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The Intimate Geographies of Panic Disorder: Parsing Anxiety through Psychopharmacological Dissection

by *Felicity Callard**

ABSTRACT

The category of panic disorder was significantly indebted to early psychopharmacological experiments (in the late 1950s and early 1960s) by the psychiatrist Donald Klein, in collaboration with Max Fink. Klein's technique of "psychopharmacological dissection" underpinned his transformation of clinical accounts of anxiety and was central in effecting the shift from agoraphobic anxiety (with its spatial imaginary of city squares and streets) to panic. This technique disaggregated the previously unitary affect of anxiety—as advanced in psychoanalytic accounts—into two physiological and phenomenological kinds. "Psychopharmacological dissection" depended on particular modes of clinical observation to assess drug action and to interpret patient behavior. The "intimate geographies" out of which panic disorder emerged comprised both the socio-spatial dynamics of observation on the psychiatric ward and Klein's use of John Bowlby's model of separation anxiety—as it played out between the dyad of infant and mother—to interpret his adult patients' affectively disordered behavior. This essay, in offering a historical geography of mid-twentieth-century anxiety and panic, emphasizes the importance of socio-spatial setting in understanding how clinical and scientific experimentation opens up new ways in which affects can be expressed, shaped, observed, and understood.

MARY J.'S PANIC

In 1981, in the popular psychology magazine *Psychology Today*, psychiatrists Paul Wender and Donald Klein heralded the promise of biological psychiatry by emphasizing that revelations about the centrality of biological malfunctions in mental illness

* Department of Geography, Durham University, Lower Mountjoy, South Road, Durham, DH1 3LE, United Kingdom; felicity.callard@durham.ac.uk.

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would show many of the tenets of psychodynamic theory to be “irrelevant or even misleading.”¹ They illuminated their argument with brief case histories. One was of a twenty-three-year-old agoraphobic woman whom they called “Mary J.” She was an unmarried buyer for a department store who was suffering from debilitating panic attacks: “She would suddenly be overcome by dizziness, a pounding heart, and an inability to catch her breath while walking down the street or riding on public transportation.”² Wender and Klein noted that physiologically oriented specialists had tended to diagnose her as suffering from nerves or a virus and had been unable to help Mary J. when she stopped using subways and buses in favor of taxis, ensconced herself in her parents’ home, and gave up her job. Mary J., feeling increasingly desperate, sought a psychoanalyst. Wender and Klein reported that on the couch:

she began to suspect that the panics might be related to a love affair. Six weeks before the attacks started, she had been quite upset: her lover had moved to another city. The analyst closed in on that possibility with penetrating questions. Had her sexual adjustment been guilt-free after she had begun the affair? Didn’t her fear of being out on the street reflect her unconscious doubts about her sexual self-control—that is, her fear of identification as a streetwalker? Didn’t her clinging dependence on her family show her fear of adulthood and her unconscious desire to substitute her father for other men?³

Mary J. spent four years “rework[ing] such baroque structures” while her symptoms came and went. In the end, she left the analyst disillusioned and turned to behavior therapy, a newer, shorter, and cheaper form of treatment. Wender and Klein described how she was given instruction in relaxation exercises and desensitization—exposure to public places—and how, despite initial progress, the panic attacks returned with a vengeance. Mary J. and her parents were, by then, reportedly desperate and resigned

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¹ Paul Wender and Donald Klein, “The Promise of Biological Psychiatry,” *Psychol. Today*, February 1981, 25–41, on 25.

² *Ibid.*, 31.

³ *Ibid.* The reference to “her fear of identification as a streetwalker” alludes to psychoanalytic models of agoraphobia. Freud, in his letter to Wilhelm Fliess of 17 December 1896, wrote, “I actually confirmed a conjecture I had entertained for some time concerning the mechanism of agoraphobia in women. No doubt you will guess it if you think of ‘public’ women. It is the repression of the intention to take the first man one meets in the street: envy of prostitution and identification” (Sigmund Freud and Wilhelm Fliess, *The Complete Letters of Sigmund Freud to Wilhelm Fliess, 1887–1904*, trans. Jeffrey Moussaieff Masson [Cambridge, Mass., 1985], 217–8). While Freud himself wrote little specifically on agoraphobia, the associations he made between agoraphobia and sexual fantasies regarding the street were taken up by the next generation of psychoanalysts; see, e.g., Walter Schmideberg, “Agoraphobia as a Manifestation of Schizophrenia: The Analysis of a Case,” *Psychoanal. Rev.* 38 (1951): 342–52. The figure of a woman who sequesters herself indoors and thereby removes herself from circulation in the public sphere has appeared a number of times in twentieth-century psychiatric and psychological writings on phobias and anxiety. In 1964, e.g., A. H. Roberts brought to visibility the figure of “the housebound housewife” in a retrospective study of married women in London. See Roberts, “Housebound Housewives—A Follow-Up Study of a Phobic Anxiety State,” *Brit. J. Psychiat.* 110 (1964): 191–7; see also subsequent research on this figure by psychiatrist and behavioral therapist Tom Kraft (e.g., “Sexual Factors in the Development of the Housebound Housewife Syndrome,” *J. Sex Res.* 6 [1970]: 59–63). Understanding the overdetermined way in which gender, class, and the articulation of the public and private spheres are—within modernity—bound up with each other, is, I would argue, central to understanding histories of fear and anxiety—both within and beyond the domains of science and medicine.

