessity – a fundamental question for the purposes of my research here in relation to the Controversial Discussions.

Borrowing from Latour's provocative style, it is even possible to assert, from an ANT perspective, that the biological 'past' of the Oedipus complex cannot be older than 1911, when Freud first used the term 'phylogenesis' in his *Schreber* study (1911a). The Oedipus complex would therefore precede its pre-historical past by gaining fundamental currency between 1908 and 1910 (Forrester 1980), while subsequently becoming the major ingredient of *Totem and Taboo*. Of course, I speak in terms of temporal priority according to chronological establishment of different networks of associations: first the Oedipus subject as a complex, then its relations to a totemic past, all of them eventually helping to underpin phylogenesis as producing a factual status to claims in psychoanalysis.

As discussed in the Literature Review, Bergmann writes that during the classical-Freudian period, 'psychoanalysis had discovered a truth many people find difficult to accept, namely that sexuality does not begin in adolescence, but during infancy, and that infantile sexuality culminates in the Oedipus complex' (2004 p 4). For Bergmann, then, dissidence was demarcated and dismissed for its resistance to psychoanalytic knowledge, equated with a 'pathological phenomenon based on resistance or relapse from a painful truth' (p 88). In other words, internal demarcation in psychoanalysis was based on the acceptance or denial of psychoanalytic 'truth', such as the centrality of the Oedipus complex and sexuality (Makari 2008 pp 290-2). Considering that *Totem and Taboo* was written and published during this same classical-Freudian period, the articulation of the Oedipus complex with phylogenesis followed, and helped to consolidate, a path for internal demarcation. So the emergence of the Oedipus complex out of *Totem and Taboo* as universal and phylogenetically transmitted through generations offered a basis to enforce internal demarcation, equating production of facts with psychoanalytic truth from an ANT perspective. Ultimately, then, with this equation, psychoanalysts are able to claim the Oedipus complex as a matter of fact,[67] while also paving the way for other psychoanalysts to make claims on similar grounds, using a similar criterion of internal demarcation.

Bergman's classical-Freudian period was a moment of consolidation of the psychoanalytic movement (Makari 2008) with the departure of figures such as Adler and Jung (Bergmann 2004; Makari 2008), and the establishment of unity around the 'painful truth' of psychoanalysis. With Freud's death two years before the onset of the Controversies, Klein could no longer rely on his

[67] See glossary.

opinion and authority to validate the credentials of her findings. However, she could still invoke Freud by employing similar processes of knowledge production in psychoanalysis, which I think goes some way towards explaining the negotiation based on biological grounds for internal demarcation during the Controversies. Remembering that Latour said 'a science is rarely made up of a context; it is most of the time made up of a *previous* science' (Latour 1988 p 19), I am positing that it is the dynamics of internal demarcation in biological terms at the Controversies that is made up of previous articulations, from those which were instrumental in the black boxing of the Oedipus complex. Since it is so evocative, instructive, while opening itself to different and pertinent issues, Susan Isaacs' previously used quote is once more referred to by me:

> We know that every child passes through the Oedipus complex. We find it determining the character and history and neurosis of every adult patient. (…) Exactly the same applies to the 'depressive position' and early psychotic phantasies (…). On Melanie Klein's view, every infant and young child experiences this phase of development (King and Steiner 1991 pp 458-59).

To paraphrase this quote in ANT terms: we know that every child passes through the Oedipus complex (because it has been black boxed as psychoanalytic 'truth' and as a universal fact). We should believe that exactly the same applies to the (process of demarcation and production of psychoanalytic knowledge concerning the) '"depressive position" and early psychotic phantasies'. In Melanie Klein's view (these are not a matter of her personal opinion but rather of universal 'facts'), every infant and young child experiences this (phylogenetically and ontogenetically certain 'fact' which is displayed as a) phase of development.

Klein arguably imports the same structure of associations as a rationale to produce facts in psychoanalysis when she states, for example, that 'the relation of the infant to his mother ['s breast] is based on phylogenetic inheritance and is ontogenetically the most fundamental of all human patterns' (p 757), as does Isaacs when she alludes to phylogenesis by asserting that 'every child passes through the Oedipus complex'. From an ANT perspective, these are but two examples echoing *Totem and Taboo*'s structure of psychic features, turned to psychoanalytic facts, as well as the logic of internal demarcation based on a consensus around a psychoanalytic truth.

5. Conclusion

In this chapter I have provided an ANT account of the process of translations around the Oedipus complex and its articulation with phylogenesis through the extensive associations in *Totem and*

Taboo. In Section 3, I identify personal conviction, thinking in cases, and biology as sequences of knowledge production. Here, the challenge to produce a fact is connected with the translation of a subjective experience in clinical work into inherent experience. Considering this process, I have also provided an account of how Freud articulates a chain of associations through *Totem and Taboo* comprising actors such as Freud, clinical accounts, texts, letters, patients, totemic rules, anthropology of 'savages', incest rules, animal phobias, present, past, 'collective' mind, psychic development, phylogenesis, and so on. I have read these articulations as playing an important part to producing an inversion where a biological past comes to explain an inherent necessity of the Oedipus complex in the present. The outcome of this is a black box, where the complex array of associations undergoes a temporal reversal (with certainty of the past explaining the present, instead of the present informing a tentative past), and where the Oedipus complex can be simply taken as a fact through phylogenetic inheritance.

I am not interested in the historical path connecting *Totem and Taboo* with the Controversial Discussions. With the arguments provided here, I have shown instead that the 'good' demarcating move based on biological grounds imports in fundamental ways many of Freud's previous efforts of associations, according to an established rationale originally applied to the Oedipus complex, and potentially followed by the depressive position. It is in this sense that I see contiguity between the Controversies and *Totem and Taboo.*

Chapter 7: Considerations on Modern Thinking

The emergence of a criterion for internal demarcation at the Controversial Discussions, and the articulation of phylogenesis with the Oedipus complex in psychoanalysis, are both couched in a constructivist perspective, where knowledge production is a key feature, and where facts are the outcome of a process comprising several actors and, eventually, a black box. The constructivist approach is part of ANT's understanding of scientific activity, as already discussed in Chapter 3, Section 5. It is from this vantage point that I have followed associations and discussed aspects of knowledge production that are relevant in the context of the Controversial Discussions.

With this chapter, I want to step away from following associations and actors for a moment. I want instead to account for what ANT is challenging and leaving behind with its brand of constructivism. That is, I am interested in debating modern thinking (Latour 1993), not simply in theoretical terms (as in Chapter 3, Section 6) but rather in terms of its presence in the Controversial Discussions. As I discuss in Chapter 2, there is a gap in the literature on psychoanalytic controversies, where the term 'science' is never fully addressed. At the same time, the research question guiding this thesis concerns how psychoanalysts internally demarcate their notions. In my view, a full engagement with this question and the term 'science' also involves the presence of modern thinking at the Controversial Discussion because, as much as I provide the presence of knowledge production at this debate, constructionism stands as an alternative and in tension with modern thinking. While I have subscribed to a constructivist approach to address the Controversies, my comments in this chapter aim to address this tension between constructivism and the realism of modern thinking, in order to make my argument throughout this thesis more meaningful.

1. Modern thinking underpinning the bad move

In *Beyond Nature and Culture* (2013), Phillipe Descola writes that 'moderns neither do what they say nor say what they do' (p 86). I think this quote encapsulates ANT's mandate: challenging the dual cosmology of Nature and Society, and replacing it with hybridity and the constant work of translations that produce a world of natures and cultures. I use the contrast between realism and constructivism, which underpins Latour's work (1993), to further consider the bad demarcating move of 'claiming the truth'.

The bad demarcating move identified in Chapter 5 was the claiming of truth on one's side. Considering Bloor's idea of necessary uncertainty at scientific controversies, appropriated by Latour, I emphasised the poverty of associations underpinning this approach in ANT terms. Behind this ineffective stance, I think there is also a shared understanding of how to negotiate order at the Controversial Discussions, which connects with the tension identified by both Latour and Descola in their challenge to modern thinking.

In *After Method* (2004), Law defines five assumptions usually implicit in claims of truth, supported by modern thinking, and in line with ANT's critical understanding of Nature. In addition to Descola's (i) *out-thereness* (the reality that is 'out there') Law also stipulates: (ii) *independence,* the idea that 'external reality is *usually independent of our actions and especially of our perceptions*' (p 24). With this assumption of independence, the world 'out there' is only waiting for correspondent representations of it. An additional assumption on reality is (iii) *anteriority,* the notion that reality has existed before, regardless of our understanding of it. With (iv) *definiteness,* Law argues that reality is seen as made up of 'a set of *definite forms or relations'* (p 24). Euro-American empirical experience assumes the possibility of recognising patterns in reality 'that are more or less specific, clear, certain, definable and decided' (Ibid.). With this assumption, a scientific quest becomes a matter of discovering the rules that relate to representable objects (p 25). A final assumption is (v) *singularity*, the common-sense notion 'that the world is shared, common, the *same everywhere'* (p 25).

Significant passages during the Controversial Discussions evidence the five assumptions on reality, for example, when Winnicott states:

> What is this scientific aim? The scientific aim is to find out more and more of the truth. (…) This search for truth is a cycle of three phases: piecemeal objective observation; construction and testing of theory based on observed facts; and imaginative reaching out in front of accredited theory towards the invention of new instruments of precision, these opening up new fields for objective observation (King and Steiner 1991 p 87).

It can be argued that a scientific truth seen as 'a piecemeal objective observation' assumes that reality is 'out there' and can be reached with 'new instruments of precision'. Also, the reliance on 'observed facts' may imply *independence* of a *singular* truth that is 'out there' and can be observed empirically. An 'instrument of precision' gives the sense that this truth existed previous to its discovery (*anteriority*) and only needs new 'inventions' in order to be accessed. The idea of 'finding out more and more of a truth' also seems to convey a belief in *definiteness*, as if an observed fact had a clear identity and could be added to old discoveries. The assumption that a clear truth exists brings about the possibility of establishing definite forms of stable relations in the unconscious. Winnicott's confidence in a generalised truth ('the truth') based on scientific terms also conveys the sense that events in psychic reality are part of a *singular* and common reality, capable of being shared by others. Klein's unlimited support of Winnicott's statement towards an 'unrestricted search for progress in our scientific work' (p 90) reinforces Winnicott's position on these terms.

It is not hard to find other significant examples pointing to the pervasive presence of a shared modern understanding throughout the Controversial Discussions. For example, Melanie Klein's statements regarding the univocity of truth (and reality) throughout the Controversial Discussions provide evidence of her commitment to the assumptions of *singularity, anteriority* and *definiteness*:

> I should like to say that as long as this Society has any claim to be called a scientific Society there must be disagreement (…). A scientific Society is a group of people who are seeking to discover new truth (King and Steiner 1991 p 204).

> I began to understand the origins and contents of depression and of the immense range of human feelings, of the strength of love and hate, sorrow and hope and with it the realization of a very rich inner world…But it is an overwhelmingly difficult task to describe this knowledge to others who cannot see it (p 232).

> I am not afraid of fighting against anybody, but I *really don't like* fighting. *What I wish* to do is to let quietly others participate in something I know to be true, important, and helpful, to let them share in it and to teach them if they are willing to learn (p 232).

> Many of the facts I have presented in this paper have been recognised by a number of observers and therefore, in the long run, all of us should be able to check them. Closer scrutiny, more attention to details and, most of all, the correlation of observations together with some degree of inference drawn from our knowledge of the unconscious of somewhat older children should enable us to clarify our minds on the emotional life of the infants (p 788).

> I am open to the argument that guilt is there from the beginning, and that a particular constellation of feelings I described is only a further stage in the evolution of feelings of guilt (p 827).

These examples convey a sense of *singularity* in terms of a generalised truth that should be shared by others, as a single knowledge that others cannot see and yet already exists. The idea of *singularity* is also clearly present, for example, in relation to the (pre-)existence of guilt from the beginning; the notion of *definiteness* is also present with the use of emotions such as guilt, love, hate, sorrow and hope. Furthermore, throughout the Controversial Discussions, there is a great number of references to an existing reality underpinning objective knowledge in psychoanalysis. Psychoanalysts' use of terms such as 'facts', 'objectivity', and 'confirmation of hypothesis' indicates the assumption of a reality as 'out there', anterior, definite, independent, and singular.

