

Interpersonal Rivalries, Gender and the Intellectual and Scientific Making of Psychoanalysis in 1940s Britain

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This article examines the 1940s debates regarding the status and professional orthodoxy of psychoanalysis following Sigmund Freud's death, by exploring the Anna Freud–Melanie Klein Controversial Discussions in the British Psychoanalytical Society. Focusing on the work of now-forgotten analysts Melitta Schmideberg and Edward Glover, and on their relationship with Klein and her supporters, the article reveals how these neglected, yet important, debates were complicated by interpersonal and professional ties, processes of the professionalization, and changing gender norms. Although historians of psychoanalysis have not ignored the jealousies, resentments, and complex relationships between psychoanalysts, these scholars often continue to view these as separate from the processes of creating science. Here, instead, I view the personal and the intellectual in tandem, thus challenging the divide between scientific reason and affect. Rather than imposing a separation between the scientific and the personal, I suggest that we should explore how historical actors negotiated the divide themselves. Indeed, I demonstrate that the study of interpersonal contexts is an invaluable tool for understanding the development of psychological disciplines.

Keywords: history of psychoanalysis after Freud, Anna Freud–Melanie Klein controversial discussions, World War Two, Britain, Gender and Science

It is by now a well-accepted maxim of the history of science and intellectual history that claims to professional authority, objectivity, and truth are all too contingent and changeable (see, e.g., [Daston & Galison, 1999](#); [Shapin, 1994](#)). Historians have problematized the gendered ways in which disciplines have been shaped by a dialog with what was perceived as amateurish or unscientific (e.g., [Smith, 2000](#); [Winter, 1998](#)). In psychoanalysis, the question of orthodoxy began receiving attention in Sigmund Freud's lifetime, but became urgent after his death at the beginning of the Second World War (see [Forrester, 1997](#), pp. 13–43; [Gay, 1988](#)).¹ In this article, I examine the turbulent psychoanalytic debates in 1940s London known as “the Anna Freud–Melanie Klein Controversial Discussions” (hereafter, “CDs”) in the British Psychoanalytical Society (BPAS). I focus on two important, yet historiographically neglected, participants—psychoanalysts Melitta

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¹ For the purposes of this article, I define “psychoanalysis” as a science in the broadest sense, that is, as a discipline aiming at methodical discussion and observation of (what analysts see as) human nature, with the purpose of producing claims that are objective and universally true.

Schmideberg and Edward Glover—and on their relationship with Klein and her supporters. During the discussions, central issues of the discipline were at stake. These included the legitimacy of differing approaches to psychoanalysis, the nature of psychoanalysis as a science, and the meaning of psychoanalytic orthodoxy after Freud's death. These debates, I will show, were complicated by a dense interpersonal network of familial and professional ties, processes of the professionalization of the discipline, and changing gender norms. The fact that the major trio of players in the debates included a mother (Klein), a daughter (Schmideberg), and possible lover (Glover) makes those relationships all the more intriguing. Interpersonal interactions, I argue, are an important engine in the development and refinement of intellectual ideas.

It is only recently that historians of science and the family have begun to examine how personal affinities have shaped scholarly work. Deborah Coen, for example, has innovatively explored the ties between familial and scientific worlds in the turn-of-the-century Viennese Exner family (Coen, 2006; see also Algazi, 2003; Browne, 2002; Coen, 2014; White, 2002). Similarly, historian Emily Levine (2011) has called attention to the bearing of married life on the 20th-century scholarship of Erwin and Dora Panofsky in Germany and the United States. Historians of emotions, too, have researched the emotional side of knowledge production (e.g., Hayward, 2009; White, 2009a). They showed that although science presented itself to the public as dispassionate, emotions of both scientists and students were influential. They have also argued that the cognitive and the affective work codependently in the production of knowledge, the formation of judgments, and the making of meaning. Thus, they questioned the mythology of science as emotionally neutral (see White, 2009b).

Building on these works, I make a different point, arguing that interpersonal rivalries, animosities, and petty quarrels are equally important conditions influencing the production of knowledge; they are not external or irrelevant, but rather constitutive, in the making of science. Indeed, they serve, as a powerful and fruitful instrument for the advancement of ideas.² Scholars of the history of psychoanalysis have not ignored what some see as the more “gossipy” aspects of the discipline, that is, the jealousies, resentments, and complex emotional relationships between analysts. Biographers, especially, look at both the “life and work” of famous psychoanalysts. However, although such scholars often describe their protagonists' resentments, most continue to isolate them from the “great ideas” produced by psychoanalysts. In their efforts to protect the discipline's validity, such writers examine interpersonal relations but do not use them as a legitimate tool for historical analysis (e.g., Dyer, 1983; Gay, 1988; Rodman, 2003). Here, instead, I view the personal and the intellectual in tandem, demonstrating that animosities and niggling innuendos are as important to the development of ideas as much as any other context, and that the heated back-and-forth discussion forced participants to reformulate and hone their ideas in ways they otherwise would not have. Rather than draw a line between the scientific and the personal, we should ask how historical actors mapped this divide themselves, trying to keep the two separate in the politics of their dispute (see Coen, 2006, p. 398).

In my case study of the CDs, British psychoanalysts in the 1940s understood their profession as a hermeneutic discipline of interpretation, but also as a field of science that aspires toward objectivity and truth.³ Indeed, psychoanalysis at the time was infused with a fantasy of objectivity, in which the “Great Psychoanalyst” was supposedly detached from personal circumstances. Psychoanalysts hoped to offer interpretations that are universally true, existing beyond the individualities that developed them. Though they

² See also Shapira (in press).

³ On later philosophical debates on this question by Paul Ricoeur, Jurgen Habermas, and others, see Fusella (2014).

used their scientific selves to understand their patients, they claimed impersonality in their views, aiming to exclude human influence from their theoretical arguments (White, 2009a).⁴ Because psychoanalysts worked not in laboratories but in clinics, in close proximity to their scientific objects (that is, their patients), and because the status of psychoanalysis was challenged at the time, they were particularly careful to claim impartiality in their scientific statements.⁵ Psychoanalysts attempted to rise above hostile personal and familial relationships—and yet, as I demonstrate, their ideas evolved in relation to them. Petty quarrels were of central importance in the development of knowledge in psychoanalysis and are to be taken seriously if we are to fully understand this profession. This insight has implications beyond psychoanalysis, as it allows us to address and trouble the notion that the scientific, investigating self can be detached from personal relations.

Despite being of key importance to modern intellectual history and the development of psychoanalysis, the archival records of the CDs have hardly been analyzed by historians.⁶ Indeed, studying these dense accounts is no small task.⁷ The records contain innovative ideas, yet often read like a thriller, filled with gossip and intrigue, laced as they are with harsh words of discord. The records reveal that the development of psychoanalytic thought was tied to highly personal conflicts, prompting rival analysts to invoke shifting definitions of “science” in their efforts to condemn their opponents and argue for their own views.

As early as the 1910s and 1920s, Jewish, Central-European, lay psychoanalysts Anna Freud and Melanie Klein pioneered two different models for child psychoanalysis. Anna Freud worked in Vienna after being introduced to psychoanalysis by her father, Sigmund Freud, who was also her analyst. Klein, meanwhile, was an autodidact (encouraged by analysts Sándor Ferenczi and Karl Abraham), working first in Budapest, then in Berlin. Klein’s first paper (Klein, 1920) was based on the treatment of her first child patient, her own son Erich (it was assumed she had also analyzed her daughter Melitta, but as scholar Claudia Frank [2009, p. 19] has shown, this was not the case). Beginning in the late 1920s, Anna Freud and Klein began disagreeing over how deep child psychoanalysis should go, the necessity of an educational goal, and the nature of the relationship between child and analyst. Klein immigrated to Britain in 1926, where she enjoyed the support of many BPAS members (Grosskurth, 1986; Meisel & Kendrick, 1990), whereas Anna Freud arrived in 1938 as a Jewish refugee fleeing the Nazis, with her father and some family members and colleagues (Young-Bruehl, 1994). With both Klein and Anna Freud on British soil, and following Sigmund Freud’s death, the tension between the two escalated dramatically. Their theoretical differences were intensively debated between British and refugee psychoanalysts and reached a collision in the wartime CDs.

The CDs are often seen by psychoanalysts as a metaphorical war between Sigmund Freud’s “two daughters”—one of them his biological daughter and the other a potential intellectual heir—fighting over who would continue the work of psychoanalysis. Yet the CDs were also a war between Klein and her own daughter, Melitta Schmideberg (an

⁴ The objectivity of the analyst in the clinic (but *not* in theoretical papers) was challenged early on in the work of Sándor Ferenczi (see Forrester, 1997).