to her becoming a long-term invalid. But Wender and Klein reported that only a year later, she had experienced a profound affective and social transformation: she was living in the city on her own in an apartment, had returned to her job, and was excited about possible marriage plans. (Let us notice, here, how effective therapeutics transforms “bad” affect into “good,” such that equanimity and happiness are demonstrated through a turn to normative forms of sociality [returning to a job, excitedly focusing on marriage].) What had happened? The psychopharmacologists revealed that Mary J. had volunteered to join a clinical experiment for the treatment of phobias that involved taking an antidepressant medication. The drug had stopped the panic attacks, and accompanying psychotherapy had “helped her to control her anticipatory anxiety and allowed her to resume normal activities.” The symptoms, we are told, did not return when Mary J. stopped taking the medication six months later.

Wender and Klein’s vignette of Mary J. was in the service of their own strong commitment to the effectiveness of biological psychiatry—and as such demanded a disparagement of other clinical approaches to agoraphobia, particularly that of psychoanalysis, and a narrative that culminated in the triumphant success of drug therapy. Their article was published a year after the American Psychiatric Association had anointed the new category of panic disorder in the third edition of the *Diagnostic and Statistical Manual of Mental Disorders (DSM-III)*—a category that had been brought into being in large part through Donald Klein’s psychopharmacological research that stretched back to the late 1950s, and whose triumphant arrival on the stage of American—and subsequently international—psychiatry was, after a long journey, undoubtedly ensured by Robert Spitzer (the chair of the *DSM-III* Task Force) selecting Klein to join the Task Force on Nomenclature and Statistics.⁴

Wender and Klein’s article should be read as a celebration not only of drug treatment as cure but of drug treatment as diagnostic dissection tool. The article not only celebrated how psychopathological affect might be successfully treated via drugs but exemplified a more wide-ranging logic, developed in large part by Klein, and assembled from both empirical and conceptual elements, in which the very shape and timbre of that psychopathological affect might be identified and parsed through drugs.⁵ In the same year that Wender and Klein mused in print over Mary J.’s panic, Klein, in a chapter intended for a scientific readership, celebrated what he considered his analytical and methodological breakthrough, namely, “the power of the experimental technique of pharmacological dissection whereby one can pierce through the fascinating, confusing web of symptoms and dysfunctions to tease out the major participant variables by attending to specific drug effects.”⁶ It was a bold claim that came to inaugurate and cement a new approach to the study of anxiety. Klein’s pharmacological interventions had, he averred, disinterred panic disorder—a phenomenon, and a very particular manifestation of pathological affect, that had hitherto remained largely ignored by dint of erroneous formulations concerning agoraphobia.

⁴ American Psychiatric Association, *Diagnostic and Statistical Manual of Mental Disorders*, 3rd ed. (Washington, D.C., 1980). See Hannah S. Decker, *The Making of DSM-III: A Diagnostic Manual’s Conquest of American Psychiatry* (New York, 2013), chaps. 4, 5.

⁵ Klein was of course operating as part of a much larger collectivity of researchers grappling with how best to conceptualize and describe drug action. Nonetheless, there was something singular as well as compelling about how he formulated his model of “psychopharmacological dissection,” which buttressed what would become a new nosological entity.

⁶ Donald F. Klein, “Anxiety Reconceptualized,” in *Anxiety: New Research and Changing Concepts*, ed. Donald F. Klein and Judith G. Rabkin (New York, 1981), 235–62, on 242.