Susan Isaacs, in her presentation of unconscious phantasy, writes that 'it is the facts which have compelled this extension [of the knowledge of early mental life] (p 271). She goes on to say that 'part of the evidence which I am bringing forward for my views consists in showing the relationships between facts' (Ibid.). Where 'facts' in psychoanalysis are concerned, she especially draws attention to: 'a fundamental fact of general development (…) namely, the fact of *genetic continuity* (…). Probably no psychoanalyst would doubt the fact of genetic continuity, but this fact is not a mere abstract principle. It is a concrete instrument of knowledge, enabling us to trace threads of development backwards and forwards' (p 285). Isaacs' use of the term 'fact' in this paragraph confirms reality as singular and universal ('no psychoanalyst would doubt the fact', 'showing the relationship between facts'), as well as independent and pre-existent (it is the facts which have compelled analysts). Furthermore, Susan Isaacs was concerned with emotional factors hampering scientific investigation and knowledge:

> If our aim, the justification of our existence as a society, be the cultivation and furtherance of psychoanalytic science, and the mutual support of members in all endeavours to acquire and disseminate psychoanalytic knowledge, then it seems clear that our rules and constitution, the framework of our collaboration, should be such as to give the maximum safeguard against personal feelings (King and Steiner 1991 p 59).

Isaacs arguably subscribed to a view of scientific knowledge as cumulative, shared, common, and the same everywhere: 'the cultivation and furtherance of psychoanalytic science (…) in all endeavours to acquire and disseminate psychoanalytic knowledge'. Thus, Isaacs strongly suggests that knowledge is universal, since it is supposed to be shared. Once more, the single factor hampering this knowledge was 'personal feelings', which illustrates Latour's views on moderns as splitting Nature (innate knowledge (p 39)) from culture or subject perspective (e.g. Isaacs' personal feeling, Strachey's religious behaviour (p 33), Friedlander's personal considerations (p 75) or Brierley's creed (p 209)).[68] Bringing this back to Law's framework, then, knowledge stemming from reality is considered

[68] ANT's solution is of course to question this division as a given, and to posit instead hybridity that is eventually purified.

anterior and should ontologically exist regardless of personal understandings of it. Only from this perspective, then, would it be possible to claim the 'truth'.

Other psychoanalysts also subscribed to a realist view on scientific knowledge. Walter Schmideberg, for example, insisted that psychoanalytic knowledge is meant 'to further Freudian psychoanalysis' (p 40). Winnicott argued that 'the scientific aim is to find out more and more of the truth' (p 87) and Klein remarked that psychoanalysis 'endeavours towards an unrestricted search for progress in our scientific work' (p 90). Low added that the psychoanalytic endeavour should pursue a scientific attitude 'which I take to mean as much objectivity as possible (…) in scientific work' (p 46), while emulating Freud's ultimate 'devotion to the truth, the whole truth, and nothing but the truth' (p 49). Low's commitment to psychoanalytic knowledge as singular ('the truth') was backed up by Anna Freud's comment that 'two theories could not co-exist. If two points of view do not coincide the first [question] is to find out which is more accurate' (p 217) – or closer to reality. These different contributions to the debate reinforced the view of a shared understanding of 'facts' according to singular truths. Glover shared this view, saying that the truth is either complemented by new truths or else there should be falsification of one or the other (p 216).

Within a modern cosmology, hypotheses are to be verified against truths stemming from a reality 'out there', independent from and anterior to humans. Accordingly, Brierley posited the scientific credentials of psychoanalysis in a similar manner to those of the natural sciences, giving her view that Freud's psychoanalysis was 'a system of hypotheses designed to explain as accurately as possible the working of the human mind' (p 209). Based on the same belief in the emergence of theories out of an accumulation of observations, then, Isaacs asserts in her exposition on unconscious phantasies that 'we have a full confirmation of Melanie Klein's hypothesis as to [the infant's] early wishes and phantasies towards his mother' (p 304) which is also 'fully confirmed by the analysis (…) [of young infants] carried out by Mrs Klein and others as well as by the observational data' (p 324). The opposite camp also subscribed to these principles of observation in reality and in hypothesis testing, as Hoffer states that 'the libido theory too is a biological hypothesis but unlike the death instinct it lends itself comparatively easily to correlation with psychological facts' (p 550).

The above examples were taken from participants in the Kleinian, Freudian, and non-aligned camps, indicating how pervasive the modern cosmology was throughout the whole of the Controversial Discussions. This conveys a unity of systematic views of psychoanalytic so-called 'facts' according to a modern perspective. The discussion reliant on the term 'science' at the Controversies was couched in modern terms to an important extent, with the firm belief in a reality 'out there', independent of human distortion, and ontologically existing according to Descola's account of Nature described in

Chapter 3. Participants in the Controversial Discussions took this common-sense understanding (Law 2004) as unproblematic. As a result, reality is taken for granted as a departing point, and knowledge is understood as progressively accumulated and furthered according to a frontier of knowledge, with 'a suggested universality of their content' (Descola 2013 p 121). However, as much as realism was present at the Controversial Discussions, the struggle to produce knowledge also pervaded the debate. In order to account for the term 'science' more fully, I have provided elements here that convey the tension between efforts to produce knowledge, through all the work of associations used at the Controversies, and the idea that truth is out-there, simply waiting to be reached. Both sides of this tension can be identified at the Controversial Discussions, and my approach to it acknowledges, but leaves behind, the presence of modern thinking at the debate.

2. The 'good' move towards biology

Descola's provocative sentence, that 'moderns neither do what they say nor say what they do' (2013 p 86), contrasts the existence of two sides, which can remain unacknowledged to one another in their tense duality – the world of purified facts or black boxes, and the hybrid world of associations – unless they are brought together (Latour 1993). One side of this tension concerns 'the bad' move and modern thinking in terms of truth assertion and 'reality'. The other side concerns what I have called 'the good' move and the use of biology at the Controversial Discussions. Taken together, they stand for the co-existence of two ways to consider the presence of 'science', and related terms, at the Controversial Discussions: what moderns say (to assert truth), and what they do (to produce knowledge through articulation of actors and associations).

This co-existence of a supposed duality and its actual connection is addressed in theoretical terms in *We Have Never Been Modern* (1993). With a more philosophical tone, Latour points to a dichotomy that distinguishes purification from associations (p 11). The co-existence of these sides are also be represented by Janus, the two-faced Roman god representing transition and duality, constantly invoked in *Science in Action* (1987). In this context, I regard 'the good' and 'the bad' move together as they co-exist at the Controversial Discussions. First of all, I do not speak of modern thinking to berate Klein, her followers, or any of the participants at the debate. It is hardly a novelty to associate realism with psychoanalysis in the first decades of the 20th century. However, I still think it is important to account for modern thinking, in order to account for its counterpart, the process of knowledge production, with all processes of associations that take place at the Controversial Discussions in an agonistic fashion. Instead of ignoring the presence of modern thinking and the term ‘science’ altogether, as the current literature on psychoanalysis does (Chapter 2), my approach to the Controversies has been to consider the presence of ‘science’ according to a sociology of associations that not only responds to the presence of ‘science’ but also stands in tension with it.

My second point is to emphasise that ANT does not take a normative stance on sciences.[69] With a focus on knowledge production, Latour’s investigations of scientific activity start in a quasi-anthropological fashion, in order to follow those who call themselves scientists (e.g. Latour 1987) and their struggle to produce facts. As already discussed in Chapter 3, this approach creates an important division between being descriptive and being normative. At the same time, with the absence of a normative criterion defining what science is, the ANT framework does not allow me to make any assertion on what is scientific and what is non-scientific, or even scientism. Therefore, I make no judgement on whether psychoanalysis is a form of scientism or not. Suspending judgement, I also do not regard modern beliefs as leading to naïve realism in psychoanalysis (since being ‘naïve’ refers to the normative status of non-naïve realism). In this context, being modern only refers to realism, as discussed above, which is often shared in different strands of scientific activity. In this respect, I grant psychoanalysts the right to believe in realism, as I would grant it to a physicist, a biologist, or a chemist. And I try to challenge realism and offer an alternative constructivist perspective in psychoanalysis, much as ANT does in relation to physics, bacteriology, micro-biology, and so on. In other words, psychoanalysts may believe in realism, now and then, as much as other scientists may. Yet, at the same time, such a belief should not stop one from investigating that, regardless of what they say or believe, scientists and psychoanalysts alike are concomitantly doing a great deal of work of associations, in efforts to produce truth.

3. Conclusion

In this chapter, I have delved into the idea of asserting pre-existent truth as one way of providing internal demarcation. I relate participants’ attempt to provide internal demarcation in connection with a modern understanding of reality. From today’s perspective, it is very tempting to equate the use of the term ‘science’ with scientism, especially considering the broadly negative response to the scientific status of psychoanalysis. This is not the case in this thesis, since ANT includes no criteria of scientific demarcation. This lack of criteria may be regarded as a limitation of ANT. However, at the

[69] To the best of my knowledge, ANT does not provide any normative stance to demarcate and define science. This point was already discussed in Chapter 3, Section 3, including notes 31 and 32. Also, the only moment in his work where Latour speaks of this type of demarcation (that is normative demarcation and not internal demarcation) is in Latour (2004a). There, Latour borrows concepts from sources such as Stengers (1997) and Després (1996) to talk about scientific normativity, instead of relying on the notions developed by him, throughout his work.

same time, it helps to clarify that I do not attribute a 'scientistic' attitude to Klein. I cannot judge Freud in terms of 'scientificism' or even of 'biologism'. I make no claims to a scientific status for psychoanalysis in this study.

I have paused to delve into modern thinking to present the tension between constructivism and realism, to account for 'what psychoanalysts say (think in modern terms), and what they do (work with associations)', to paraphrase Descola. This tension is embodied by the two criteria of internal demarcation presented at the Controversies: the 'bad' move of 'claiming the truth', and the 'good' move invoking biology and its associations. Acknowledging both also accounts for the presence of the term 'science' more fully at the debate. In the next chapter, I now return to following associations and their implications for the dynamics of demarcation at the Controversial Discussions.

Chapter 8: Towards the Ending of the Controversies

Among varied reactions, the Controversial Discussions elicits specific reactions from its readers. On the one hand, a fascination for what is discussed, oblivious to dangerous bombs crashing down on the surrounding area. On the other hand, a profound sense of futility given the endless discussions likened to Byzantine debates on the sex of angels. To these reactions, it is possible to add a third: a psychoanalytic wish to understand a series of dissensions (Bergmann 2004), and the profound dissatisfaction with how differences and debates took place in the field (Bernardi 2002; Widlocher 2008; Barros 2013). This chapter takes up this perspective, repositioning the Controversies within an ANT perspective.

1. The Oedipus complex as an obligatory passage point

The Controversies took place some thirty years after the publication of *Totem and Taboo* (1912-1913). Having achieved consolidation in the psychoanalytic movement, Freud's notion of the Oedipus complex, and the very idea of psychoanalytic knowledge, were now firmly established in Britain. Freud was no longer an isolated figure within a Viennese community, and the psychoanalytic movement was no longer threatened by powerful rivals or reliant on one single living person. Furthermore, the consolidation of psychoanalysis only increased with its official acceptance by medical bodies in England as a legitimate practice (King and Steiner 1991) and with the institutional aim of preserving Freud's theories at the heart of the International Psychoanalytic Association's (IPA) statutes (King and Steiner 1991). In other words, these are all indications that an extensive network of actors took shape, supporting Freud's notions, and sustaining the Oedipus complex as a successful black box about which 'no more has to be said' (Latour 1987 p 23).

While Freud's main positions were largely unquestioned at the Controversial Discussions, taken instead as the black boxed starting point, Klein's were heatedly challenged. Their different standings can be depicted, for example, with the fundamental question in Brierley's memorandum, which set the programme of debate: 'is a theory of mental development expressed mainly in terms of the vicissitudes of infantile object-relationship [(i.e., *Klein's formulations*)] compatible or incompatible (…) with a theory in terms of instinct vicissitude [(i.e., *Freud's formulations*)]? (King and Steiner 1991 p 212). Furthermore, Brierley specified her question in terms of the views 'on the development of the psyche from infancy to the close of the Oedipus phase' (Ibid.). Given this clear emphasis on a developmental perspective, and the focus on Klein's work, the whole debate was arguably structured to determine what happens before the establishment of the Oedipus complex. The factual status of Freud's Oedipus complex, as stemming from biology, arguably set the benchmark for Melanie Klein's work in relation to pre-oedipal stages.