⁵ The interwar press attacked the Society’s legitimacy, as it included both medical and lay members, but with President of the BPAS Ernest Jones’s help, in 1929, the British Medical Association recognized that psychoanalysts are legitimate and separate kind of practitioners (Report of Psycho-Analysis, 1929).

⁶ On the reception of psychoanalysis beyond the CDs, see Rapp (1988), Richards (2000), and Shapira (2013).

⁷ These records have been assembled by King and Steiner in a 1,000-page book, *The Freud–Klein Controversies, 1941–45* (King & Steiner, 1991). Comparing the records in the book with the notes in the BPAS Archives reveals that King and Steiner have done a superb job, and I therefore use these published archival sources. See also Grosskurth (1986, pp. 279–362) and Young-Bruehl (1994, pp. 266–269).

analyst herself), as well as between Klein and other BPAS members. These aspects have been neglected in the limited existing literature on the CDs, which mostly focuses on Klein and Anna Freud (Aguayo, 2000; Donaldson, 1996; Steiner, 2000; Viner, 1996; Vorus, 2003). My goal in this article, therefore, is also to explore the role of those leading the opposition to Klein's ideas during the CDs, namely, Schmideberg and Glover, as the existing scholarship has not fully explored the multifaceted nature of the controversies. Both Schmideberg and Glover were early supporters of Klein. But in the mid-1930s, during Schmideberg's analytic treatment with Glover (prominent analyst and possibly her lover), they turned away and formed an alliance against Klein. I show how Schmideberg and Glover had a central role in intensifying the theoretical tensions, while also turning the discussions to highly personal, often offensive lines, thus endangering Klein's livelihood and reputation, forcing her and her followers to defend the legitimacy of her ideas.

Klein was upset by her daughter's criticism, viewing it as a "pathological" lashing out against her as a mother, not as intellectual development. Klein's accusation gained traction, and the view of Schmideberg as mentally ill ultimately doomed her to obscurity.⁸ Glover, once an influential analyst, second only to Ernest Jones the President of the BPAS, was similarly discredited and forgotten (Roazen, 2000). The marginalization of Schmideberg and Glover, then, was tied to the outcome of the CDs, meriting close examination. The common assumption is that when the CDs ended, members compromised and agreed on separate training paths for Kleinian, Freudian, and "independent" analysts, thereby preventing the Society from splitting into separate institutions. This view obscures the fact that this compromise had victims: Schmideberg and Glover paid the price and eventually had to leave the BPAS. This had implications for the development of the field, as potential theoretical trajectories posited by the two were taken off the table.

Were the heated debates unique to the field of psychoanalysis? I follow George Weisz's (1975) suggestion that sectarian tendencies develop among different schools of science, and not only psychoanalysis, under certain conditions. These conditions include the utopian nature of the discipline, the marginalized social status of members of the scientific school, a real or perceived hostile reaction to the discipline from the outside, leaders of high status, and intense relationships between master and students. The early psychoanalytical movement developed precisely under these conditions, which produced an inclination to sectarianism. Psychoanalysts believed their discipline could offer the world a great truth and felt criticized by the scientific establishment of their time. Moreover, founder Sigmund Freud inspired loyalty to the cause. He served as a both mentor and analyst to many members of the movement, who became dependent on him, both financially and emotionally. But as the 20th century progressed, and psychoanalysis expanded beyond his direct influence, the discipline grew slightly more tolerant of a wider range of interpretations.

In inter war Britain, psychoanalysis enjoyed popular acceptance, and the BPAS had some independence from Freud. It was also not predominantly Jewish, and was less isolated, with members maintaining ties with the diverse professions in which they were trained. Yet, especially after the Jewish refugees joined in the 1930s, analysts in Britain remained a close-knit group, entangled in a volatile web of personal, familial, and specialist affiliations. The very nature of their training through personal analysis only aggravated the members' tendencies toward obedience or rebellion against their mentors. Family relationships and professional-political loyalties burdened the atmosphere in the BPAS. However, as Weisz (1975) notes, tolerance did grow in the 20th century. The development of competing scientific values served to moderate tendencies toward exclusivity, reverence for the master, and passionate interrelations among individuals. Weisz admits that at the same time, a scientific ethos of objectivity can paradoxically exacerbate

⁸ On Schmideberg's work up to the CDs, see Shapira (in press).

tensions and ultimately lead to explosive debates. However, overall, he argues, the scientific value placed on objectivity prevented psychoanalytic doctrine from deteriorating into group dogma. In the CDs, we shall see, the discourse of science indeed both aggravated and relieved existing tensions (Weisz, 1975).

The CDs ended, then, with a dramatic turn of events, in which Glover and Schmideberg lost all power and the Society successfully stayed together. The debates, I show, were concerned not solely with ideas, but with the very discipline of psychoanalysis, its boundaries, and its future. However, arguments about defining scientific objectivity were entangled with personal disputes and antagonistic dynamics. Although the Second World War created the circumstances facilitating the debates, it also set their tone, with participants using contemporary wartime terms and dilemmas as metaphors in their argumentation. In a certain respect, the volatile debates were a microcosm of the war raging beyond the Society's walls. By exploring these arguments, this article demonstrates how psychoanalysis was formulated through theoretical debate, personal intrigue, and historical circumstance. I will shed new light on the CDs and their place in the history of psychoanalysis.

1927–1941: Things Escalate Before the CDs

In order to understand the CDs and to situate Schmideberg and Glover's views vis-à-vis Klein and Anna Freud, some background is necessary. In the interwar years, Anna Freud and Klein advanced competing visions of child psychoanalysis, both taking Sigmund Freud's (1909/1955) essay *Little Hans*, the first known analysis of a child, as their starting point.⁹ Both women believed they were building on Freud's views, yet they each made different scientific assumptions about psychoanalysis, the child's mind, and the suitability of children for analysis. Klein believed that child analysis cannot be educational or normative, as this would not be true analysis. The Oedipus complex, which she dated 2 years earlier than Sigmund Freud, should be deeply analyzed and conveyed to children, without fearing for the fragility of their superegos. Anna Freud, a teacher with ties to left-leaning pedagogues, quickly grew critical of such views. In 1927, she attacked Klein in her book (A. Freud, 1927/1974), offering her own techniques of child psychoanalysis in opposition. That same year, Klein and her British supporters—including Glover—published a series of articles in retaliation, expressing their disagreement (Klein et al., 1927). The exchange was confrontational. Anna Freud appreciated Klein's invention of analyzing child's play, but believed Klein went too far by equating child's play with adult free association, and was critical of her wish to find complex symbolic meaning behind every play gesture. Unlike Klein, Anna Freud argued that children, still preoccupied with their parents, cannot form a transference bond with analysts. Moreover, because she believed children do not yet have strong superegos, analysts must also educate, and not give free reign to all drives (A. Freud, 1927/1974). Klein strongly objected to this approach. In Klein's view, child's play must be thoroughly analyzed in order to discover unconscious conflicts and negative emotions. Unlike Anna Freud (1927/1974), Klein believed children establish strong transference to the analyst (Klein et al., 1927). Like other Viennese analysts combining corrective pedagogy with psychoanalysis, Anna Freud mostly related children's suffering to an environment unable to meet the emotional needs as infants. By contrast, Klein paid less attention to an unloving environment, focusing instead on the child's anxieties, and good and bad imagined "internal objects" (Grosskurth, 1986; Makari, 2009, pp. 427–431; Viner, 1996; Young-Bruehl, 1994).

⁹ The indirect analysis took place mostly through the boy's father meeting with Freud (see S. Freud, 1909/1955). Hermine Hug-Hellmuth was the first woman analyst to treat children.

These different views developed in a dialectic, combative exchange, which was also unequivocally personal in nature. These 1927 discussions were portrayed by Klein and Freud as an objective debate, but as Russell Viner (1996) argued, the language they employed reveals how their authority and their criteria for the legitimacy of knowledge was based on the supposed experience of the analyst, their adherence to Freudian orthodoxy, and the perceived personal soundness and integrity of the theorist. In other words, personal accusations mattered. Criticizing Klein, Anna Freud argued that only practical experience would settle their theoretical arguments. But Klein sardonically replied by emphasizing her extensive experience, contrasting it with Anna Freud's. In fact, Anna Freud admitted that she did not follow the rules of adult psychoanalysis, but wished to augment them so they would fit what she saw as the immature and dependent nature of the child's mind. This allowed Klein to attack her for departing from Freudian orthodoxy, and to present herself as the true translator of adult method. Despite the use of orthodoxy as a weapon against Anna Freud, Klein was ultimately unable to settle the debate in her favor, because her ideas about children were perceived as too radical. However, in order to further cast doubt on Anna Freud's views, some of Klein's supporters (like Ernest Jones and Joan Riviere) brought up her personal history, claiming that Anna Freud's incomplete treatment with her father led her to fear the full exploration of infantile neurosis. Klein herself believed that Anna Freud completely misunderstood the nature of psychoanalysis by believing that wicked instincts might be strengthened by being made conscious (a notion directly opposed to the words of Sigmund Freud, who nevertheless supported his daughter for personal reasons; Viner, 1996).¹⁰

The rhetorical use of personal accusation to discredit opposing views in the debates and the reliance on personal alliances escalated after 1927 by the new actors, Schmideberg and Glover. Their acrimonious relationship with Klein and the Kleinians played a major part in the CDs.