What might dwelling on this particular moment—the disinterring of an emotion by a clinician and a scientist via his observation and analysis of drug effects—contribute to our understandings of the long history of models and experiences of fear, anxiety, and phobias? Otniel Dror and colleagues, in their introduction to this volume, ask whether “discrete scientific developments structure the expression, experience, visibility, or nature of emotions?”⁷ While panic as a phenomenon and a topic of concern in the history of science and medicine stretches much further back than Klein’s post-World War II drug experiments, the emergence of panic disorder arguably established a new way through which manifestations of overwhelming, negative affect—experienced by individuals in particular socio-spatial settings—could be understood, narrated, and, indeed, experienced. If agoraphobia as a term consistently posed the question of the agora (Why did it provoke fear? How ought it to be construed?), panic disorder posed questions about the ontology of the affective phenomenon—panic—itsself. Klein himself argued that drug action allowed the observation of two ontologically distinct kinds of anxiety (anticipatory anxiety and panic) that had been conflated in earlier models and theorizations of anxiety. As historians of science and of the emotions, we might instead continue to ponder whether the particular socio-spatial arrangement of Klein’s psychiatric wards—one that meshed patients’ physiologies, pharmacological action, patient and staff behavior, and practices of observation—structured the very way in which particular affects came to be expressed, shaped, and understood.

I will be particularly attentive, here, to the need to understand how geography figures both in the production of psychopathological affects and in clinical and scientific accounts of those psychopathological affects. The socio-spatial assemblage of epistemological, methodological, and observational techniques that underpinned Klein’s work of psychopharmacological dissection operated significantly differently from the one that, since the 1870s, had at its center a clinician puzzling over how to understand and interpret the actions as well as the affective distress of a figure attempting to navigate his way through an urban, public landscape that he could not comfortably inhabit or traverse.⁸ The neurologist and psychiatrist Carl Friedrich Otto Westphal, who originated the term agoraphobia in the early 1870s, for example, opened his seminal essay by describing how, “for some years patients have repeatedly approached [him] with the peculiar complaint that it is not possible for them to walk across open spaces and through certain streets and that, due to the fear of such paths, they are troubled in their freedom of movement.”⁹ Westphal’s task, and subsequently that of many neurologists and psychoanalysts who saw agoraphobic patients in their consulting rooms and in outpatient clinics in the late nineteenth and early twentieth centuries, was to respond to those patients’ phenomenologically rich accounts of their inability to move through

⁷ Otniel E. Dror, Bettina Hitzer, Anja Laukötter, and Pilar León-Sanz, “An Introduction to *History of Science and the Emotions*,” in this volume.

⁸ Note that I have used “he” here. Part of the complex history of gender in relation to agoraphobia is addressed in Felicity Callard, “Understanding Agoraphobia: Women, Men, and the Historical Geography of Urban Anxiety,” in *Exploring Women’s Studies: Looking Forward, Looking Back*, ed. Carol Berkin, Judith Pinch, and Carol Appel (Upper Saddle River, N.J., 2006), 201–17.

⁹ Carl Friedrich Otto Westphal and Terry J. Knapp, *Westphal’s “Die Agoraphobie” with Commentary: The Beginnings of Agoraphobia*, trans. Michael T. Schumacher (Lanham, Md., 1988), 59. The German original is Westphal, “Die Agoraphobie: Eine Neuropathische Erscheinung,” *Arch. Psychiat. Nervenkrankheit*. 3 (1871): 138–61. Westphal’s other writing on agoraphobia included “Über Platzfurcht: Briefliche Mitteilungen,” *Arch. Psychiat. Nervenkrankheit*. 7 (1877): 377–83; “Nachtrag zu Dem Aufsatz ‘Über Agoraphobie,’” *Arch. Psychiat. Nervenkrankheit*. 3 (1872): 219–21.

open squares and down particular streets. Those narratives bound agoraphobic anxiety tightly to particular urban locales: Westphal, for example, memorably recounted features of the Berlin cityscape (including particular squares, as well as the Charlottenburg zoo) that precipitated extensive fear in the three agoraphobic individuals whose case histories he enumerated in his 1871 article.¹⁰ The logic outlined by Klein emerged from a different socio-spatial world. It was at some distance both from Westphal's symbols and markers of nineteenth-century urbanicity and from the sedate consulting rooms of neurologists and psychoanalysts; it entailed different dynamics of clinical observation. If, for Westphal, the figure, gestures, and affective tenor of "the agoraphobe" provoked clinical intrigue by dint of his stalled passage across the public spaces of the agora, Klein's archetypal, phobic-anxiety figure emerged within the claustral spaces of a mental hospital. What would come to define him or her would not be, as in the case of Westphal's patients, an uncomfortable relationship to walls, passageways, and public squares, but panic attacks, helplessness, and the need for a reassuring, parent-like figure. Thus while Wender and Klein's plot regarding Mary J. in *Psychology Today* commenced with the puzzle of her breathlessness on city streets, neither Klein's own practices of observation nor his investigatory frameworks were centrally preoccupied with the textures and socio-spatial specificities of the urban landscape. How Klein's logic came to be articulated and how the framework in which it was housed worked is what I shall track in this article. If histories of the emotions have provided many nuanced accounts of how temporality is construed and mobilized in different models of particular affects, there has perhaps been less explicit attention to spatiality.¹¹ This essay is intended, then, as a contribution to the historical geography of the emotions: I am particularly interested in how Klein worked with, and characterized in a particular way, his anxious patients' experiences of the inpatient ward, as well as how the diagnosis of panic disorder dispensed with an explicit emphasis on the subject's relationship to space (cf. agoraphobia).