Payne, for example, in one of the initial scientific discussions, regarded the Oedipus complex 'as the most important psychological event in the attainment of mental health because it implied a successful development to genital maturity' (p 336). Reinforcing the universality of the complex, Balint reminded his colleagues that 'the Oedipus complex is the nuclear complex of every neurosis' (p 347). Anna Freud added that the Oedipus complex is observable from infants' third year (p 423), implying that it is a fact open to observation and verification in reality. Klein aspired to a status for her notions on equivalent terms with what she saw as Freud's discovery of the Oedipus complex (p 788). All these examples reinforce the view that the Oedipus complex was an established truth by the time of the Controversial Discussions.

Yet, the Oedipus complex is also an obligatory passage point – a phrase coined by Latour (1987).[70] For him, an obligatory passage point is a fundamental black box between two systems of alliances, concentrating one of the largest number of associations (p 139), tying mostly all actors (p 156), and standing 'between anyone's goal and the fulfilment of this goal' (p 152). Latour's quote above suggests that it is not only a fact in biological terms; the Oedipus complex also underpins the link between any psychoanalyst's goal of therapeutic success and the fulfilment of this goal by addressing neurosis. It is also what fundamentally ties every patient, clinical practice, technique, psychoanalysts, biology, and so on, in a vast array of actors, arguably making up the largest network of associations in psychoanalysis. Thus the Oedipus complex is black boxed as necessary and fundamental, but is also an obligatory passage point to the 'cure' of neuroses. It is in relation to this status as a fundamental black box that Klein's notion of the depressive position and related articulations were measured.

2. Two similar networks of articulations, two different statuses

Karin Stephen asserted during the Controversies that 'Freud was the first to throw any real light, but our object should be to follow him not in blind faith, but in his own devotion to the truth, the whole truth, and nothing but the truth' (King and Steiner 1991 p 49). Yet, from an ANT perspective, Freud's notions cannot simply be taken as 'the truth', while Klein carries the burden of proof for her own claims. Indeed, we have seen that Freud's main claims, such as the Oedipus complex, were

[70] See glossary.

constructed in a similar way to Klein's construction, as displayed at the Controversies: they both relied on a limited sample of patients, worked based on transference, and took part in the construction of fact via black boxing and through the use of a biological notion, such as phylogenesis. If anything, then, Freud could be accused of the same sins as Klein was during the Controversies. However, the only reference to what Latour understands as the co-production of fact and fabrication (Latour 1999b) in relation to Freud comes in a fleeting passage from Isaacs. As Melanie Klein's opponents relentlessly accused her of inference –as a synonym for fabrication and distortion of reality – Isaacs responded by reminding her listeners that psychoanalysis is almost entirely built upon inferred knowledge, suggesting, without naming him, that Freud's posited facts were as much constructed as Klein's:

> Dr Friedlander [p 406] refers to the fact that Mrs Klein's views as to mental life in the first year is 'inferred knowledge' as of course it is. In this passage, Dr Friedlander, like some other speakers, questions the validity of inferred conclusions. Actually, the technique of psychoanalysis itself is based almost entirely upon inferred knowledge (King and Steiner 1991 p 444).

With this response, Isaacs opened the possibility for all to open the black boxes in psychoanalysis in broader terms, including the Oedipus complex. But her comment was ignored and the debate continued to focus on Klein's claims instead. And so accusations were continuously made against Klein, while virtually nothing was said about Freud. An ANT perspective on the Controversial Discussions suggests that both Freud and Klein could be similarly accused of the sins of construction and enmeshing personal convictions with facts. What separated Freud from Klein was that his notions had already been black boxed by that time, whereas Klein's had not; this does not mean that Freud's were true and Klein's were false, at least not from an ANT perspective, but rather that the status of objective truth had already been conferred on the former but not on the latter. What would differentiate them is a successful inversion in time, in ANT terms, and brought about by black boxed status.

3. Agonistic dispute

The previous Chapters have established that biology was the major criterion for internal demarcation at the Controversies, and in the Methodology Chapter I indicate that ANT regards controversies as an agonistic struggle. So, while the aim of the Controversial Discussions was to a black box truth in biological terms, the means to this goal played out in terms of strengthening or weakening the associations underpinning the claims of the other party. From this perspective, scientific controversy is arguably a collective search for order via enactment of winning (or losing) credibility around the construction of facts, in contentions where many strategies can be invoked (Zammito 2004 p 152-3). These situations are called trials of strength in ANT (Latour 1987 p 53). Depending on one's capacity to address these challenges, one's position is strengthened or weakened, moving closer to or farther away from producing a black boxed truth. I focus now on the ways in which Klein and her supporters relied on a chain of associated articulations, in order to give support to their claim on the depressive position as a new obligatory passage point in psychoanalysis, according to a process of knowledge production

Opening the series of Scientific Meetings with her paper on phantasy, Susan Isaacs writes that 'the relationships which we have come to discern (…) have led many of us to extend the connotation of the term "phantasy" in the sense which I am now going to develop. It is the facts which have compelled this extension' (King and Steiner 1991 p 271). With these opening remarks, Isaacs immediately embraces a stance of truth in support of Klein's notion of phantasy, emerging from 'compelling facts' that speak for themselves. On these same grounds, Isaacs remarks that unconscious phantasy and related notions stem from new facts (p 266), so that such notions are a necessary consequence of observed facts (p 268) or mental facts (p 269), with their 'own laws and characteristics' (p 269), compelling Klein to pursue an extension of psychoanalysis. In order to back up this assertion, Isaacs draws on baby observation studies. She writes that 'unconscious phantasies are the primary content of all mental processes' (p 276), thereby clearly alluding to the universality of a reality 'out there'. At the same time, she notices that 'all the infants studied' (p 301) in Shirley (1933) were 'facts' conforming to Bühler's studies (1930), with the implied understanding that different observational studies of babies would converge with Klein's views. Isaacs considers several works on baby observation to be 'objective studies' providing evidence of sensations and feelings forming the basis of persecutory fears and phantasies of an attacking, bad mother (King and Steiner 1991 p 302). She praises these sources of evidence, suggesting that Klein's notions are true to the extent that they share the same truth previously confirmed by 'objective observations' made in relation to studies of babies. Here, I think the idea of 'objectivity' could gain in strength with the association established between phantasy and other infant studies. In this sense, Isaacs is not simply stating a truth. From an ANT perspective, she fosters a black box by displaying the chain of associations behind Klein's claims and borrows credibility from other studies when she writes, for example, that 'we have to give far more experimental weight to the felt hostility of the external world over a considerable period in early development, than we had realized' (Ibid.). Isaacs then draws on the same sources to conclude categorically that 'in these observations as to the infant's active concern with his mother's body – both living and aggressive – we have full confirmation of Melanie Klein's hypothesis as to his early wishes and phantasies towards his mother' (p 304). In other words, Isaacs attempts to align other actors in the dispute with Klein's views based on baby observation because 'the study of the behaviour of the infant and young child (…) is an independent science with its own highly developed technique, procedures and safeguards (…) revealing detailed facts' (p 450). Similarly, Heimann's paper presents an abridged version of Isaacs' association between infant observation and phantasy: 'observation of the baby shows quite directly how he gets to know the outside world' (p 506). Others follow suit, like Jones when he states in the first Scientific Meeting that phantasies have been amply confirmed 'by the observational data to which Mrs Isaacs has often drawn our attention' (p 324). Or when Klein highlights the facts supporting her views, and makes reference to direct observation in proving her notion of phantasy and its contents (p 762).

From an ANT perspective, the comments above are not a matter of opinion, perspective, or discourse. These textual and oral statements are part of a dispute, revealing an array of associations. In the case above, I have focused on unconscious phantasy, infant observation, developed technique, other texts, children, babies, reports, and so on, that are invoked to strengthen Klein's assertions as ontogenetic and phylogenetic facts. At the same time, connecting unconscious phantasy with infant observation strengthens Klein's position by connecting her claims to other black boxes (e.g. 'all the infants studied'), and frustrating the dissenter's job to question these associations. Paraphrasing Latour (1987 p 24): if you start believing in these claims, then the belief in the depressive position (especially as an obligatory passage point) is reinforced; the whole is taken as a package and goes where it leads (in Klein's research programme to produce a fact).

The same stance of supporting one's claim towards objectivity and certainty was reinforced by creating other associations with the depressive position, such as biology, as when Heimann makes the case for the early existence of the mechanisms of introjection and projection in the mind: 'the most fundamental vital processes of any living organism consist of intake and discharge' (King and Steiner 1991 p 507). Expanding from this biological rule to what she sees generally as 'the laws of life' (Ibid.), Heimann states that 'the mind, also a living organism, is no exception to this rule: it achieves adaptation and progress by employing throughout its existence the fundamental and basic processes of introjection and projection' (Ibid.). Therefore, connecting introjection and projection with what she sees as laws of life, Heimann asserts that 'it is their derivation from inherent biological facts that leads us to attribute such essential effects and functions to them' (p 507). In other words, she reinforces the certainty of Klein's assertions (on introjection and projection) in tandem with a biological ‘truth’. This line of reasoning leads her to conclude that ‘I may just point to the invariable equation between breast and penis, penis and child, child and faeces, and the universal oral theory of sexual intercourse and the anal theory of birth. We know how important these facts are; one of the reasons for their existence is the universal operation of introjection and projection’ (p 508). Heimann goes as far as explicitly praising ‘Mother Nature’: ‘the patterns Nature uses seem to be few but she is inexhaustible in their variation (p 507)’. Klein also echoed this strategy of associations underpinning her claims, saying, for example, that ‘I have on several occasions expressed the views that the relation of the infant to his mother is based on phylogenetic inheritance and is ontogenetically the most fundamental of all human patterns’ (p 757).

In addition to biology, unconscious phantasy, and infant observation, adult analysis was posited as confirming ‘the various facts and hypotheses regarding early development, first formulated in the analysis of young children (see, for instance, papers by Brierley, Glover, Heimann, Isaacs, Jones, Payne, Rickman, Searl, Schmideberg, Scott, Sharpe, Sheehan-Dare, Winnicott, etc.)’ (p 298). So clinical material from psychoanalysts’ work with children was also used as another association to strengthen Klein’s claims, even though there was no further discussion of it during the Controversial Discussions (for example p 283, p 299, p 445, p 514). Within this context, clinical cases were but one element comprising the network of associations in the agonistic dispute at the Controversies.

Freud’s texts were also used as another type of association to support Klein’s findings. In fact, his work was recurrently cited throughout the Controversies, and used as a reference on several occasions for a great range of topics. References were made to Freud’s work, for example, in relation to methodology in psychoanalysis (p 266), metapsychology (p 269, p 505), the existence of oral impulses (p 281, p 509, p 516), use of biology (p 504), animistic thinking (p 524), anxiety (p 758), and so on. Steiner (2000b) notes Klein’s supporters focused on Freud’s later works, such as *Negation* (1925a), *Beyond the Pleasure Principle* (1920), *The Ego and the Id* (1923), and *Inhibitions, Symptoms and Anxiety* (1926). From an ANT point of view, then, Freud becomes but another source of associations for strengthening claims of truth as discussed above. During the Controversies, Klein’s work was often positioned as either confirming Freud’s initial views on the prospects of child analysis, or as a further development of them. For example, Isaacs comments that there were some areas ‘not fully explored by Freud himself (…) [which] have been taken up and made use of by Melanie Klein’ (King and Steiner 1991 p 266), and that ‘we are entitled to claim Freud’s concept of primary introjection as a support for our assumption of activity (…) in the earliest phase of life’ (p 279). Furthermore, ‘I considered Freud’s postulates of “hallucinatory wish-fulfilment” (…) and showed how many of his comments (…) *imply* phantasies’ (p 314), and ‘Freud greatly expanded and revised his metapsychology (…) [and] Melanie Klein's work is more closely related to these later developments of Freud's views' (p 454). Heimann adds that 'Freud constantly emphasizes the relation of introjection to the loss of the object' (p 509). The constant articulation of Klein's notions in relation to Freud's work invokes a logic along the lines of 'Freud said so, and I expand or develop what he said; since what Freud said is true, therefore what I say should also be regarded as true'. This validation through seeking convergence with Freudian texts also had a performative effect (Austin 1962) since it functioned as a warning that 'we do the same as Freud or in agreement with what was initially discovered by Freud'. Therefore, connecting Klein's views with Freud's constitutes another type of convergence through associations, with the intention of validating Klein's views. Here, the objective is to present her notions as stemming from a supposed credible psychoanalyst, like Freud, who searches for the 'facts' of early mental development. Forging this type of association, Isaacs observes: 'We know that every child passes through the Oedipus complex' (King and Steiner 1991 p 458), and immediately adds: 'Exactly the same applies to the "depressive position" and early psychotic phantasies' (Ibid.). In other words, Klein's notions should be regarded in tandem with the black boxes established by Freud. Put another way, Klein should be able to claim her notion of the depressive position as an obligatory passage point, as much as is the Oedipus complex.