Born in Vienna, Melitta Schmideberg was introduced to psychoanalysis by her mother in Budapest. Melitta studied medicine and psychoanalysis in Berlin, where she also met her partner, analyst Walter Schmideberg. As anti-Semitism rose in the continent, the Schmidebergs moved to London in the early 1930s and joined the BPAS. Melitta was an active member and a prolific writer on psychoanalysis, often supporting Klein's work. However, in 1934, she sought psychological and professional independence from her mother and began analysis with Glover, who had been a member of the BPAS since 1921, one of its earliest members. Glover was also the leading personality at the Institute for the Scientific Study of Delinquency (ISTD), where Schmideberg worked. There, they advocated psychoanalytic approaches to crime in public, professional, and state forums. The analysis of Schmideberg by Glover turned both of them decisively against Klein.¹¹

In Schmideberg's case, the seeds of independence from Klein can be traced back to the time before she met Glover. For example, Schmideberg's early work on child psychoanalysis can be placed between Klein and Anna Freud's competing views: Like Anna Freud, Schmideberg analyzed the actual mother-child relationship, yet overall she was closer to Klein, believing that therapy must directly address deeper inner dynamics in full. Similarly, in Schmideberg's work on delinquency, she was among the leading ISTD members to popularize the idea that environmental factors were important, but that asocial behavior was critically linked to psycho-internal dynamics—here again, Schmideberg situated herself between Klein and Anna Freud, but still remained closer to Klein's ideas (Shapira, *in press*). After forming a personal alliance with Glover, however, starting in 1935 and continuing to the beginning of the war, Schmideberg began to strongly criticize Klein's views and personality. This critique led her to develop novel contributions to the

¹⁰ See also Jones letter to Freud, May 16, 1927, in Paskauskas (1993, pp. 617–618).

¹¹ For the full discussion of her pre-CDs work, see (Shapira, *in press*).

ongoing discussions about the new discipline. For example, in opposition to Klein, who believed reassuring patients prevents the emergence of anxieties and full analysis, Schmideberg called for analysts to comfort patients. She even went as far as to lend money, books, and cigarettes if she felt this would help the analysis. Contesting Klein and her followers, Schmideberg wished to demonstrate that analysis is not “sacred,” but rather a more flexible, humane encounter. She thus developed a technique that was less strict and more empathic (Schmideberg, 1935). Schmideberg also argued against psychoanalysis becoming “a new religion,” and the notion that people should be “fully analyzed,” an idea she mocked and believed Klein wrongfully encouraged. For Schmideberg, this notion reflected a Kleinian delusion of grandeur. The goals she preached were less utopian and more modest, and ultimately remained unnoticed by scholars. Alluding to Klein’s limitations as a mother, Schmideberg added, “Some analysts seem to assume as a matter of course that analyzed parents are also the best parents. This is definitely not the case” (Schmideberg, 1938, p. 138). By the beginning of the Second World War, then, the gloves were off; armed with a scornful tone and personal accusations, and encouraged by her alliance with Glover, Schmideberg began arguing against some of the sacred tenets of Kleinian psychoanalysis, offering important theoretical innovations of her own (Shapira, *in press*). The personal and the intellectual were thus closely tied in her case. The same was becoming true in Glover’s case: “Edward Glover and I had agreed to ally and to fight [Klein’s ideas]” (Schmideberg, 1971, p. 63), she retrospectively confessed.

Initially, after Klein immigrated to London, Glover vigorously supported her work as complementary to, rather than incompatible with, that of Sigmund Freud. Klein’s ideas on childhood phantasy and the preoedipal period, Glover believed, added fine detail to Freud’s schematic structural theory about the id, ego, and superego. Glover then worked to offer a rapprochement between Sigmund Freud and Klein (Aguayo, 2000, 2002). In 1929, he argued that because Freud stressed the resolution of the oedipal complex, along with the structuralization of the superego in latency, Klein, too, could deal with the inception of the superego and postulate a maternally driven oedipal complex in the first years of life (Aguayo, 2002; Glover, 1930). In 1933, Glover even extolled Klein’s (1932) book *The Psychoanalysis of Children*, writing, “I have no hesitation in saying that it constitutes a landmark in analytic literature worthy to rank with some of [Sigmund] Freud’s classical contributions” (Glover, 1933, p. 119). However, even before meeting Schmideberg, Glover was not a blind follower of Klein. He presented light critique of her book, for example, claiming she was not scientific enough in her terms, and that she had not fully theorized her observations of children (though he reassured readers that such matters would be settled “in the ordinary procedures of science”; Glover, 1933, p. 127). Though Glover showed impatience with the overlaps in her writing, he commended her for the footnote apparatus in her book (Glover, 1933, pp. 123–129). However, as Joseph Aguayo (2002) proposed, Glover’s overall sympathetic efforts at theoretical appeasement between Klein and Sigmund Freud were ill-timed, occurring just as the differences between Klein and Anna Freud emerged. In 1934 to 1935, Klein’s work advanced into a new theoretical phase, and she spoke of developmental “positions,” causing Glover to withdraw his support. As Klein’s new model of psychological life pushed Sigmund Freud’s structural model to the sidelines, Glover’s mediation attempts, which were based on this model, became irrelevant. Unlike Schmideberg, he did not fully formulate his critique of Klein in print until after the war. Moreover, he continued to employ a Kleinian viewpoint, in his work on crime being an example, in which he stressed the importance of inner reality (Aguayo, 2002; Glover, 1945; Shapira, 2013).

Glover’s personal alliance with Schmideberg fueled his theoretical turn away from Klein. Thus, by 1935, he suddenly became convinced that Klein was a pseudoscientific “deviationist.” Referring to her emphasis on the importance of phantasy in the first year of life, he accused her of advancing a new worldview that began with a

“mystical interpretation of life immediately after birth” (Aguayo, 2002). He used personal accusations against Klein, revealing his power struggle as a male doctor aiming to discredit a lay woman analyst. As a medical man, he stated, he could no longer accept Klein’s lay theories as scientific and compliant with Freudian orthodoxy. In Britain, lay candidates were encouraged by Jones to undergo medical training, but they were nevertheless welcomed by the BPAS. Even so, as a lay analyst, Klein was vulnerable to such claims. Revealingly, Glover did not dare make the same accusations against Sigmund Freud’s daughter, who was also a lay analyst (See Shapira, 2013, pp. 8, 19, 21).

Indeed, interestingly Glover and Schmideberg might have formed a coalition against Klein with Anna Freud. However, this did not happen. I agree with Aguayo’s (Aguayo, 2002) argument that, in the polarized atmosphere of the BPAS, old alliances were not easily forgotten. Despite their later convergence of views, Anna Freud never overcame her earlier distrust of Glover as a Kleinian enthusiast. This could also explain her reluctance to form an alliance with Schmideberg. Glover, once prominent in the BPAS, was rapidly losing credibility among the Viennese, while also no longer supporting Klein and her followers. He thus found himself in “an organizational and institutional ‘no man’s land’” (Aguayo, 2002, pp. 131–132). Glover resigned in 1944, but not before he and Schmideberg launched a full personal-theoretical attack on Klein during the wartime CDs, turning their fellows into adversaries. Their accusations directly accelerated the need to elucidate, advocate, and refine Kleinian dogma.

The Wartime Debates and the Schmideberg-Glover Attacks

The CDs were a battle of ideas under real wartime fire. Was Klein’s work a legitimate continuation of Sigmund Freud’s endeavors, a sign of growth of the field? Or rather (to use analyst Marjory Brierley’s metaphors), was Klein merely a poor geographer who, in her delight in mapping fresh territory, threw away all other maps and therefore could no longer be considered Freudian? (King & Steiner, 1991, pp. 925–926) What are the fundamental tenets and scientific standards of psychoanalysis? Can lay women develop the discipline as well? Can analysts venture in directions Freud did not anticipate? Interpersonal relations played a profound role in the discussions surrounding these issues. The CDs were also about the administrative and public nature of the BPSA. Many Society members felt that Jones (then semiretired) and Glover (effectively replacing him), representing the Society’s medical men, held too many powerful leadership positions. Between 1941 and 1944, the disagreements were addressed in meetings, papers, private letters, and discreet phone calls. I will show how the discussion of the scientific validity of ideas was not detached from personal preoccupations. In order to scientifically discredit an opponent, analysts called into question the psychological and professional credibility and soundness of mind of individuals. Indeed, scholars have ignored the fact that these highly personal accusations led to the writing of some of the key theoretical papers in 20th-century psychoanalysis (e.g., by Susan Isaacs; King & Steiner, 1991, pp. 264–321, 501–532). These CDs papers were written “on the defensive,” clarifying ideas in order to stave off allegations. Acrimonious rivalries influenced the formulation of the definition of science, forcing opponents to articulate their ideas in ways they would not have, had it not been for the personal war waged by Schmideberg and Glover.