* * *

Wender and Klein's vignette of Mary J. represented in schematic form the trajectory that approaches to agoraphobia, and agoraphobic anxiety, took in the United States in the second half of the twentieth century. That trajectory moved from the hegemony of psychoanalytic or psychodynamic approaches, to the markedly different therapies and etiological arguments of the behaviorists, to the near inescapability of psychopharmacology for the understanding and treatment of anxiety, panic, and phobias.¹² As different therapeutic regimens jostled for preeminence, they were accompanied by different models of phobic anxiety: those models worked with a variety of phenomenologies of psychopathological affect, of the kinds of individuals who were most

¹⁰ Westphal, "Die Agoraphobie" (cit. n. 9). See also Felicity Callard, "'The Sensation of Infinite Vastness': Or, the Emergence of Agoraphobia in the Late 19th Century," *Environ. & Planning D: Soc. & Space* 24 (2006): 873–89.

¹¹ Though for an account that takes seriously the problems of both temporality and spatiality in providing a finely grained history of how the affective "accidents of everyday existence" were "transformed into the objects of psychiatric epidemiology," see Rhodri Hayward, "Sadness in Camberwell: Imagining Stress and Constructing History in Postwar Britain," in *Stress, Shock, and Adaptation in the Twentieth Century*, ed. David Cantor and Edmund Ramsden (Rochester, N.Y., 2014), 320–42.

¹² Up to the 1950s and 1960s, most clinicians in the United States aligned themselves directly or indirectly either with the work of Freud or with the psychobiological or psychosocial approaches of those such as Adolph Meyer and William Menninger. The late 1950s was a period of great transformation in the American psychiatric establishment. Psychoanalysis was facing attacks from several

susceptible to being gripped by such affect, of narratives about how such affect would manifest on and through the body, and of what precipitated and maintained those manifestations of affect. Those models were subtended by both implicit and explicit claims about the kind of clinical expert, and the kind of observational practices in which he or she was proficient, that were appropriate to identify and then to intervene upon that affect.

I shall be interested in analyzing specifically how Donald Klein's early psychopharmacological research—conducted in collaboration with the psychiatrist and clinical researcher Max Fink—helped to transform the techniques and conceptual apparatus for observing, identifying, and parsing psychopathological manifestations of anxiety.¹³ Surprisingly, there has been relatively little fine-grained historical work that has focused specifically on the imbrication of observational and epistemological frameworks in mid-twentieth-century psychiatry, and their centrality in grounding not only particular diagnostic entities and/or symptoms but particular conceptualizations of psychopathological affect.¹⁴ This article intends to make a contribution to that body of literature.

My argument relies on analyses of published documentation. It therefore reckons with, as well as potentially further embeds, the rhetorical power of Klein's and Fink's written arguments—whether in journal articles reporting their empirical findings or in retrospective narrative accounts of the discovery of psychopharmacological dissection. I am aware of the gap that undoubtedly exists between those tidied, published accounts, and the actual heterogeneous practices of observation that would have taken place in the clinical spaces that acted as the crucible for the development of Klein and Fink's conceptual architecture. Nonetheless, given the centrality of the scalpellic logic of psychopharmacological dissection to the emergence of panic disorder, and the surprising dearth of theoretico-historical elaborations of that logic in the history of psychiatry, close consideration of the workings of those published texts is, I argue, justified.¹⁵

quarters: behaviorists accused psychoanalysts of unscientific methods and proposed very different models of fear and anxiety that derived from the early twentieth-century classical conditioning experiments of researchers such as Ivan Pavlov. Prominent among them was Joseph Wolpe, who developed methods of behavioral desensitization to phobic objects. See Wolpe, *Psychotherapy by Reciprocal Inhibition* (Stanford, Calif., 1958).