The extension of associations brought forward above to sustain Klein's notions form what Latour sees as successive defensive lines. That is, as an effort to establish Klein's work in a position to define the depressive position as a necessary and fundamental step in psychic development. Yet Klein's opponents drew on the same sources as her supporters did, but with the opposite aim of shaking up and weakening the chain of associations, and the supposed objectivity of her claims. As a result, those against Klein would also establish a pattern in the agonistic argumentation during the Controversial Discussions by asserting that Klein's notions should be regarded as false, contingent, because they were subjective constructions and fabrications. That is, they attempted to undermine the status of the depressive position and Klein's other notions by undermining the credibility of associations as being mere inferences, distortions, and misconstructions.

This can be seen when Friedlander says that 'analytical knowledge about the first year of life (…) has been inferred from the analysis of grown-ups and children; also, Mrs Klein's conception of the process going on in the mind of the child in the first year of life is inferred knowledge, because children, even if they are only two years of age, do not talk about and do not show directly any memory-traces of the first year of life' (pp 406-407). With her comment, Friedlander does not question the existence of phantasies; yet, she weakens Klein's claims by introducing doubt: Friedlander is 'not satisfied with the evidence so far put forward' (p 406), since Klein allegedly went beyond the facts by inferring analytical knowledge of one-year old babies from older children. Following this line of argumentation, Friedlander concludes that she and others do 'not believe that phantasies of that kind do occur in children as young as that' (p 406). With this assertion, Friedlander tries to break the association forged between infant observation and child analysis. In other words, she is reinforcing a subjective element, contingency, and therefore weakens Isaacs' attempt to associate unconscious phantasy, the depressive position, with a factual status.

Friedlander thus emphasises the inferred nature of Klein's conclusions, highlighting the subjective nature of her views, while fostering dissention. Anna Freud also undermines Klein's notions' credibility by drawing attention to what she sees as the tentative nature of the problematic conjectures regarding unconscious phantasies: 'its existence is inferred from circumstantial evidence collected in the later years of childhood. That means that the inferences drawn from the later material are necessarily influenced by the theoretical views held by the various analysts' (p 420). So Friedlander and Anna Freud attempt to weaken Klein's associations since unconscious phantasies cannot 'speak for themselves' (Latour 1987 p 23). They warn that Klein had crossed a line, going beyond the facts and fostering false generalisation according to a theoretical bias that disregards the integrity of a reality 'out there' to be observed. Put differently, Friedlander and Anna Freud added to a chorus that pointed out the constructed aspects of Klein's investigation in order to undermine – even to invalidate – her claims in terms of what some regarded as newly discovered psychoanalytic facts.

In addition to child analysis, a similar agonistic process takes place in relation to infant observation. Commenting on some of the observation studies used by Isaacs, Lantos for instance introduces doubt into Klein's associations, noting that 'however that may be, the fact that the infant shows signs of discomfort and the tendency to flee back to pre-natal peace and uninterrupted satisfaction is in no way the same as to feel the outside world actively hostile with all the consequences implied by such feeling' (p 350). Lantos therefore increases uncertainty by pointing to Isaacs' active role in connecting infant observation studies with her conclusions, and therefore argues against a logic of certainty that would simply confirm Klein's formulations. According to Lantos 'there is Shirley's report about the first active social responses of the child (…). Actually a rather idyllic description, without a sign of aggression of any kind. So we are rather taken aback when we suddenly are faced with the following conclusion: "In these observations (…) we have full confirmation of Melanie Klein's hypothesis"' (pp 350-51). With this passage, Lantos adds to the pattern of critique based on accusation of subjective views, leading her to conclude that 'now if these are the facts on which her views are based, we are discouraged and without hope of coming to an understanding' (p 352).

The logic of casting suspicion on Klein's assertions persisted throughout the debate. A similar process took place with biology, for example in Friedlander's reaction to Heimann's paper: 'the straightforward exchange of biological and psychological conceptions leads to erroneous conclusions' (p 539). Hoffer adds that 'the libido theory too is a biological hypothesis but unlike the death instinct theory it lends comparatively easily to correlation with psychological facts' (p 550). So once again, there is suspicion concerning the fabricated nature of Klein's assertions, this time stemming from the allegation of a faulty use of biology, according to Hoffer and Friedlander. Glover adds to the argument by declaring: 'why not permit Dr Heimann (or Mrs Klein) also to use such biological analogies in dealing with psychic structure and function? The answer is: simply because neither Dr Heimann nor Mrs Klein are content with analogies. They convert analogies into literal identities' (p 558). Glover attacks Heimann's reference to biology when he states that her argumentation is a process of 'faulty reasoning, confusion of biological analogies with psychological facts (…) jumping from one to another whether they have any connection or not, confusion of subjective and objective, goes on throughout the whole paper' (p 559).

This pattern of argumentation continues unabated when Freud is introduced. For every source of association supporting the facticity of Klein's psychoanalytic notions, the same source is used to discredit her views. While Klein and her followers saw themselves as extending Freud's established views, their opponents disagreed. Glover notes that 'Mrs Isaacs' concept of phantasy is in opposition to Freud's basic concepts of the function of the psychic apparatus' (p 396). He later adds that Isaacs creates an alternative metapsychology, such that her views were 'incompatible with the views of Freud' (p 399). Anna Freud subtly reinforces this argument when she suggests, for instance, that there was 'an outstanding difference between Mrs Klein's theories and psychoanalytical theory as I understand it' (p 418). This comment implies that Klein was at odds with the production of truth in psychoanalysis. In addition, Anna Freud counters Klein's views on early infancy as 'a dark and otherwise empty period with the psychic content which rightfully belongs to it' (p 420) by invoking Freud's notion of autoerotism, and associated ideas, as taking place in the initial stages of psychic development.

These examples are taken from sections two and four of *The Freud-Klein Controversy 1941-45*. Given the enormous number of actors involved (phylogenesis, infant observation, child analysis, clinical cases, etc.) I did not try to isolate and track them through the book. My reading strategy has consisted instead of mapping out and providing a picture of an agonistic clash unfolding at the Controversies, through constant disagreement, and the use of associations in a veritable war to produce or undermine fact construction. To every association put forward to strengthen Klein's claims, there was a rebuttal that tried to weaken her notion of the depressive position as a fact. Given that the examples above were taken from across the discussion meetings, encompassing all the papers presented at that time, they provide a picture of how a clash took place over Klein's claims, with neither side giving in at all.

4. Mixed results towards the resolution of dispute

The Controversial Discussions were formally over once the programme ran its course, after the presentation of four papers and ten discussion meetings. Strachey's final report concerning training analysts' differences in technique and clinical practice was also approved at that same time. Where the question of internal demarcation is concerned, Strachey writes in his report that the Training Committee is 'not *directly* concerned with the truth or falsity of the views upon theory and practice held by Member of the Society' (King and Steiner 1991 p 670). This point in the report was formally approved by members at a Business Meetings in February 1944. So, Strachey's report had an open-ended non-conclusive approach to the issue of demarcation concerning Klein's notions. In a subsequent Business Meeting, members were also called upon to decide on providing another report for the whole debate, while drawing up what they regarded as the scientific lines of their discussions (p 896). According to the minutes of this meeting, this is the response:

> THE CHAIRMAN said that she did not think that the present meeting could make a decision, as there were so few Members present but that Members might be asked to express their views. After some discussion it appeared that the majority considered that it would be better to let individual Members put forward papers expressing individual views rather than formulate conclusions at the present time. The Chairman said that views of other Members could be ascertained in the future (p 896).

This conclusion stands in contrast with the Controversial Discussions so far. While there was previously a great deal of energy and confrontation, there is now a mute atmosphere. More importantly, there is no resolution for Klein's assertions. In this sense, the conclusion stands in continuity with the rest of the Controversies: in spite of all efforts, the impasse remained. No clear internal demarcation was provided, there was no consensual resolution; but tolerance was granted because of an imperative to respect different views. In this respect, the Controversial Discussions is a failed controversy as it ends with a stalemate. In other words, from an ANT perspective, the absence of any positive conclusion also means that the depressive position failed to achieve a factual status, at least at that point in time, and in this sense did not stand on equal terms with the Oedipus complex, either in its significance as an obligatory passage point, or as an undisputed phylogenetically given aspect of mental development.

The outcome of the Controversial Discussions is not the expected resolution of scientific controversy in ANT. On the one hand, Klein's notions did not achieve a factual status – the very ANT criterion that ends controversy. On the other hand, Latour defines a fact as the event capable of silencing opposition. From this specific perspective, the Controversial Discussions was partially successful. With the withdrawal of Edward Glover, Anna Freud, the Schmidebergs, and the Viennese cohort (e.g. Low, Lanto, Friedlander, Hoffer) partway through the proceedings, there were no more contestants to stand against Klein's claims. Usually, the silencing of opposition, according to ANT, is equated to the black boxing of a claim. Yet, in the case of the Controversial Discussions, the opposition was silenced without Klein's claim being black-boxed. Instead, opposition was shut down by resignation from the debate – and sheer exhaustion, I would say. To the best of my knowledge, Latour has never predicted or discussed an outcome where participants abandon controversy this way. When Sylvia Payne, the chairman in the quote above, says that 'she did not think that the present meeting could make a decision, as there were so few Members present', she makes her comment in reference to providing a 'report drawn up on scientific lines' (p 896). I disagree with her view on the issue of Member numbers here. The lack of present Members was not the real problem. It was instead the incapacity to use associations in a way that produced a new fact according to the established criterion of internal demarcation. This would not have changed with more participants because no consensus, nor pathway towards consensus, was ever glimpsed during the debate.

5. Conclusion

In this chapter I have provided an ANT account of the dynamics of the Controversial Discussions. This chapter showed the extensive presence of agonistic dispute around Klein's assertions throughout sections two and four of *The Freud-Klein Controversy 1941-45*, which I consider to be the heart of the Controversial Discussions. In Section 1 of this chapter, I present the Oedipus complex not only as a black box but also as an obligatory passage point, connecting several other associations – among them neurosis and the prospects of 'cure'. It is against this major black box that the depressive position is measured at the Controversies.

I provide an account of the Controversial Discussions in terms of weakening or strengthening the chain of association underpinning Klein's depressive position. I follow the articulation of actors such as biology, infant observation, clinical cases, Freud's work, and so on, as they were associated with

Klein’s claims. As shown in Section 3 above, the use of associations is present throughout the debate, with the two sides digging themselves into two ‘trenches’. For all the effort involved, the associations put forward never managed to convince either side to validate or invalidate Klein’s claims. However, the participants’ inability to move from their positions does not need to be taken as intolerance or unresolved psychological conflict. While I do not rule out these factors, I also conclude that there is something in the structure of knowledge production in psychoanalysis that makes it very hard to achieve consensus where facts are concerned.

The outcome of the Controversial Discussions is well-known in the psychoanalytic milieu. In Section 4, I have commented on its outcome in relation to ANT’s expected outcomes and solutions to a scientific controversy. In my view, ANT does not provide an explanation of the specific cause as to why claims turn into black boxed facts. As Latour remarks, the temporal reversal associated with black boxes, from controversial uncertainty to unproblematic certainty in science, ‘is one of the most puzzling phenomena we encounter when following [scientist’s] trails’ (1987 p 98). In this sense, he treats the production of facts as events. In order to account for them, ANT provides a theory based on the idea of strengthening associations, aligning different actors according to the same interests, and silencing opposition. These are elements that lead to the black box, even though they do not explain exactly why or when a claim becomes a fact. It remains a mysterious event that was not observed by the end of the Controversial Discussions.