During the Blitz, many psychoanalysts were evacuated to the countryside or enlisted in the army, and Klein herself evacuated to Scotland. Glover and Schmideberg, meanwhile, stayed in London with the Jewish refugee psychoanalysts who, as enemy aliens, were not allowed to leave town. Glover wryly noted that thanks to the war, members who stayed

in London had a “very pleasant time through the Blitz” (Grosskurth, 1986, p. 284)¹²; Klein’s absence, he thus suggested, made shelling appear like a negligible nuisance. And so “a strange peace descended on the [Training] Committee” at the Society. With the Kleinian representatives absent, Glover recalled, discussions were “correct and friendly,” with no one contradicting him or Schmideberg (both quotes are from Grosskurth, 1986, p. 303).¹³ As the Blitz went on, Klein realized that although she was physically safe, her professional position was in peril, and she returned to London. As Klein’s biographer aptly put it, “Melanie Klein might have been afraid of German bombs, but she was still more afraid of the invasion of German-speaking analysts and their occupation of her territory” (Grosskurth, 1986, p. 281).

The conflict officially escalated in the winter of 1941, when Melitta Schmideberg, along with Barbara Low, Adrian Woolf (Virginia’s brother), and Karin Stephen demanded an “Extraordinary Business Meeting” to discuss the atmosphere in the BPAS and to examine the legitimacy of Klein’s ideas (King, 1991, p. 34). The First Meeting was held on February 25, 1942.¹⁴ The Society thereafter met for seven more Extraordinary Meetings in the subsequent 2 years. These meetings led to a set of 10 “Discussions of Scientific Controversies” from January 1943 to May 1944 (in effect, “the Controversial Discussions”) to discuss Klein’s work (King, 1991). By introducing personal arguments against Klein, Schmideberg and Glover led to a polarization of the conflict. Though some of Schmideberg’s and Glover’s statements provoked outrage and were refuted by members, they lent momentum to the debates against Klein, leading to their escalation.

The Extraordinary Business Meetings

Discrediting rivals by attacking personal behavior. Throughout the Extraordinary Business Meetings, analysts denounced their rivals’ personal behavior as a way to theoretically discredit them. In the Second Meeting on March 11, 1942, Schmideberg wove personal, theoretical, and scientific concerns together as she attacked Klein’s ideas, personality, and leadership, accusing her mother and her supporters of creating a Society with little freedom of expression. Schmideberg conveyed her disagreement in a sneering tone, accusing specific Kleinians of mistreating those who disagree with them. She cited examples of members and candidates who had allegedly been mistreated by the Kleinians, who, in her opinion, had “an admirably organized co-operation” and held the power to “make or wreck” members’ practice and reputation (King & Steiner, 1991, p. 95). She claimed that the “Kleinian clique” started a “crusade” against member Nina Searl, aiming to discredit her after she made contributions that did not comply with their own dogma. Schmideberg was outraged that a leading Kleinian even suggested that Searl and her husband should leave the country for daring to protest against the Kleinian “intrigues” (King & Steiner, 1991, pp. 93–94). “To sum up: it is sufficient to say that every Member who was not 120 per cent Kleinian has been attacked systematically, directly or indirectly,” she wrote (King & Steiner, 1991, p. 94). Schmideberg (1938) argued that Kleinians wanted analysts to become converts, forcing their opinion on everyone. She thus likened them to a cult, implying that the requirement of blind adherence to Klein as a master was in itself sufficient to discredit her theories.

¹² See also Bluma Swerdloff (1965).

¹³ Once Klein and her supporters were back for the discussions, Schmideberg used this and pettily complained that those who are asking for change were those who left London for the 9 months of bombing and had perhaps rejuvenated in the countryside (Grosskurth, 1986, p. 291).

¹⁴ During the heated meeting, Schmideberg ferociously accused the Kleinians of creating an atmosphere of intimidation within the Society and forming undemocratic “cliques” (King & Steiner, 1991, p. 40).

Glover spoke, too, criticizing conduct as a way to cast doubt on the validity of ideas, arguing that the Kleinian faction in the BPAS used the Society's bureaucratic bodies to manipulate its influence and limit the free exercise of scientific opinion. Before 1930 to 1934, he believed, there was a good culture of discussion in the Society, but afterward, "gossip and criticism" hindered creativity and democratic debate, with Kleinians using majority block votes to influence training in the Society (King & Steiner, 1991, pp. 101–104).

Using "science" to discredit rivals. Indeed, throughout the meetings, the scientific standards of individuals were questioned, and the objective ethos of "science" was employed—often in exasperating tones—as a tool to damage the credibility of foes. In the Second Meeting, for example, Schmideberg criticized the Kleinians on what she termed as "scientific grounds"; she believed that the Kleinians hid behind "ambiguity" and "vagueness" and that "they lack in the most elementary scientific discipline" (King & Steiner, 1991, p. 98). Speaking as a woman with the privilege of a university education, it was easy for her to attack her lay mother. Interestingly, Schmideberg thus sided with male medical members at the Society like Glover and John Bowlby, who used their medical degrees and authority to argue against Klein. They all envisioned science as a field in which emotion is overcome by reason, and truth is made evident to others through clinical data. Yet Schmideberg purposefully cast doubt on this vision of impartiality, stating,

In a manner somewhat reminiscent of Dr. Goebbels, they [the Kleinians] try to impress us by repeating time after time the same slogans, by putting forward exaggerated claims and dogmatic statements, by accusing their opponents and intimidating the hesitants [sic], by a constant play on emotions of every sort, instead of presenting and substantiating their theories according to scientific standards. (King & Steiner, 1991, p. 98)

But these loaded, provocative words should be understood in light of the fact that the contemporary standards of scientific validity in psychoanalysis were very much unclear, and that Schmideberg directed her accusation against the Kleinians alone. Schmideberg's own papers from the time, like those of most other analysts, relied mainly on anecdotes from patients, and were therefore no different from Klein's in either form or reliance on data.

Next, Schmideberg boldly accused her mother of scientific plagiarism, arguing that Klein's contributions belonged to Sigmund Freud, Abraham, Ferenczi, and others (King & Steiner, 1991, p. 95). Schmideberg's husband, Walter, similarly claimed that Klein's ideas belonged to Felix Boehm (King & Steiner, 1991, p. 86). These seemingly "purely scientific" accusations were obviously not personally unbiased, as twice in the discussion Klein asserted she had referenced the works, only for Walter Schmideberg to counter that her followers presented these ideas as hers, and that she therefore "should beware of her friends, with her enemies she will be able to deal herself" (King & Steiner, 1991, p. 105).

Setting out in the Fourth Meeting, May 13, 1942, to further substantiate her accusations that her mother had committed plagiarism, Schmideberg's arguments reveal how inexorably linked personal and theoretical-scientific matters were in psychoanalysis. The references in Klein's (1932) book were "reasonably accurate," according to Schmideberg, but this was only because Glover had read the manuscript, noting which author to quote, whereas Schmideberg herself compiled the bibliography and index. In Klein's later articles, claimed Schmideberg, Boehm is not mentioned, giving the impression that his claims were Klein's (King & Steiner, 1991, pp. 150–151). Anna Freud then intervened and argued that it is naïve to believe that when one is convinced of one's views, only legitimate methods will be used. It is inevitable, she continued, that the practical consequences of this for candidate training should be

reviewed, even if all people concerned behave well (King & Steiner, 1991, pp. 154, 156). With this statement, it is clear that for Anna Freud, too, scientific views were tied to personal dynamics.

Glover, too, used “science” to question Klein’s ideas. In the Second Meeting, he again drew a distinction between destructive gossip and manipulation and the productive functioning of a scientific community working to establish commonly held views as truth. He belittled Klein based on her gender, again reducing her work to female “gossip and manipulation” (King & Steiner, 1991, p. 145). However, it is clear that with their passionate accusations, both he and Schmideberg were intensifying the contentious atmosphere within the Society, in quite the reverse of their declared aims of keeping the discussion impartial. Indeed, their claims in the name of science were motivated by personal vendetta, and were expressed with highly emotional language, causing a commotion.

Klein was not alone in finding such accusations unsettling. Schmideberg’s and Glover’s words were perceived as outrageous by several other BPSA members, who, in turn, brought up their sarcastic and aggressive behavior in an attempt to discredit their claims. At the Third Meeting on April 15, 1942, Sylvia Payne, the Honorary Secretary of the BPAS, a moderate who valued contributions of various theoretical orientations (King, 1991, pp. xvii–xviii), reprimanded them for their strong words and unreasonable accusations, accusing them of relying on “gossip and indiscreet comments,” as she called them, directly reversing their charges. Payne censured Schmideberg’s naming of members without their consent and charged her of wrongfully accusing the Kleinians of what could legally be named “malpractice” (King & Steiner, 1991, pp. 109–110). Payne instead called for constructive efforts and professional behavior (that is, behavior that excludes personal preference and emotion).

Joan Riviere, too, criticized Glover’s and the Schmidebergs’ behavior and style, and was infuriated by the derogatory remarks made about Klein’s personality and behavior. Riviere, a supporter of Klein, called for cooperation in scientific work, and compromising to bring peace to the Society. Riviere believed that the Schmidebergs and Glover had stirred up “hostile personal feelings, and stimulate[d] and prolog[ed] prejudice, rancor and feuds” (King & Steiner, 1991, p. 113). She called Melitta Schmideberg’s claim that the Society is ruled by a small clique “a fantastic accusation” and strongly condemned accusations of plagiarism. She stated that Glover and Schmideberg had no evidence for their bold accusation that Kleinians manipulate the hidden politics within in the Society to strengthen their cause. Instead, she reminded the Society that convincing others of the truth of one’s conclusion is an important part of science. Matters should be conducted, she said, more impersonally and based on their merits (King & Steiner, 1991, pp. 113–119). Both sides of the CDs, then, bolstered their position with the claim that it was “in the name of science,” while the opposing side was unprofessional and unscientific. Subsequently, the Society struggled to deal with this volatile dynamic. During the discussion, analyst Donald Winnicott even said, “I am appalled by the situation. I cannot believe that we have so spent all that time on such matters, especially in wartime.” He then asked Schmideberg to withdraw her statements, but she refused (King & Steiner, 1991, p. 128).

On July 23, 1942, Klein circulated a memorandum in which she provided her own definition of “scientific progress,” writing,

As long as this Society has any claim to be called a scientific Society there must be disagreement, and what Dr Glover calls controversy, between the Members; though it need not be embittered by personal animus. A scientific Society is a group of people who are seeking to discover new truth, not a group who

are resting content at any stage in the search for knowledge and are satisfied with that. (King & Steiner, 1991, p. 204)

Discrediting the soundness of rivals: Style and content. Tying together the personal and the theoretical, the two sides not only used the notion of science as a means of rebuttal but also questioned the psychological soundness of their rivals. They tackled theoretical differences by attempting to destroy the reputation of individuals. Klein, for example, criticized Glover's supposedly innocent scientific accusations against her, by addressing his personal psychology. Writing a letter to Payne on March 16, 1942, Klein claimed that Glover was a man "jealous of the successful intellectual, i.e., rival woman" (Grosskurth, 1986, p. 295) and was tragically driven by Schmeideberg. Later that month, Payne wrote back, suggesting that Glover feared he would not be the next President of the Society and would lose all other positions, pointing at the possible political motive behind his theoretical ideas (Grosskurth, 1986, pp. 296–297). Payne thought that the Schmeidebergs' fault-finding behavior alienated people who might otherwise be friendly to their cause. She hinted that Jones had made an error in judgment in bringing Glover's relations with Melitta to the attention of the training committee, suggesting that "the affair could have been dealt with privately" (Grosskurth, 1986, p. 297). Klein's biographer, Phyllis Grosskurth, wrote that Payne might have been implying that Glover and Schmeideberg were having a love affair (Grosskurth, 1986, pp. 296–297). However, scholar Pearl King disagrees, arguing that Payne was suggesting that Glover's psychoanalytical treatment of Schmeideberg had not been thorough, therefore causing her public aggression against her mother (Grosskurth, 1986, pp. 297, n*).

In his capacity as the President, Ernest Jones wrote a letter to Klein on April 14, 1941, noting the unrestrained behavior of the Schmeideberg–Glover alliance. He believed that Schmeideberg's influence on Glover "was the more powerful of the two" (King & Steiner, 1991, p. 229). Nevertheless, Jones still saw Glover as his only potential successor. Klein replied to Jones, describing how upset she was. She feared the problems created by Glover and her daughter would have devastating effect on her work, and that

whether or not Melitta's influence over him was the more powerful of the two, undoubtedly it is he who is the much more dangerous of the two because owing to his skill, better tactics and various other circumstances he is successful in his consistent endeavors not only to harm the new work in which I have a share but to bring about a regression in psychoanalysis which goes far beyond the time when this work started. (King & Steiner, 1991, p. 230)

Thus, Klein presented Glover as a danger not only to herself but also to scientific progress in the discipline. By citing this loftier goal, she created sympathy for her cause, dismissing Glover's views as politically motivated and biased.

Klein explained that although she would defend the legitimacy of her ideas, she did not like doing so and added, "The fact that my daughter is one of my main opponents has a bearing on this wish not to fight" (King & Steiner, 1991, p. 232). This must have been a difficult time for Klein. Not only had her daughter turned against her—the Society had shifted away from its earlier support of her work, with members now split between her work and the work of Anna Freud (King & Steiner, 1991, p. 232). Rather than recognize Schmeideberg's words as possibly legitimate critique, Klein considered the best tactics to deal with her daughter's hostile accusations. As in Glover's case, she raised questions about the soundness of Melitta's judgment in her personal life and, by extension, in matters of theory. Klein wrote to her supporters on April 15, 1942:

One can certainly generally refer to the sharpness of her [Schmeideberg's] *criticism* of younger members and of people in general, *up to the time she turned against me*. Another of her victims was Anna Freud

at the Congress in Lucerne in the summer, 1935, and I am quite sure that Anna Freud *remembers* this paper *very well*—and that is certainly a point that can be mentioned [that is as something to Schmideberg's attack]. (Grosskurth, 1986, p. 297; italics in original)

Thus, Klein used her daughter's expressive language to argue against her theoretical objections. She went as far as to suggest that Melitta was mentally ill. Whether she believed this or was merely trying to refute Schmideberg's claims is not entirely clear. But Klein added,

From one thing I feel very strongly we must refrain, even though it is quite unfair to ourselves—and that is to make any aspersions or accusations which cannot be at once supported by irrefutable facts, not as they appear to us, but as they appear to the others. . . . And there is even one very obvious fact which I feel quite sure *should not be mentioned*, nor even hinted at by any of us, and that is Melitta's illness. I think it must not even be hinted at by any of us either privately or as an argument in the coming discussions. I am convinced, however true it may be, it will be held against us if it is mentioned. (Grosskurth, 1986, p. 297; italics in original)

Interestingly, some of the Anna Freudians, too, perceived Melitta Schmideberg's style as overly subjective, petty, belligerent, unscientific, and, indeed, un-British in its expressive language. Viennese refugee Eva Rosenfeld described her discomfort while witnessing this interaction between Klein and Schmideberg by saying,

At the meetings I could only see something quite terrible and very un-British happening, and that was a daughter hitting her mother with words and this mother being very composed, quite quiet, never defending herself, but having such power in that society, being so powerful that it really didn't matter what Melitta said. (Grosskurth, 1986, pp. 242–243)

Marjorie Brierley, Klein's detractor, thought, too, that Schmideberg's behavior was offensive (quoted in Grosskurth, 1986, p. 306). Klein telephoned Anna Freud on May 1, 1942, reporting in a letter to Payne that Freud was "inclined to regard Melitta's attacks more in the way of a naughty child" (quoted in Grosskurth, 1986, p. 299)

The fact that Schmideberg was Klein's daughter, combined with her hostile style, made it easier for members to dismiss her claims. Some thought that the same was true of Glover. Karin Stephen, initially one of the first to join the movement against Klein, wrote to Klein on July 28, 1942:

Really Glover is behaving like a lunatic, but fortunately people are really, I think, becoming shocked at his lack of straightforwardness and recognize how dangerous it would be to let him stay in power. So, although it must be maddening for you, it is probably a good thing in the long run that he has made these wild accusations! (Grosskurth, 1986, pp. 204–205)

However, as Klein predicted, Glover was harder to dismiss.