Chapter 9: Discussion

1. Introduction

I began this thesis with a picture, a first-hand account from someone taking part in what came to be known as the Controversial Discussions. I do not remember the first time I heard about this scene, but this story has been told to me again from time to time by different people. There is certainly something fascinating about the story of a man standing up to remind others that bombs are crashing, with nobody stopping the discussion, even paying attention to him or the explosions. But to my mind, more interesting is the fact that this story has been repeatedly told: I think what ultimately drives repetition is both the range of possibilities for making sense of the Controversies, and the frustration of forever waiting for a satisfactory explanation to account for what happened in that context.

I set out to investigate the Controversies with an initial research question: how does psychoanalysis demarcate itself, internally, separating the concepts regarded as valid knowledge of the mind from those it rejects as invalid. My impulse to formulate this question, to research and find my own response to it, stems from the problem of fragmentation as it has pervaded my journey in psychoanalytic training. I was faced with these different psychoanalytic schools, with what looks like a veritable confusion of tongues, among heated debates, inter-group animosity, and my own sense of perplexity facing all this. Given the great number of reasons to make sense of the Controversial Discussions (failures in working through paranoid anxiety, group dynamics, the meaning of death, temporality, institutional politics, creativity, paradigms, and power struggles), the available accounts have omitted precisely what participants set out to discuss: the pervading attitude and arrangements towards resolving psychoanalytic differences in relation to the term 'science'. It was in this context, and given my own personal motivations, that I found myself intrigued and dissatisfied with these accounts of the Controversial Discussions. My journey through this study has led me to investigate the Controversies through views on the nature of psychoanalytic knowledge.

As the investigation has unfolded, my research question has grown in complexity. In the context of the Controversial Discussions, I set out to investigate my question according to the literature in sociology of science, and mainly in relation to an ANT framework, where internal demarcation is equated with the production of a fact. Within a specific scene, 'cut' by its formal arrangements, my investigation has led me to identify how psychoanalysis demarcates itself, internally, according to the possibility of establishing claims in biological terms. It is along these lines that I have studied a network of associations, articulated in relation to the Oedipus complex and phylogenesis. Furthermore, it was against these parameters that I have accounted for the dynamics of internal demarcation at the Controversial Discussions, in relation to Klein's notion of the depressive position

I now turn towards an account of internal demarcation, my findings in this study, limitations concerning my theoretical framework, its novel contributions, and its implications.

2. Outcome and the question of internal demarcation

As I wrote in the previous Chapter, the Controversial Discussions ends in a formal stalemate, where no clear solution is put forward by participants, and no resolution for Klein's assertions was offered at that time. In this sense and from an ANT perspective, the depressive position failed to materialize as an event, and be transformed into a black box or a fact, at least by the end of the Controversies. Without this, no clear internal demarcation was provided, no disqualification or validation was achieved, and only tolerance and a lack of conclusion were offered in 1944. This is the outcome of the Controversies, thought in relation to knowledge production. Yet, I think it is very important to make a clear distinction here. With the end of the formal arrangements after a series of Scientific Meetings, what was over was the Controversial Discussions as it was originally set up, and not controversy itself surrounding the status of Klein's assertions. So, in the absence of a clear internal demarcation at that time, controversy surrounding Klein's views remained and persisted after the Controversies. Therefore, there is an important distinction to be made between the outcome of the series of formal meetings, which I covered in this study with my reading of *The Freud-Klein Controversies 1941-1945*, and the actual end or solution for controversy surrounding knowledge production of Klein's assertions. While they overlap, the Controversies and controversy are not one and the same thing.

Scientific controversies can persist beyond their formal and delimited arrangements for debate. This is a normal event in sciences. A good example in the STS literature is the one concerning the existence of gravitational waves. Einstein had originally predicted their existence in the 1910's, and Collins (1985) studied a dispute in the 1970's on this subject, in relation to Joseph Weber's experiments. While Weber, an American physicist, claimed that he had found evidence on the existence of gravitational waves through his experiments, the scientific community at that time soon discredited his views. Doubts remained about the existence of such waves beyond that debate, and it was only in 2016, with LIGO (Laser Interferometer Gravitational-Wave Observatory) that the matter was finally settled, with Einstein's prediction finally validated as a fact. Despite their differences, what I want to highlight is that both Klein's and Weber's processes of internal demarcation took place at one point, sharing in common the fact that the solution to controversy surrounding their subjects lasted for longer and beyond the original dispute – one in the 1940's the other in the 1970's. So the end of a formally delimited debate does not enforce the end of a controversy. It may be that it will require a different moment, a different setting, or more from a network of associations for matters to be settled.

There was no internal demarcation by the end of the Controversial Discussions. From a perspective informed by ANT a fact had to be produced to terminate controversy, such a fact being what I considered as the depression position to be taken as a specific black box, an obligatory passage point. It is from this vantage point that I regard the Controversial Discussions as ending in a stalemate, that is, as an agonistic dispute concerning internal demarcation in psychoanalysis, which was not able to produce a fact or summarily dismiss Klein's claims as invalid. Yet, as I have already mentioned above, the question of internal demarcation around her claims persists beyond the end of the Controversies.

Segal (1979), for example, writes many years later that 'the depressive position marks the crossroad of development between the point of fixation of psychoses and that of neuroses' (pp 80-1). With this remark, she states as a matter of fact the existence of the depressive position within psychic development. Later on in the same text, she also states that Klein did attach importance 'to the fact that the depressive position is a universal phenomenon' (p 89) while also insisting on Klein being 'able to study it not only in relation to pathological but also to normal development' (ibid). These statements are made as simple matters of 'fact' within psychic development. In this respect, they are similar to Latour's account of the factual status of DNA in the 1980s (contrasting with controversy in the 1950s, as discussed in Chapter 3, Section 4). The hint that Klein's notion of the depressive position could be taken as a black box, subsequent to the Controversies, is also reinforced by other commentators. Hinshelwood (1991), for example, defines the depressive position, and writes in relation to it that 'the infant, at some stage (*normally at four to six months*), is physically and emotionally mature enough to integrate his or her perceptions of mother, bringing together the separately good and bad versions (imagos) that he or she has previously experienced' (p 138). Here again, the depressive position is stated as a developmental stage ('*normally at four to six months'*), which implies its existence as a fact in phylogenetic-ontogenetic terms. Both Segal and Hinshelwood write as Kleinians, taking Klein's notion of the depressive position as a fact. Given that many in the Freudian, Independent, and other psychoanalytic groups do not accept this notion – and others such as the paranoid-schizoid position, posited immediately after the Controversial

Discussions (Klein 1946) – as an unquestionable fact, how are we to understand the fate and status of knowledge production in psychoanalysis? How to make sense of truths which are accepted as universal and unquestioned by some and not by others?

It is not my intention, and it is beyond the scope of this discussion, to provide a full array of commentators that came to take Klein's notion of a depressive position as a black boxed fact, several years after the Controversial Discussions. The examples above only indicate that controversy unfolds beyond the Controversies and needs more understanding. I suggest the possibility that Klein's notion of the depressive position is indeed taken by some as a simple departing point (a black box). Yet, in the context of the plurality (Wallerstein 1988) of contemporary psychoanalysis, such views are not held by all. If it is the case that a black box, produced in developmental terms, can be accepted by some but not all psychoanalysts, what sort of internal demarcation has been achieved in psychoanalysis? How should one think about this partial validation, this partial acceptance? It is at this point that I refer back to my study on the Controversial Discussions.

3. Implications from phylogenesis

With my research question, I set out to address the issue of how psychoanalysis internally demarcates itself, using a framework that equates internal demarcation with the production of a fact. While the Controversial Discussions ends with a stalemate and no internal demarcation, there was nevertheless a process of internal demarcation throughout the debate, especially in sections two and four of *The Freud-Klein Controversies 1941-1945,* with the deployment of articulations in an agonistic battle, and through struggle to produce a fact couched in biological (phylogenetic and ontogenetic, with one implying the other) terms.

Following the negotiations of actors in a specific scene, I have identified what I called the 'bad', 'neutral', and 'good' demarcating moves (Chapter 5). In Chapter 6, I trace part of the array of associations as articulations between heterogeneous actors helping to underpin the production of the Oedipus complex as a universal, phylogenetic fact in psychoanalysis. My aim there was to understand, through the articulation of phylogenesis, the role played by biology within the process of psychoanalytic knowledge production, and the reason for such a criterion to be imported into the Controversies. As I have argued in Chapter 6, the reason for using phylogenesis and biology to address a psychoanalytic problem is the need to produce assertions that are not regarded as contingent, or limited to a few clinical cases. This is arguably a problem to be confronted by any psychoanalyst intending to posit claims as facts, as was the case of Melanie Klein at the

Controversial Discussions. It is against this problem that I identify a major reason for the articulation of phylogenesis in psychoanalysis – because it allows a change of scale, from a sample of patients and contingency, to necessity and generalisation associated with facts.

In addition to addressing my research question in terms of how psychoanalysis produces its facts, my finding concerning the role of biology at the Controversies is also a contribution to knowledge production literature in psychoanalysis. Given the central role of phylogenesis in the dynamics of internal demarcation, my analysis makes a novel contribution to a debate that includes Forrester's (1996) idea of thinking in cases and the analogy established between couch and laboratory in STS (e.g. Krause and Guggenheim 2013). In other words, knowledge production depends on a chain connecting cases, technique, a setting, but also biology.

Stengers (2000) provides a useful distinction between laboratory and field sciences, which illuminates the role of phylogenesis as accounted for in this study. According to Stengers, while laboratory sciences rely on experimental apparatuses, field sciences need to work in other ways to make their claims (p 139). So, without recourse to experimental apparatuses and 'replication', field sciences need to rely on what Stengers regards as 'the possibility to collect *indices* (…) to identify relations, not to represent a phenomenon like a function furnished with its independent variables' (p 140). In relation to such *indices* that establish relations without recourse to laboratorial specifications, Stengers (2000) relies on Ginzburg (1980), as he makes the original differentiation between a science of proofs (Stengers' laboratory sciences) and a science of indices (Stengers' field sciences). Ginzburg (1980) includes archaeology, geology, physical astronomy and palaeontology as instances of sciences of indices. To this list, Stengers (2000) also adds Darwin's evolutionary biology, with its 'phyletic heritage, [and] pathways of development' (Gould and Lewontin 1979 p 581). In common, all these sciences of indices are defined by 'making retrospective prediction' (Ginzburg 1980 p 23) because 'when causes cannot be repeated, there is no alternative but to infer them from their effects' (ibid). In other words, in the absence of a laboratorial setting where experiments can be repeated, field sciences rely on knowledge production through one alternative path if knowledge is to be produced: the use of inference of causes from effects, with *indices* being the fundamental connection between what can be regarded as 'effect' to their 'causes'.

Like field sciences, psychoanalysis does not rely on any sort of laboratorial repetition (Forrester 1996; Krause and Guggenheim 2013). In this context, I think phylogenesis becomes central because it shows how knowledge production in psychoanalysis bears similarities to field sciences. It is beyond the scope of this thesis to draw on the implications of the debate involving Gould and Lewontin (1979) and neo-Darwinians. I also do not seek engagement with a discussion concerning in-depth differentiations between field and laboratory sciences. I draw on Stengers (2000), Gould and Lewontin (1979) and Ginzburg (1980) for the purpose of discussing phylogenesis as the very *index* operating in psychoanalysis, and operating in a similar fashion to those indices used by field sciences. I am not equating psychoanalysis with field sciences but rather pointing to a feature in the psychoanalytic process of knowledge production. The given definition of indices as those instruments 'making retrospective prediction', where 'causes are inferred from their effects' fits perfectly with my argument in Chapter 6, where I showed that the totemic origins in the 'past' of the Oedipus complex cannot be older than 1911 because they are made out of a 'retrospective prediction' in relation to Freud's clinical work. Furthermore, part of my argument in Chapter 6 also shows exactly how 'causes' (biological transmission of the Oedipus complex) become inferred from their 'effects' (a limited sample of cases of neurosis in children) through the work of translation that seeks convergence between previously unrelated actors. Therefore, if field sciences can be defined according to the use of such indices, so psychoanalysis can be regarded as relying on a similar explanatory framework, where knowledge production is concerned.[71]

With the discussion above I face an unforeseen limitation to my theoretical framework. Both ANT and STS focus their studies on controversies solely in relation to laboratorial situations, such as Latour's (1986, 1988) and Collins' classic studies (1985) of Pasteur's microbes and Weber's gravitational waves. ANT has never stipulated a distinction between field and laboratory science, I think, because it has just focused its studies of controversies in relation to laboratories.[72] Even so, I have found the theoretical tools provided by ANT very useful in accounting for the Controversial Discussions, and follow its dynamics of internal demarcation. So ANT did provide a way to understand the Controversial Discussions and engage with the term 'science' and its repercussions in the debate. However, with psychoanalysis bearing a fundamental resemblance to field sciences, I am confronted with implications that I believe have not yet been properly accounted for in ANT.