Personalized statistics? In the Fourth Meeting, May 13, 1942, the professional–personal battles carried on, as Glover continued his attack on Klein, only to find himself on the defense. He provided statistics—seemingly the ultimate objective tool of science—to show that Kleinians could have a majority block vote, thus ensuring the acceptance of their professional views without adequate scientific discussion (King & Steiner, 1991, p. 137). He denied that his arguments were driven by his "offensive aspirations" or personal "hostility" toward Klein or the Kleinians (King & Steiner, 1991, pp. 137–138). Glover also defended the Schmidebergs, stating that Kleinians should confront their accusations instead of resorting to "martyrdom" and "self-righteousness" (King & Steiner, 1991, p. 142). He sharply added, "Everyone here knows that the standard of professional and scientific discretion in psychological societies is lower than that of

natural science societies or ordinary medical societies” (King & Steiner, 1991, p. 142). Here, again, we see how both sides of the debate employed discourses of science while accusing each other of being wrong.

Payne retaliated with entirely different statistical figures to repudiate the claim that the Society was dominated by Kleinians. This led to an important resolution in the Fifth Meeting: to refrain from “personal attacks.” The Society agreed to Brierley’s suggestion to uphold an “armistice” and affirm the right of all members to “complete freedom of speech within the limits of common courtesy” (King & Steiner, 1991, p. 174). This allowed the more in-depth “Discussion of Scientific Controversies” to begin. In a private letter to analyst Barbara Low, Brierley mentioned her dislike of some members’ usage of the term “Freudian psychoanalysis” to describe Anna Freud’s ideas as opposed to Klein’s. Brierley wrote,

Psychoanalysis is not a creed but a science, and the work of Freud is not a final revelation of absolute truth but a system of hypotheses designed to explain as accurately as possible the working of the human mind as inferred by him from clinical and other psychological data. Freud continuously modified and expanded his hypotheses, his theory evolving parallel with his growth in knowledge. (King & Steiner, 1991, p. 209)

Though Brierley did not support Klein, she did not agree that the discipline ended with Sigmund Freud. If psychoanalysis is to survive, it must continue to develop. She added,

A living science is evolutionary and democratic, not static and authoritarian. . . . It is not a question only of finding out whether irreconcilable differences between Anna Freud and Melanie Klein but rather, whether we can ALL continue to co-exist as members of one Society and, particularly, whether that Society can re-establish its right to be considered a genuinely scientific society. (King & Steiner, 1991, pp. 209–210)

The science of psychoanalysis according to Brierley, then, is characterized by change and the ongoing communal process of establishing the commonly held views which defined it.

War as a metaphor for intellectual dispute. Throughout the meetings, the different parties employed the language of war in discussing theoretical differences, envisioning diverse views as utterly incompatible with one another, instead of looking for scientific common ground. For example, in the spirit of the 1940s fight for democracy in Europe, Schmideberg opened fire (so to speak) against Klein in the Second Meeting, defiantly declaring that no society could exist without a measure of tolerance. By implication, she was arguing that like citizens fighting for a culture of democracy, so should analysts strive to prevent (Kleinian) totalitarianism within the BPAS. She provocatively stated,

Analysts who believe themselves to be tolerant are in practice less so than any group of people I know, with the possible exception of the Nazis. Some persons have the gift to bring out the best in others. Certain Members seem to have the gift to bring out the worst. (King & Steiner, 1991, p. 95)

As another example, Payne, too, compared the war in the BPAS with the Second World War: “The conflict [in the Society] is extraordinarily like that which is taking place in many countries and I feel sure that it is in some way a tiny reverberation of the massive conflict which pervades the world” (King & Steiner, 1991, p. 110).

The Discussions of Scientific Controversies: The Case of Phantasy Between Personal and Theoretical Preoccupations

As the Extraordinary Business Meetings ended and the more theoretical Discussions of Scientific Controversies (or Controversial Discussions) began, Schmideberg played a smaller role, while Glover chaired most meetings and remained active in the opposition

against Klein. Papers on issues such as regression, training, introjection and projection, and phantasy were discussed in exacting detail. I will examine the disagreements on the role of phantasy as a “case study,” revealing the entanglement of personal and theoretical preoccupations. In order to avoid direct confrontation, Klein, for the most part, let her followers defend her ideas. Susan Isaacs, a Kleinian analyst and one of the most famous and influential educationalists of the interwar period, stood in defense of Klein’s notion of phantasy (on Isaacs, see [Graham, 2009](#); [Woolridge, 1994](#), pp. 111–135). Importantly, as mentioned, the personal accusations against Klein drove Isaacs to write what would become one of the central postwar papers of psychoanalysis. Thus, instead of exploring all theoretical viewpoints in depth, I propose a sociocultural history of science and ideas, tracing the evolving, subtle dynamics of an existing hybrid personal–theoretical back-and-forth.

Letters in her archives support the view that Klein treated the discussions as an opportunity to defend and develop her ideas. Writing to her supporters, she aimed at uniting them to protect her from attacks. In a letter to Sylvia Payne on May 31, 1942, Klein complained about the personal tone in the denunciations of her theories:

As it is at present I have been pressed into an extraordinary position in which I can be accused in every way, without being able either to justify myself (because this stirs ill feelings in the Society!) or to be protected sufficiently by the officials. I don’t wish to be melodramatic . . . but I and my friends are now treated like outlaws in the Society; I think you overrate the value of considering people’s susceptibilities, and underrate the important of the truth being stating in this flagrant case. (quoted in [Steiner, 1991](#), p. 263, Note 18)

Klein, then, saw an urgent need to strategize with her allies before the explosive theoretical debates with her opponents. She wrote, on June 27, 1942,

It seems to me that it is of no particular value for us to prove that the others are lacking in knowledge, or go wrong in this or that point, but to show that we have something to convey to them which at least some of them can accept. What counts, I think, is to re-establish our position in the Society in respect to the actual value of our work, and that can only be done patiently by giving them at a time no more than they can digest, and also present it in a way which makes this possible for them. (quoted in [Steiner, 1991](#), p. 247)

Preparing for battle, Klein urged her supporters to refresh their memory of every word Freud had ever written. Notably, she confessed that her own theory “is only partly formulated, and partly it is still in the making” (quoted in [Steiner, 1991](#), p. 247). The personal confrontation with her opponents therefore forced her to further crystalized her ideas. Klein monitored Isaacs’s rebuttals in the debates, instructing her in the best defense tactics (quoted in [Steiner, 1991](#), pp. 245–246).

In her paper on phantasy dated January 27, 1943, Isaacs argued that the primary content of all mental processes is unconscious phantasy. Phantasies, for her and Klein, are the basis of all unconscious and conscious thought processes ([Isaacs, 1943/1991](#), pp. 271–272). Unconscious phantasies are the psychic representation of the life and death instincts, and they begin in a primitive way from the first month of a baby’s life, when she or he experiences the first “good” and “bad” partial object relations. In this paper, we can see how the personal accusations against Klein influenced the manner and style in which Kleinians defended their theories. Isaacs’s entire paper is written in a defensive style of argumentation, hoping to counter accusations, and advocating that the Kleinian notion stems directly from Sigmund Freud’s genius work (she quotes him at length to fortify her position). Isaacs states that the problems Klein explored had been raised, but not expanded on, by Freud. The discussions, she continues, are an opportunity for bringing into focus the relationship between earlier and later theories. Throughout the paper, she confronts the views of other members directly. She even cleverly teases Glover and Schmideberg by

selectively quoting their early work, in which they express strong support of Klein (Isaacs, 1943/1991, pp. 271–272).

The Kleinian view on phantasy was seen as unacceptable by Anna Freud and her followers Dorothy Burlingham and the Hoffers. In Anna Freud's reply to Isaacs in the First and Forth Discussion on January 27 and April 7, 1943, she argued that it was impossible to speak of a proper psychic life during a baby's early months (King & Steiner, 1991, pp. 417–425). Instead, she argued that in the first months of life, babies are mostly concerned with survival, and that libidinal impulses (rather than violently aggressive phantasies) were overwhelmingly important during this time of life. In other words, for Anna Freud, the death instinct had a lesser role than for Klein. Nevertheless, Anna Freud noted that "the underlining difference of opinion does not refer directly to phantasy activity but, partly to dating as before, and partly to divergence of views about instinct theory" (King & Steiner, 1991, p. 424).