I think there is one trait associated with field sciences and production of facts that seems particularly important, in the context of internal demarcation in the Controversial Discussions. It relates to the difficulty in field sciences for one claim to invalidate an alternative competing claim.[73] In this sense, Stengers (2000) notices that 'no terrain is valid for everyone, no one can authorize the "facts" in the experimental sense of the term. What one terrain allows us to affirm, *another terrain can contradict*,

[71] Mezan (2014) provides an interesting account on similarities in the argumentative styles between Darwin's evolutionary biology and psychoanalysis (pp 565-6).
[72] With this focus on laboratories, and lack of distinction between field and laboratory life, ANT and STS consequently do not provide any theoretical tools to distinguish 'soft' and 'hard' sciences.
[73] On the other hand, ANT is predicated on the laboratory sciences' feature that competing claims invalidate one another (e.g. Latour 1987, 1988, 1999b).

without one of the witnesses being false, or without preventing the two situations from being judged as intrinsically different' (p 140). In other words, it is only in laboratory sciences that to affirm a claim implies invalidating contradictory claims as false. That is not the case with field sciences, since their original site of investigation, the terrain, 'in the depths of the ocean, in the museums where collected fossils are examined, in the forests where samples are harvested' (p 139) can lead to contradictory findings that are 'intrinsically different' (p 140) without invalidating one another as false. The point of non-invalidation by contradiction is also exemplified by Lewontin and Gould (1979) as they discuss their field science (evolutionary biology): in natural history, all possible things happen sometimes; you generally do not support your favoured phenomenon by declaring rivals impossible in theory' (p 585).

In hindsight, I realise that all controversies studied by Latour provide the pattern where one claim emerges as black boxed, and the competing ones are ruled out. Yet contradictory claims could co-exist as equally valid in field sciences even if they contradict one another and do not cohere. Looking back on my investigation, I have at times applied the ANT tenet that shutting down all opposition is one of the ways to regard scientific activity as producing a fact (or as one of the ways to identify a black box, see glossary). This tenet in specific may need to be reconsidered in relation to psychoanalysis. Following the distinction proposed above in relation to field and laboratory sciences, the equation in ANT that shutting down opposition through the showing of strong associations *is* fact production could be more appropriate in the context of laboratory science activity. This equation would not apply for field sciences, where conflicting claims can coexist without being mutually exclusive. So there can be alternative facts produced without the need of coherence or unity. The implication is, then, that indices can produce facts that are incoherent with one another, without any of them being necessarily invalid. Expanding from this towards psychoanalysis, I posit that the clinical setting in psychoanalysis should also to be thought of as a kind of 'terrain', even though the setting also shares some features with a laboratory. In the specific case of psychoanalysis, the setting may remain rigid (Krause and Guggenheim 2013) but patients are 'non-replicable'. Being unique, patients inform the psychoanalytic setting as a 'terrain', where variation does not depend on changing the terrain (different oceans, different museums, or different forests) but rather on different patients. To paraphrase Stengers (2000 p 140), what one set of patients observed in the rigid psychoanalytic setting allows us to affirm, *another set of patients can contradict*, without one of the witnesses being false, or without preventing the two situations from being judged as intrinsically different. While I point to this limitation of ANT in relation to my study case here, ANT still provides relevant tools for the investigation of knowledge production as I have demonstrated in this thesis. There are, for example, the notions of knowledge production, the agonistic dispute, black box as an event that purifies associations and establishes starting points as facts, and the importance of associations underpinning any claim. These and other tenets hold true and have helped me navigate *The Freud-Klein Controversy 1941-45* according to my research question, and in relation to psychoanalysis.

I have shown that participants in the Controversies searched for coherence with Freud's work to validate Klein's claims, as discussed in Section 5 of Chapter 3. Yet, as discussed in the previous paragraphs, coherence should not be invoked in internal demarcation where competing claims are based on the production of *indices*. The Oedipus complex as a black box and its associated notions (libidinal development, primary narcissism) are no guarantee or condition for the invalidation of the depressive position because they should not be required to cohere. Therefore, if we add the implications of Stengers (2000), Gould and Lewontin (1979) and Ginzburg (1980) to my analysis, the process of internal demarcation at the Controversies would still depend on the production of the depressive position as a fact. But this fact should be allowed to emerge, if it is well articulated, regardless of cohesion or any strict sense of continuity with the Oedipus complex.

With the above, I do not share the view of some authors (e.g. Bergmann 2014) that the stalemate or deadlock by the end of the Controversial Discussions is a fundamental flaw or failure of psychoanalysis. Instead, I consider it in terms of an incapacity to produce a new fact, at that point in time. Beyond this, I think that the Controversies mark the moment when psychoanalysis is confronted with the fact that its indices and claims out of terrains, similarly to field sciences, do not need to cohere. In this way, the stalemate can be regarded as the beginning of fragmentation in psychoanalysis, with Melanie Klein's depressive position showing that it is possible to assert claims, to take the underlying associations seriously, and not incur any fundamental flaw. With the considerations above, what has been achieved with the outcome of the Controversial Discussions is an opening moment in psychoanalysis, a situation of possibilities where its black boxes, its facts produced through the use of a phylogenetic index, may be regarded as valid even if incoherent and contradictory among themselves. Before the Controversies, the Oedipus complex was the main fact structuring psychoanalytic knowledge. Thereafter, this structure lost its false sense of cohesion and unity, without there being a fundamental problem with it. So the Controversial Discussions did not demarcate any new psychoanalytic fact. Instead, they demarcated as acceptable the possibility of co-existent contradictory truths. Along these lines, it is possible to accept co-existent contradictory truths, as one adopts a constructivist perspective, and leave aside a modern understanding of psychoanalytic knowledge.

4. Pluralism, stalemate, and knowledge production

In 1987, Robert Wallerstein was the president of the International Psychoanalytic Association (IPA), the umbrella institution that represents all psychoanalytic associations in the world. The 35th IPA bi-annual congress was taking place in Montreal. In the opening-day plenary, he selected 'a topic of sufficient importance to the worldwide psychoanalytic community, in the hope that a dialogue about it initiated or furthered here [at Montreal] can enhance our shared psychoanalytic understanding and commitment' (1988 p 5). His topic was the increasing psychoanalytic diversity, 'or pluralism as we have come to call it, a pluralism of theoretical perspectives' (ibid). In his address, Wallerstein's main argument is that there is a diversity of psychoanalytic theories in present-day psychoanalysis: 'multiple (and divergent) theories of mental functioning, of development, of pathogenesis, of treatment, and cure' (p 11). At the same time, he posited a common ground to all psychoanalysis, based on 'our shared definitional boundaries. Here we do, I think properly, revert to Freud and his definitional statement of 1914 on "the facts of transference and of resistance"' (p 12). For this commonality, he invokes Freud's *On the History of the Psychoanalytic Movement* (1914), and quotes him: 'any line of investigation which recognizes these two facts and takes them as the starting-point of its work has a right to call itself psychoanalysis' (p 16). In essence, Wallerstein mapped out contemporary psychoanalysis as diverging into different theoretical perspectives, which nonetheless would share a common clinical purpose based on the 'two facts' mentioned above.

Two years later in the following IPA congress in Rome, the topic of a 'common ground in psychoanalysis' became the main theme of the whole congress. It is against this background that he concludes in his paper that the diversity of theoretical structures in psychoanalysis 'is not yet adequate to those common clinical understandings or to our deep needs as a discipline and as a science of the mind' (1991 p 19). He also adds: 'if we are to become both the kind of discipline and of science that we desire to be, we do truly need the kind of coherent and unifying overall theoretical structure that Freud tried so brilliantly to create' (ibid). Years later, but still on the same topic, Green (2002, 2004) criticises Wallerstein's points above, highlighting that psychoanalysis was undergoing a crisis where not even the posited common ground based on clinical work really existed, or was too loosely defined in reality. Wallerstein (2005) subsequently responded to Green, agreeing with his concerns about a sense of a psychoanalytical crisis while also positing a solution based on the need for an ‘overarching coherently unified clinical theory, and ultimately then an increasingly unifying general theory’ (p 637).

With this thesis, I try to contribute to the question of pluralism in psychoanalysis, centred on the co-existence of divergent psychoanalytic theories. As a way out of this situation, Green suggests that psychoanalysis needs a debate based on ‘the exposition of a sequence of sessions and on a psychoanalytic process revealed at sufficient length’ (2005 p 628) in order to address the ‘kinship between different theories’ (ibid). Resonating with Green’s suggestion and the need to find a solution for fragmentation, Wallerstein (2005) writes that ‘it is my argument that what Green calls for here is exactly what I call empirical research’ (p 637), and ‘what Green calls for here is exactly what I have always meant by the necessary empirical research on which incremental building of the psychoanalytic knowledge base must rest’ (ibid). Finally, ‘it is my expectation for the continuing evolution of psychoanalysis that a growing body of empirical process and outcome research in psychoanalytic therapy will build increasingly the body of evidence of common ground’ (ibid), which should lead to clinical theory and general theory becoming coherent and more unified.

With the above, there are two main issues I want to highlight. The first is Green’s call for improving the debate in psychoanalysis, similar to that of Bernardi (2002), as discussed in Chapter 2. The second is the idea that addressing pluralism relies on ‘empirical research’, as it would bring about an increased common ground, more cohesion, and unity. I agree with the first issue, whose aim is to pursue better methodological standards for discussions, but I consider the second issue more problematic. My contribution to the debate on pluralism focuses on this point, and is of course based on my study here.

This thesis has been centred on the process of knowledge production in psychoanalysis. By the time of the Controversies, the Oedipus complex was already black boxed. With the depressive position, I give indications in section 2 above that it may have been black boxed after the Controversial Discussions for some but not others. Nevertheless, what I see in common with both notions is that in order to achieve their status as a fact, there needs to be previous and laborious work of negotiation involving associations. My point then is that the very reality of facts depends on an interaction of many actors. From the perspective of a sociology of association, we could say that reality is a highly mediated output which can only be established after, and according to, the associations that produce facts. Within this process of fact production, there can be associations with phylogenesis, clinical cases, infant observations, child analysis, totemic societies, theoretical perspectives, technique, use of transference, and so on. Within this constructivist perspective, it does not make much sense to follow Wallerstein's call for 'empirical research' to illuminate and help unify different theoretical perspectives.

If my reading of the Controversial Discussions based on a constructivist perspective is plausible, this study can offer the following insight to understandings of psychoanalytic pluralism: it is possible to improve the debate and understand it better, but neither fragmentation nor lack of coherence will necessarily go away. In a sense, to call for 'empirical research' now is somewhat equivalent to saying that dispute at the Controversial Discussions could be sorted out based simply on careful clinical examinations, as these should provide definitive and unifying solutions. The call for a definitive solution such as this seems even more problematic to me, if we consider that Collins (1985) argues for the lack of any definitive solution, a lack of crucial experiments in sciences that can easily sort out and unify differences (Chapter 2 Section 2). After all these years, and for all sorts of debates and clinical examinations that have already taken place, what I think was inaugurated with the Controversies – the possibility of non-coherent facts (entangled with differences in technique and theory) coexisting in psychoanalysis – becomes an ever more plausible option than waiting for 'empirical research' to provide a definitive solution. My study suggests a view of psychoanalytic pluralism as a question enmeshed in the production of knowledge out of terrains and in terms of highly mediated facts. These should not be required to cohere or even be taken as necessary and universal.

5. Contribution to sociology of science and psychoanalysis

I turn my attention now to the sociology of science, with a focus on the text that has delved the most into psychoanalysis. The main argument of Krause and Guggenheim (2013) concerns the question of differentiation of research, diagnosis, and treatment in psychoanalysis, in relation to divided spaces, tools, and professional roles. Like many fields of expertise (e.g. medicine), psychoanalysis covers these three aspects of research, diagnosis and treatment (p 193). However, psychoanalysis does not truly distinguish them, given its insistence on a specific setting as 'central to all aspects of knowledge-production' (p 187), and without ever adapting this space to any of these three purposes 'in their own right' (ibid). Specifying this unusual non-differentiation of tasks, Krause and Guggenheim (2013) attempt to explain some of the problems in psychoanalysis in relation to this lack of division.

One central point for Krause and Guggenheim (2013) is to compare the psychoanalytic setting with the laboratory as a place of research. For them, psychoanalysis builds its setting so as to produce placelessness and decontextualisation in a similar way to the scientific laboratory (p 194). However, unlike the laboratory, the psychoanalytic setting does not adjust itself 'to different objects of knowledge or to the different questions it seeks to explore. Psychoanalysis standardises its setting in the same way for all conditions and for all patients' (p 196). As a result, Krause and Guggenheim (2013) see psychoanalysis as not being able to generalise out of laboratories as natural sciences and psychology do, due to its incapacity to control specific conditions and factors in a more flexible setting (p 196). Given what they see as rigidity, psychoanalysis only operates by relying on singular cases to 'show the universality of the theory' (ibid) but without ever developing 'intermediate categories, such as for groups of patients or conditions' (ibid). What is more, there is a lack of progress in advancing psychoanalytic knowledge, since 'theoretical knowledge always already exists as a concept (e.g. as a neurosis, such as the Oedipus complex) and it is then newly adapted for each specific case' (p 197). Therefore, the generalisation that should arrive from a more flexible setting instead only occurs due to Freud: 'generalisation in claim-making happens, and it happens mostly via the texts of Freud, who had already taken the step towards generalisation for all future psychoanalysis. Every generalisation in psychoanalysis arrives back at Freud – if it does not arrive back at Freud, it is no longer psychoanalysis' (p 197).

With psychoanalysis concerned only with the generalisation-single pattern as shown above, 'beyond a set number of neuroses identified in Freud's writing psychoanalysts reject all newer labels such as anorexia or addiction and do not produce their own' (p 201). So the structure of knowledge production is not only problematic in relation to generalisation but also due to its insularity and incapacity to update itself. For Krause and Guggenheim (2013), the lack of engagement with these newer labels are the very indications of psychoanalytic insularity, since psychoanalysis would remain stuck in not recognising 'anything between the individual case and the universal properties of psychic structure identified by Freud' (p 201). The issue of insularity coupled with the lack of differentiation are also at the root of other problematic issues for psychoanalysis, such as 'the relative lack of interest among psychoanalysts in verifying outside the practice whether it is effective' (p 202), let alone concerns with the efficacy of psychoanalytic treatment outside the consulting room, as these issues would be more appropriately addressed if diagnosis were properly recognised as a discrete task (p 200) separated from the setting.

Krause and Guggenheim (2013) see as problematic consequences stemming from a psychoanalytic practice that refuses to break down its setting into more specialized spaces, tools, and professional roles. In this sense, they are comparing psychoanalysis with other activities such as medicine, where 'the research lab is different from the research hospital and from the GP practice. Various kinds of medical instruments exist for different kinds of diagnosis and treatment' (p 193). In this medical differentiation of discrete tasks, for example, there can be a delegation of 'lesser tasks to non-professional[s]' (p 199), such as nurses, who are not necessarily the medical doctor. In addition to laboratory sciences, psychology, and medicine, Krause and Guggenheim (2013) draw on examples involving architecture, law, academia, cognitive behavioural therapy (CBT), and even criminology.

I now turn to how my own research can inform some of the points raised by Krause and Guggenheim (2013). One key aspect is my proposition that psychoanalytic knowledge production resembles field rather than laboratory sciences. The main distinguishing feature is the idea of 'replication', which is not possible with the former but helps to define the latter. Krause and Guggenheim's approach does indeed point to the limitations on the use of the psychoanalytic setting, in relation to laboratory sciences in general and more specifically to medicine and psychology. Yet, their line of inquiry seems to restrict their argument to the idea of limitations brought upon psychoanalysis, given its rigid and non-differentiated use of the setting, rather than considering psychoanalysis as an idiosyncratic form of knowledge production, with some of its features resembling laboratories, and other aspects of it more similar to field sciences.

I therefore subscribe to Krause and Guggenheim's view that generalisation does not arise from the setting, due to its incapacity to control specific conditions and factors as would be the case in a more flexible setting (p 196). However, I disagree with their assessment that such a rigid setting is to be regarded as a lack, a failure, to follow other activities such as psychology and medicine, because it is here that psychoanalysis starts resembling field sciences. For example, I see a very different picture where Krause and Guggenheim (2013) regard singular cases to 'show the universality of the theory' (p 196), where 'theoretical knowledge always already exists as a concept (e.g. as a neurosis, say the Oedipus complex) and it is then newly adapted for each specific case' (p 197), as well as where Krause and Guggenheim (2013) regard the 'generalisation in claim-making happens, and it happens mostly via the texts of Freud, who had already taken the step towards generalisation for all future psychoanalysis. Every generalisation in psychoanalysis arrives back at Freud – if it does not arrive back at Freud, it is no longer psychoanalysis' (p 197).

I agree with the claim that generalisation does not take place in the setting, but Krause and Guggenheim fail to account for what is in the middle, between singular patients and universal claims based on generalisation; it is this aspect that relates to field sciences. This middle is precisely what happens outside and beyond the setting, with the associations discussed in this thesis, mainly in relation to phylogenesis. What unites generalisation and singular cases is precisely the process of translations and associations, which provide a path through indices towards the production of facts.

Indeed, as per my study of the Controversial Discussions, generalisation (or what was called facts, truth) was not pursued by participants according to a clinical discussion. Nevertheless, I have also shown that universal claims and generalisation were pursued at the Controversies according to a negotiated process, involving singular cases but also other associations. As in the case of the Oedipus complex, it is not simply a generalisation that takes place in Freud's texts. It is the text that helps reveal the work of associations (e.g. *Totem and Taboo,* King and Steiner 1991).

As I have argued here, and unlike Krause and Guggenheim (2013), the Oedipus complex is not a knowledge that 'always already exists as a concept (e.g. as a neurosis, say the Oedipus complex)' (p 197). From an ANT perspective, facts are events which need to be strengthened via a network of associations in order to be produced. They have a history. The Oedipus complex, for example, was not simply stated in a text, in *Totem and Taboo*. With Chapter 6, I have painstakingly followed the array of associations that eventually allowed the Oedipus complex to be more established, in connection with phylogenesis operating as an index. The status of the Oedipus complex as a factual claim evolved with time, and is identifiable as black boxed by the time of the Controversial Discussions. Here, I want to emphasise that a notion can be taken as a given, as 'always already existing' only after it has been black boxed – and not before – as with the example of DNA in Chapter 3. Furthermore, ANT posits that such a temporal reversal associated with black boxes, from controversial uncertainty to unproblematic certainty in science, 'is one of the most puzzling phenomena we encounter when following [scientist's] trails' (Latour 1987 p 98).

I argued that the status of truth had already been conferred on the Oedipus complex, but not on the depressive position, by the time of the Controversial Discussions. Yet this is not equivalent to Krause and Guggenheim's assertion that psychoanalytic generalisation is restricted to Freud. First of all, Krause and Guggenheim's contention is a static one, suggesting that generalisations in psychoanalysis happened with Freud for all generations to come. My view on this matter is that the Oedipus complex was articulated, mainly by Freud, while he also provided a criterion of internal demarcation based on an *index* of phylogenetic inheritance. Furthermore, the Controversial Discussions marks exactly the moment where the possibility of generalisations in psychoanalysis no longer depend on Freud's claims, or even cohere with Freud (see Section 3 above) without being disqualified as psychoanalysis. This can be seen in the case of the depressive position, a notion not stemming from Freud, that eventually becomes a black box, at least for some, as I suggest in section 2 above, after the Controversies. In this sense, the Controversial Discussions indicate a dynamism in psychoanalysis, opening the possibilities of new and pluralistic black boxes in psychoanalysis –a scenario that seems lacking in Krause and Guggenheim (2013).

Finally, I also take issue with the assertions in Krause and Guggenheim (2013) that psychoanalysis only relies on singular cases to 'show the universality of the theory' (p 196) but without ever developing 'intermediate categories, such as for groups of patients or conditions' (ibid). It may be the case that psychoanalysis does not respond promptly to newer labels and groups of patients, such as those related to anorexia and addiction (p 201). However, this is different from asserting that psychoanalysis does not develop intermediate categories at all. I think that psychoanalysis has developed 'intermediate categories' specific for groups of patients or conditions. Even with the rigidity of the setting, it has for example done so by selecting or grouping patients for treatment according to their specific diagnosed conditions, as in the case of the Portman Clinic, in London, where perverse and violent people have been treated as a specific group of patients. This arrangement has also led to the development of theories that should not simply be taken as generalisations (e.g. Glasser's (1979) notion of core complex, Stella Weldon's (1988) differentiation of perversion in men and women). Another example is provided by the *École Psychosomatique de Paris*. These show that developments can occur in psychoanalysis without relying on control and repetition, but rather on the possibility of adjusting to the 'terrain' (and not the 'laboratory') where treatment and research take place.

6. Conclusion

To end this study, I would like to summarise the main contributions that are made here, beginning with the term 'science'. The word 'science' remains afloat in the psychoanalytic literature (for example, it is recurrently present in Wallerstein's (1988, 2004) discussion of pluralism, in addition to the literature on psychoanalytic controversy as in Chapter 2). At the same time, it has received so much criticism from philosophy of science, among others, that it has been difficult to use the term 'science' according to a proper and consistent account. With my focus on knowledge production and internal demarcation, I have offered an alternative to what I see as the usual approach to the term in psychoanalysis, eschewing the tension between avoiding the term (due to the negative normative response to psychoanalysis) and the acceptance of a scientific realism in psychoanalysis. As I argue in Chapter 7, modern thinking can be placed within a different tension, where the term 'science' is read instead in a contrast of realism versus knowledge production. So it is against a modern background that I have shown the presence and influence of an array of associations, a network of actors, as they underpin the dynamics of the Controversial Discussions to an important extent.

With my study, I have focused on the role of biology, mainly through phylogenesis, in knowledge production in psychoanalysis. It is with biology identified as a criterion of internal demarcation that I offer an alternative reading of the Controversial Discussions. At the same time, I am contributing to the literature that relates biology and psychoanalysis by offering a different understanding of the role of biological notions originally articulated by Freud. More specifically, I acknowledge the importance of biology while avoiding Sulloway's equation of Freud to a 'biologist of the mind', or reducing psychoanalysis to a sort of a branch of biology. Instead, I am offering the argument that psychoanalysis relies on biology – and some aspects of field sciences – in order to produce its knowledge. While I have focused on the role of biology for the Controversial Discussions and the process of knowledge production in psychoanalysis, I am not suggesting that the same role can necessarily be applied to other psychoanalytic controversies. There can be other arrays of associations that emerge as more relevant in other contexts, while also revealing other important aspects within the process of knowledge production in psychoanalysis. In a typical ANT fashion, I think that each controversy needs to be addressed according to its own specificities, and to the process of following relevant actors in context.

With a constructivist perspective, I have also argued for an understanding of what is often regarded as the stalemate or deadlock by the end of the Controversial Discussions. Instead of lamenting the incapacity to cohere, to unify in psychoanalysis, after Freud's death, I argue that the Controversies are a moment of openness. Why is it that psychoanalysis seems so intolerant of non-coherence? Mol (2002) for example provides a very good counter-example of how non-coherence can harmoniously co-exist at a hospital. Accordingly, the stalemate by the end of the Controversies may give the sense of a failure. Yet, for me, it was the moment that marked the emergence of non-coherence in psychoanalysis. The failure to settle the dispute at the Controversial Discussions, as per Bergmann's (2004) complaint, should, in my opinion, be taken as cause for celebration because 'the more you articulate controversies, the wider the world becomes' (Latour 2004a p 211). There should be:

> No such trauma with articulation because it does not expect accounts to *converge* into one single version that will *close* the discussion (…) Articulations (…) may easily proliferate without ceasing to register differences. (…) The more contrasts you add, *the more differences and mediations you become sensible to*. Controversies among scientists (…) feed articulations, and feed them well. (p 211).