Although Anna Freudians were more selective in their attack, harsh, dismissive criticisms of Klein continued to be voiced by Schmideberg and Glover. Further antagonizing other members with his coarse style, in the First Discussion on January 27, 1943, Glover claimed that the Kleinian assumptions are nothing but "unsound" (King & Steiner, 1991, p. 325). Glover mockingly and disrespectfully questioned whether Isaacs's ideas on phantasy in early life were based on facts or perhaps her own—and Klein's—favored interpretations of unconscious content. Glover argued that the women were confusing Freud's "orderly series of concepts" of instinct development. Isaacs instead offered "an array of postulates some of which seem to be regarded by Mrs Isaacs as if there were axiomatic, some of which are simply interpretations—of the 'primary content of all mental processes'" (King & Steiner, 1991, pp. 325–327). For Glover, the Kleinians were building a new, messy metapsychology that reflected a complete misunderstanding of Sigmund Freud's earlier, tidy metapsychology. Glover's tactic of invoking the rule of "adherence to the original master" focused on depicting Klein's innovations as incompatible with Sigmund Freud. Schmideberg was similarly dismissive, saying that because of Isaacs's "blatant omission" of Schmideberg's later work in a manner "contrary to scientific tradition and spirit," it was hard for her to even take Isaacs's paper seriously (King & Steiner, 1991, p. 340).

Isaacs addressed these claims in the Second Discussion on February 17, 1943, by offering a different take on the role of Sigmund Freud in the history and future of psychoanalysis. Freud opened up many possible lines of research, she argued, not all of which can be explored by every psychoanalyst. Notably, his views on the death instinct and his concept of incorporated objects have been ignored or rejected by many. Freud's insight into understanding child's play in the second year of life was similarly neglected. Isaacs believed that Klein responded to these lacunas (King & Steiner, 1991, p. 377). Although Glover contended that only close adherents to Freud's ideas could carry on his scientific tradition, Isaacs believed analysts could and should develop Freud's theories, differing from him when necessary. She was also hinting that although Glover was a man of science, a lay woman like Klein may have some advantage over him, having analyzed children firsthand (King & Steiner, 1991, p. 378). Responding to Schmideberg, Isaacs retorted that she saw no reason not to quote Glover and Schmideberg's early statements, when they so eagerly supported Klein. She teased them again saying that analysts were entitled to change their views, and that "we are likely to do so if our science advances" (King & Steiner, 1991, p. 382). For her, psychoanalysis was a dynamic, evolving science.

The personal tone of argumentation fueled the theoretical claims and served to motivate the further development and defense of ideas. During the Third Discussion on February 17, 1943, Schmideberg questioned the scientific validity of Klein's claims to know the content of babies' phantasies, as Klein drew her conclusions from the analysis of adults. Schmideberg scathingly argued, "To be told 'we found in analysis' or 'the material proved' is no [scientific] argument. . . . Children have no direct memories of infancy . . .

and the ‘constructions’ made by [Kleinian] analysts must be very carefully checked” (King & Steiner, 1991, p. 395). Klein’s ideas, she concluded with disdain, are merely speculations, difficult to prove and to disprove, as it is impossible to observe the unconscious phantasies of babies. Schmideberg conceded their expressions of emotion and actions are observable, “but again we must distinguish strictly between such observations and the speculations derived from them” (King & Steiner, 1991, p. 395). Schmideberg sided with analysts who associated psychoanalysis with the natural sciences, advocating for interpretations based only on data which can be confirmed by others (see Bowlby, Figlio, & Young, 1986, p. 48). Schmideberg insisted that Klein should provide analytic material substantiating her arguments and “reveal” her methods for analysis. It appears that Klein viewed scientific standards differently; her interpretations relied on data and findings, but also on a theoretical model she had developed.

The practice of using the discourse of “science” and rhetorically manipulating the notions of adherence to Sigmund Freud in order to personally discredit opponents persisted. In the Third Discussion on March 17, 1943, Schmideberg insisted, again, that there was no way to directly ascertain the feelings or thoughts of infants, boldly calling Klein’s suggestion “unscientific” (King & Steiner, 1991, p. 394). Schmideberg declared that she strives to substantiate her arguments with empirical observations, drawing conclusions with varying degrees of probability. It was the Kleinians, she claimed, not her, who “do not draw sufficient distinction between empirical observation (analytic or otherwise) and conclusions, and between conclusions of varying degree of plausibility” (King & Steiner, 1991, p. 394).

In the same meeting, Glover claimed that Sigmund Freud’s views on psychic reality could not support Isaacs’s theory of phantasy. Phantasy in Freud’s writing, said Glover, is only an advanced form of dealing with frustrated instinct energy, and therefore appears only as a more advanced development. Freud then distinguished between psychic image presentations (whether associated with reality gratification or hallucinatory gratification) and between any variety of phantasy, whether conscious or unconscious (King & Steiner, 1991, pp. 395–397). Glover believed that this distinction was lost in Isaacs’s work. According to him, she was abolishing the distinction between the perceptual aspects of consciousness, the imaginal aspects of consciousness, the preconscious, and the unconscious (King & Steiner, 1991, p. 398).¹⁵

On April 9, 1943, Klein wrote a letter to Isaacs, directing her how to reply to the criticism, which she perceived as highly personal. Klein condemned her opponents’

trick way of accepting certain things from me while disputing them partly or on certain grounds. . . . I think it is quite essential for the whole success of our discussions to show how much really had been and is being accepted which has already become part of psychoanalysis. . . . Not with one word has A. Freud ever acknowledged that she accepted anything from us. (Steiner, 1991, p. 249)

Klein saw this as “ungenerous or rather dishonest attitude.” Glover, too, she claimed—though he seems “extremely sincere and frank”—was in fact selectively adopting her ideas, picking and choosing only what suited him (Steiner, 1991, pp. 249–250).

Klein fiercely defended her ideas, but at the same time, as Riccardo Steiner (1991) also suggested, the debates prompted her to rethink her theories, and to moderate Isaacs’s enthusiastic and absolute interpretation of her views. Klein thus used the debates to stimulate her creativity. For example, she noticed that Isaacs had insisted too much on the baby’s cannibalism during the first oral phase of sucking. The personal accusations of her opponents made Klein rethink her views, as she wrote to Isaacs, “We are confronted here with an extremely difficult problem about which I would wish still to go very carefully” (Steiner, 1991, p. 251). She admitted, “I have been puzzling and puzzling again recently

¹⁵ Glover clarified this further in King and Steiner (1991, p. 432).

over the contradiction that the death instinct must contribute to the infant's hostile attitude towards stimuli and all the negative phenomena towards the external world which he exhibits in the early days" (Steiner, 1991, pp. 251–252). Thus, in a letter to Isaacs from May 14, 1943, Klein reformulated her views in direct response to the personal attacks. In discussing Isaacs's ideas about adaption to reality starting at the very beginning of life, expressed by the baby's taking the breast or food, Klein now added, "But if this is true we would have no right to say, that this derives from the UCS [unconscious] phantasy, but only that it goes along with it" (Steiner, 1991, pp. 251–252). These new views on the baby's early development would later lead Klein to further develop her important concept of projective identification.

In the Fifth Discussion held on May 19, 1943, Isaacs addressed Glover's claims. Now invoking the name of science against him, she sharply pointed to Glover's own recent reliance on Klein's work. His distinction between "Kleinian" and "Freudian" theory was a false one, she claimed, condemning his dogmatic views and failure to revise his writing to reflect his changed ideas. By drawing attention to these matters, she was trying to destabilize Glover's omnipotent male claim to represent "science," asking members to reflect on what she believed were the political and personal motivations driving his faulty methods of discussions (King & Steiner, 1991, pp. 446–450). Her words struck a chord among many members who were weary of the harsh atmosphere in the Society.

Glover stood his ground during the Sixth Discussion of October 20, 1943, but was already losing power in the BPAS and was perceived as overly insistent and repetitive. He restated, yet again, that there were irreconcilable differences between Sigmund Freudian and Kleinian ideas (King & Steiner, 1991, p. 557). In another effort to discredit the scientific validity of the Kleinians' arguments, he now claimed they quoted Sigmund Freud falsely, making it seem as though he would have supported Kleinian views. Kleinians presented their conclusions as if they were self-evidently true, while relying on their own work to prove their conclusions, using these conclusions to support their work (King & Steiner, 1991, p. 559).¹⁶

The criticisms compelled Klein to clarify what she meant by "depressive position," another key concept under dispute, tied to her notion of phantasy (Steiner, 1991, p. 254). In a 1944 paper she presented to the Society in person, she further developed her view and outlined a more nuanced understanding of the child's development. Klein linked her paper to those by Isaacs and Paula Heimann, as she discussed the primitive way the early splitting, projections, introjections, and partial good and bad objects during the paranoid position are transformed in the depressive position into a whole object in a less primitive form. In this paper, it is evident that her views had changed, and that her insistence now was on the gradual predominance of the libidinal impulses in the baby during the depressive position and on the more balanced integration of libidinal and destructive impulses. In this way, personal accusations led Klein to reconsider some of her views and to formulate them in a more nuanced manner, thus defending herself against what she perceived as Glover's misrepresentation and oversimplification of her ideas (Klein, 1991, pp. 752–797). Despite the threat to her work, Klein concluded, "I really think that our discussions so far have very much improved my position and that of my collaborators, and that it has been very fruitful, particularly since some valuable papers have been produced" (Klein, 1944/1991, pp. 254–259). Klein therefore believed that the personal accusations had a positive outcome, namely, that of developing and advancing her school.