After the Controversial Discussions, psychoanalysis became a 'wider world' because it tolerated more articulations, more associations within it, for all frictions. It is also along these lines that I want to conclude my views on the issue of pluralism. There is a sense of crisis associated with psychoanalysis in terms of a lack of 'kinship', and too many differences. Instead of hoping that there will be a solution that unifies clinical and theoretical approaches via empirical data, as Wallerstein (2005) does, I suggest giving up on the idea of unity, and focusing instead on how to relate to the co-existence of different realities and non-coherent facts. One important consequence here is that the idea of non-coherent truths puts forward an important question. Instead of a simple search for truth, psychoanalysis producing its facts needs to establish ways in which it can negotiate and arbitrate the existence of non-coherent truths. This is a political question entangled with understanding of knowledge production in psychoanalysis. At the same time, it involves giving up on a modern stance of facts, and provokes a debate on the cohabitation of non-coherent truths that goes beyond the question of pluralism as simply a matter of different personal perspectives on psychoanalytic activity.

Appendix

The Freud-Klein Controversies 1941-1945, edited by Pearl King and Riccardo Steiner, was published in 1991, almost fifty years after the Controversial Discussions. This was the first time these relevant documents on the history of psychoanalysis in the UK became publicly available. *The Freud-Klein Controversies 1941-1945* is an almost one thousand-page book, with documents covering 'the actual discussions that took place in the British Psycho-Analytical Society between 1941 and 1945' (King and Steiner 1991, p xxvii). Dealing mostly with primary documents, the material for this book stems from the archives of the British Psychoanalytical Society, and the archives of the Melanie Klein Trust, which are deposited at the Wellcome Foundation in London.

According to its editors, *The Freud-Klein Controversies 1941-1945* is organised in chronological order and divided into five sections, each of which starts with an editorial comment. In terms of primary sources, section one comprises verbatim minutes of five Extraordinary Business Meetings, which took place between February and June 1942. These meetings took up resolutions provided by members of the British Psycho-Analytical Society. Its topics included questions concerning scientific aims and methods, scientific differences, aims of the Society, tenure of office, and whether or not institutional life should resume a degree of normality at that stage (during the war). As the meetings progressed, issues concerning the status of psychoanalysis as a subject founded by Freud, as a scientific activity, and as having to face differences with the development of Klein's ideas began to dominate the debate. The verbatim record of these meetings reveals a debate mainly in the shape of an extensive dialogue involving several members, such as Melanie Klein, Anna Freud, Joan Riviere, Susan Isaacs, Edward Glover, Ernest Jones, Melitta and Walter Schmideberg, Donald Winnicott, and so on. They took turns to raise and reply to questions, mainly in relation to what they saw as scientific problems such as the aims of psychoanalysis, Freud's conception of it as a scientific enterprise, and how to deal with emerging differences. During the debate, other issues were raised: the theoretical orientation of training analysts (and candidates), a question on technique, and the possible numerical dominance of those under the influence of Melanie Klein. At the end of the fifth Extraordinary Business Meeting, an 'armistice' was called to restrain participants from making personal attacks and accusations until they could find what they saw as a scientific solution for their scientific problems. Subsequently, at an Annual Meeting in July 1942, Marjorie Brierley provided a programme for debate centred on questions of compatibility with Freud's thinking on mental development, and a format for discussion was approved. It was decided that Melanie Klein and her followers should prepare papers and present their views, organised according to specific topics, to be followed by discussion. The first paper marks the beginning of the book's second section.

After Steiner's editorial comments, the second section starts with Isaacs' classic paper on unconscious phantasy. Thus begin the discussions on what participants regarded as scientific differences. The papers organised Klein's ideas in terms of theoretical formulations, findings, and evidence. After Isaacs' paper, there followed five rounds of discussions over five Scientific Meetings, between January and May 1943. A second paper by Paula Heimann on introjection and projection was then circulated. It was followed by two rounds of discussions, between October and November 1943. In between these two papers, *The Freud-Klein Controversies 1941-1945* includes a chapter concerning a debate on institutional matters. It considered the role and participation of the British Society of Psycho-Analysis in the UK medical scene, which was being reorganised at that time, and also concerned issues such as lay and medical doctor analysts. As with other sections, it is presented in the book as a set of verbatim minutes, mostly in terms of dialogues. In total, this second section of *The Freud-Klein Controversies 1941-1945* comprises more than three hundred and fifty pages.

Section three mainly contains memoranda and a draft report which explore the effect of what was regarded as scientific differences on the training of candidates. It covers the period between September 1943 and March 1944. Unlike sections one, two, four, and five, there are no verbatim accounts here only a series of memoranda on personal views of the Training Committee members (Edward Glover, Marjorie Brierley, Anna Freud, Melanie Klein, Ella Sharpe, Sylvia Payne) on technique, together with James Strachey's draft and final reports on the subject. This constitutes ninety pages of *The Freud-Klein Controversies 1941-1945.*

After King's editorial comments, section four begins with Heimann and Isaacs' paper on regression, followed by one round of verbatim discussion during a single Scientific Meeting. These were followed by Klein's paper on the emotional life of the infant and the depressive position, which was followed by two rounds of discussions over two Scientific Meetings. These meetings took place between February and May 1944. Section four is also marked by the absence of certain participants who had decided to leave the debate following Strachey's draft report in section three. These include Edward Glover, Anna Freud, and a cohort of 'Viennese' psychoanalysts; some of these did participate by submitting written comments to be read aloud, however. Section four comprises more than one hundred and fifty pages of *The Freud-Klein Controversies 1941-1945.*

Section five, the last one of *The Freud-Klein Controversies 1941-1945,* includes mainly verbatim minutes of three Extraordinary Business Meetings, and one Business Meeting. These took place between February and June 1944. The first two meetings deal with Glover's letter of resignation from the British Society and Institute, in terms of his views of the Controversial Discussions and how to replace him, in his institutional role at that time. The third meeting of this section deals with the formal approval of the recommendations put forward by the Training Committee in Strachey's final report. It is at the Business Meeting, the last meeting of this section, that a formal outcome for scientific differences was to be decided. After Payne's motion that no report should be drawn up on this matter, participants considered that 'it would be better to let individual Members put forward papers expressing individual views rather than formulate conclusions at the present time. The Chairman said that views of other Members could be ascertained in the future' (p 896). Section five also covers some post-Controversies institutional matters, such as limiting the tenure of office, organising members in committees, and re-defining roles within that institution. As part of this reorganisation, Sylvia Payne formally replaced Ernest Jones as the new president of the British Society in July 1944. In this new post, Payne reached out to Anna Freud in order to organise a training programme that would satisfy Freud's daughter and persuade her supporters and allow them to take part in it. This programme was formally approved in 1946, with a division between Courses A and B: '*Course A* would continue to be organized as formerly, teachers being drawn from all groups, and *Course B,* which would teach technique along the lines supported by Miss Freud and her colleagues' - (p 906). According to the editors of *The Freud-Klein Controversies 1941-1945,* this arrangement, represented 'an unwritten "gentlemen's agreement" that there should be representatives of all three "groups" on the main committees of the Society' (p 907).

Glossary

Association – the establishment of connections between actors, through the process of translations.

Black box – a central notion that is defined in different and complementary ways in ANT. A black box is a synonym for a fact within a constructivist perspective. So the term black box stands for the passage from subjectivity and singularity to objectivity and universality of facts. The term black box has its origins in cybernetics, where a piece of machinery or command is too complex. Since this complexity does not need to be fully engaged with, it is replaced by a black box. The term black box also stands in ANT for the process wherein a complex array of articulations and associations is taken as a timeless starting point. In this sense, the black box also accounts for the replacement of complexity and controversy by unproblematic simplicity. In other words, black box also marks the event wherein a claim stops being controversial and is transformed into certainty. A black box is also measured according to the capacity to render a claim unproblematic, which is the same as to say that all opposition has been shut down. Once a black box is closed, it is very difficult to open it again, and the associations underpinning the black boxed fact are no longer deemed polemic and do not draw attention as in situations of controversy.

Fact, **truth**, **objective knowledge** – these terms are used interchangeably in this thesis. They refer to the possibility in knowledge production of a claim to be black boxed. Once a claim becomes black boxed, it can simply be taken as a fact, truth, or objective knowledge.

Internal demarcation, (scientific) demarcation – internal demarcation concerns the validation or rejection of any notion put forward as a potential fact in a specific field of investigation. It should be differentiated from (scientific) demarcation, a term usually associated with philosophy of science, and concerning the status of a whole field of investigation as scientific or not. The notion of internal demarcation does not necessarily bring into question the issue of the scientific status of a specific field of knowledge.

Knowledge production – this refers in ANT to the whole spectrum of chained activities in which scientists are involved in order to produce a fact. Knowledge production encompasses any aspect of these activities within a field of investigation, such as laboratories, other sites, machines, instruments, experiments, graphs, notes, opinions, academic literature, and so on. Knowledge production can only be specified more fully as it starts accounting for the situated contexts and the particularities of each field of investigation. There is no overarching or predetermined features of knowledge production, apart from the idea that it focuses on processes wherein facts can be established. In this thesis, knowledge production refers to the process of production of facts according to the articulations as they are established in psychoanalysis.

Matter of fact, matter of opinion or concerns – matter of fact is a term in ANT that points to the end point of the process of knowledge production. It also indicates that certainty associated with a fact as indisputable and obvious is an achievement. In this context, a claim taken as matter of a fact stands in contrast with a matter of opinion or concern, which refer to the status of clams that are not taken as facts and therefore are regarded as subjective, not black boxed, and uncertain.

Objective and subjective – opposite poles in the process of knowledge production. During controversy, a claim can gain or lose in strength, becoming more objective or more subjective depending on the direction it goes in its status. Ultimately, subjective refers to a claim that is taken only by a person, according to a matter of opinion, being contingent and not certain. Objective refers to necessity and independence from personal opinion.

Obligatory passage point - point of convergence on a certain topic, question or purpose. It is also a necessary and central element for the formation of a network of associations and actors. The obligatory passage point is associated with a fundamental black box, and is often the point connecting most systems and the greatest number of associations within a network. In this thesis, for example, the Oedipus complex is considered an obligatory passage point because it is a black box capable of aligning a great number of actors in psychoanalysis. The Oedipus complex for example connects the system of psychoanalytic concepts, of knowledge of the unconscious mind, with the system of therapeutic cure of neurosis, and associated notions regarding clinical practice. So the Oedipus complex is a point of convergence of systems of associations such as 'theory', 'cure', as they are both psychoanalytic purposes - and arguably also other systems such as 'psychoanalysts', 'institutions', and so on.

Purification – the work of purification stands in ANT for the process where a division between things and subjects are created, while the hybrid world of associations is rendered invisible. Purification stands for the creation of the world divided in terms of given objects and things, and in contrast with humans (Latour 1993). Purification is a key term in ANT, of a more theoretical nature, in order to question the modern division of subjects versus objects, rendering such a division more of an achievement rather than a given division and a starting point.

'science', science, and sciences – there are different usages of the word science in this thesis. Participants at the Controversial Discussions used the notion of science during the debate. In their quotes or in specific contexts, science was kept as it was used. Now, 'science' refers to my own reference to the usage of the term by others, with the main intention of showing distance from such a usage by participants, while suspending any judgement on the scientific status of psychoanalysis. According to the context, science is also a term used by philosophy of science or ANT and STS. The term sciences, in the plural, refers mainly to the idea that each field of knowledge produces its knowledge according to its own specificities. There is no unifying formula or single process, so it makes sense to speak of sciences in relation to different and multiple activities that scientists engage with in order to produce knowledge. The term sciences also stands in contrast with the idea that there is one single Science.

Spokesperson – he or she who speaks for others, who, or which, do not speak. The spokesperson speaks for example on behalf of things (and therefore becomes those things' representative). This notion is used in this thesis in order to help in the differentiation between discourse analysis and a sociology of associations. With the latter, any participant at the Controversial Discussions is a potential spokesperson since his or her intervention at the debate can be regarded as giving voice and representing what associations have to 'say' in the process of knowledge production in psychoanalysis.

Translation – this refers to the displacements, the establishment of newly formed associations between actors, aligning them according to a shared interest (e.g. fact production).

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