Glover's tone and bipartisan role chairing the CDs meetings alienated the more moderate members of the Society (King & Steiner, 1991, p. 224). As a result, during the Annual Meeting of July 21, 1943, John Bowlby and W. H. Gillespie pushed for establishing a Medical Committee, and Sylvia Payne received most of the votes, while Glover

¹⁶ Glover made another lengthy intervention on December 17, 1943 (see King & Steiner, 1991, pp. 710–720).

received the fewest (King & Steiner, 1991, p. 869). Realizing he was losing power and was likely to lose the presidency of the Society were he to compete against her, the humiliated Glover swiftly resigned from office and membership in January 1944. Anna Freud likewise resigned from the Training Committee in January 1944 (King & Steiner, 1991, pp. 685–686). Glover therefore neither chaired nor was present at the Ninth and Tenth Scientific Discussions, which addressed Klein's paper; the discussions were instead chaired by Payne (Klein, 1944/1991, and following discussion, pp. 798–846). In a letter to the Society read on February 2, 1944, in the Sixth Extraordinary Business Meeting, Glover explained that he resigned on professional grounds alone. He wrote that it was clear to him that the BPAS could no longer claim to be scientific and was unlikely to remain a purely Freudian society (Klein, 1944/1991, p. 851). Continuing the Controversial Discussions was futile, he believed (p. 852). He argued that a new Freudian society should be formed (p. 853).

Glover further alarmed other members when he criticized the use of selection tests by army psychiatrists in public (in the *Listener* and in *Cavalcade*). He was therefore asked, on February 23, 1944, to issue a statement to the *British Medical Journal* or *The Lancet* clarifying that these were his private views and not those of the BPAS. During the Eighth Extraordinary Business Meeting, when Payne, Ella Sharpe, and others expressed gratitude for Glover's work in the Society, Schmideberg felt that they were completely insincere. Brierley insisted that the thanks were indeed heartfelt. However, Adrian Stephen worried that were Glover to rejoin the Society, these thanks would appear as an endorsement of his recent public activities (Klein, 1944/1991, p. 880). But Glover never returned to the BPAS, claiming that following his resignation, he had no obligation toward the Society, which he no longer regarded as Freudian. He even threatened to publish an article on the subject of "Recent Developments in Psychoanalysis, with Special Reference to Deviations from Freudian Theory" (Klein, 1944/1991, p. 894).

Conclusion

In this article, I have demonstrated that the CDs were not solely about arguments between Melanie Klein and Anna Freud. I examined the key roles of Melitta Schmideberg and Edward Glover, who lit the spark for the arguments which raged during the war. Their provocative accusations in 1941 to 1942 set events in motion, and it was thanks to their intervention that the CDs developed into an intriguing and extended engagement. They and others at the CDs were driven by both personal and theoretical motives, and these remained profoundly interwoven as discussion unfolded. During the CDs, various factions supported their views with different interpretations of the concept of science. Each side claimed their views were impersonal and rational, while nevertheless remaining strongly influenced by personal circumstances.

Throughout these debates, the language of the disputes and the criteria for the accepting new knowledge were closely tied to personalized rhetoric, to extensive accusations against the psychological soundness and private behavior of rivals, and a reliance on intimate alliances and the necessity of proving one's adherence to orthodoxy. The debates advanced in a confrontational, dialectic manner with rapid-fire accusations, forcing Kleinians to reassess and rearticulate their ideas, thus contributing to the maturation of Kleinian thought. In this way, the personal context which stimulated the development of psychoanalytic theory should also be considered in understanding antagonistic claims disguised as "pure scientific" queries. Personal animosity drove the provocative, unrelenting questioning of the discipline's figures of authority, kindling a passionate debate that otherwise would not have gained the same traction or depth.

In July 1944, Sylvia Payne became President of the BPAS. With Glover and Anna Freud resigned, and the Schmidebergs abstaining, the power balance in the Society shifted back to the hands of prewar British psychoanalysts. Klein was subsequently able to protect

her position and secure her views. But as the Second World War drew to a close, Payne remained displeased that Freud's daughter was not part of the Society. And so she began negotiating for the return of Anna Freud, eventually leading to the BPAS to maintain its diversity after all by allowing training in the traditions of Klein, Anna Freud, and the middle group of independent analysts (Klein, 1944/1991, pp. 920–931). But as I have proposed, the real casualties of this "Ladies' Agreement" were Glover and Schmideberg. Following his resignation, Glover joined the Swiss Psychoanalytical Society. Despite his past as one of the most powerful men in the BPAS, as well as one of its most prominent public figures, his work was forgotten and his name is known to few today. Schmideberg's work is even less known. She moved to New York, far away from her mother and her colleagues, where she thrived, continuing her work on delinquency and enjoying a prolific career as a writer. She became increasingly critical of psychoanalysis in general, and did not find herself at home among the New York Freudians, admitting that the British Society was, after all, more theoretically sophisticated. She returned to Britain only in 1961, after her mother's death in 1960. Schmideberg resigned from the British Society in 1962 and did not stay in touch with analysts there (Melitta Schmideberg, 1983).

As we saw, since the mid-1930s, Glover and Schmideberg tried to develop different versions of psychoanalysis; Glover pushed for more integration with hard science and for close adherence to Sigmund Freud's original ideas, and Schmideberg developed theoretical innovations and a more humane technique of treating patients. After Freud's death, psychoanalysts were especially eager to draw the boundaries of their discipline, lay claim to scientific authority, and insure that psychoanalysis would follow the contours of science as based on professionalism, objectivity, and universal truth. Yet, rather than disciplinary rigidity, remarkably, the postwar era eventually saw the development of diverse psychoanalytic schools (Viner, 1996). However, not all analytic viewpoints remained on the table. As they lost the battle of the CDs, Glover's and Schmideberg's visions for the development of psychoanalysis were marginalized. Thus, the personal lives of those involved shaped the theoretical trajectory of the discipline. The personal realm, then, was constitutive and pivotal to scientific advancement, the professionalization of the field, and the exclusion of competing views and intellectual visions. We therefore must treat rivalries and animosities as serious conditions for knowledge production and as fruitful social mechanisms for the development of intellectual ideas in psychoanalysis and beyond.

The rules of objectivity require overcoming private alliances, going beyond the individuals who developed ideas and above the biases arising from personal relationships. But what was seen as scientific or objective was very much in the eyes of the beholders during the CDs, in which participants offered competing perceptions of science. Rancorous interactions and the melodrama of personal affinities influenced scientific judgment and criteria for reason and professionalism.¹⁷ Petty rivalries served as an engine for thought as much as other contexts. At times, the discussion of scientific procedures and theories was expressed in a gendered manner, with reason, objectivity, and fair evaluation of facts seen as the monopoly of masculinized science set against gossip, manipulations, biased views, and, by implication, feminine preoccupations or styles. As we inquire into the history of psychology, we should view personal, hostile relations as a legitimate avenue for understanding the production of knowledge. The analysts themselves did not view personal relations as a legitimate site for intellectual production. Yet we should not take their word for it, as it is evident that personal relations stood at the heart of their realm of knowledge. We therefore must connect their interpersonal relations with an analysis of their ideas and

¹⁷ I follow Bonnie Smith's (1995b, p. 665) suggestion that as contemporary psychologists have convincingly theorized an end to mind body dualism, historians, too, should rethink the idea that in judging evidence, we step outside ourselves for the moment of ascertaining truth in the face of insights from other disciplines about the decision-making process.

deal with their lives as more than just the background for their theories. The pursuit of scientific orthodoxy was not only an intellectual journey, but an emotional one (cf. Levin, 2011; Smith, 1992, 1995a).

In the psychoanalytic debates, the scientific and the personal were not segregated, but deeply intertwined. In their practice, psychoanalysts worked closely with emotions, but in their research and theory, they tried to maintain a boundary between affect and reason, and between personal circumstances and universal claims to truth. However, as I have shown, passionate disagreement contributed to the advancement of ideas, provoking new thinking about the discipline, and producing innovative papers. Intellectual scientific work was rooted in a context of personal relations. Acknowledging this can help us better understand other fields of scientific psychological inquiry in which reason and affect are similarly entangled. The claim to scientific objectivity and drawing a neat line between scientific reason and affect are still prevalent practices in modern fields of knowledge, and must therefore be continually challenged. The CDs offers an exceptional case study of this broader insight, especially because of the complex relationships between the participants and the atmosphere of heightened anxiety among psychoanalysts during the war.

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