¹³ Other historical and sociological investigations of agoraphobia and anxiety include Anthony Vidler, *Warped Space: Art, Architecture, and Anxiety in Modern Culture* (Cambridge, Mass., 2000); Paul Carter, *Repressed Spaces: The Poetics of Agoraphobia* (London, 2002); Jackie Orr, *Panic Diaries: A Genealogy of Panic Disorder* (Durham, N.C., 2006); Callard, “‘The Sensation of Infinite Vastness’” (cit. n. 10); Kathryn Milun, *Pathologies of Modern Space: Empty Space, Urban Anxiety, and the Recovery of the Public Self* (New York, 2007); Shelley Zipora Reuter, *Narrating Social Order: Agoraphobia and the Politics of Classification* (Toronto, 2007).

¹⁴ Let me be clear. There are of course historical, anthropological, and sociological studies that have attended carefully to psychiatric case records to demonstrate how diagnostic categories were made (e.g., Jonathan Metzl, *The Protest Psychosis: How Schizophrenia Became a Black Disease* [Boston, 2009]). However, relatively few have analyzed how particular epistemological frameworks were bound up with certain practices of observation and their eliciting and interpretation of particular data. One example that focuses on late nineteenth-century psychiatric nosology is Kathleen M. Brian, “‘Occasionally Heard to Be Answering Voices’: Aural Culture and the Ritual of Psychiatric Audition, 1877–1911,” *Hist. Psychiat.* 23 (2012): 305–17.

¹⁵ Klein's early experiments with Max Fink are discussed and analyzed in David Healy, *The Anti-depressant Era* (Cambridge, Mass., 1997), 191–3; Orr, *Panic Diaries* (cit. n. 13), 170–2, 205–9. Nei-

HOW DO THE DRUGS ACT?

The synthesis of chlorpromazine in 1951, and its arrival on the psychiatric scene in France in 1952, was a key moment in the development of psychopharmacology. It was termed a “major tranquilizer,” not least because of its striking effects on the behavior of some psychotic patients. These effects raised intriguing problems for psychiatrists and pharmacological researchers,¹⁶ for it was becoming clear to them that they possessed few methods to assess not only the effectiveness of drug therapies but also the effects of other treatments (such as lobotomy and electroconvulsive therapy) already within the psychiatric therapeutic armamentarium. In the United States, unanswered methodological, epistemological, and ontological questions about drugs and drug action increasingly preoccupied the Committee on Psychiatry of the National Academy of Sciences–National Research Council, the National Institute of Mental Health (NIMH), and the Committee on Research of the American Psychiatric Association. In 1956, the “Conference on the Evaluation of Pharmacotherapy in Mental Illness” was organized by the psychiatrist Jonathan Cole¹⁷ and the neurophysiologist Ralph Gerard in order to break new ground. The conference had grown out of conversations within and across those committees and organizations and was intended to address the nub of the problem, namely: “Do the drugs act? How do the drugs act? What if the drugs act?” The domain of affect would be central to any kind of answer to the first question, since any determination regarding therapeutic benefit for psychiatric disorders would undoubtedly consider potential affective as well as cognitive transformations in the bodies of those ingesting those drugs.

At the time of that conference, the protocols and frameworks surrounding the organization and practice of clinical evaluation in general, and of clinical trials in par-

ther systematically addresses the logic underlying the observational practices that underpin Klein’s “psychopharmacological dissection.” I am sympathetic to Viola Balz’s arguments vis-à-vis the dangers of relying on published material by and interviews with psychiatrists and other scientific researchers (Balz, “Terra Incognita: An Historiographic Approach to the First Chlorpromazine Trials Using Patient Records of the Psychiatric University Clinic in Heidelberg,” *Hist. Psychiat.* 22 (2011): 182–200). As she rightly notes, this ends up silencing the patient’s voice and rendering invisible the complex relation between clinician and patient around which the act and action of drug taking took place. My longer-term aim is to work with the patient records from the Hillside Hospital, to which I have not yet been able to gain access. Here, I focus on the logics mobilized by psychiatric clinicians in their published texts, logics in which the patient’s voice is, for multiple reasons (some of which will become clearer later in this article), attenuated if not obliterated. Since the outcome of Klein’s logic was wide-ranging and influential, I believe it is worth analyzing.

¹⁶ David Healy provides a comprehensive overview of the arrival of psychopharmacology, including the momentous synthesis of chlorpromazine. Healy, *The Creation of Psychopharmacology* (Cambridge, Mass., 2002); see also Healy, *The Antidepressant Era* (cit. n. 15). Viola Balz and Volker Hess provide a detailed analysis of the challenges raised by chlorpromazine in Germany (Balz and Hess, “Psychopathology and Psychopharmacology: Standardisation from the Bottom Up, Using the Example of Neuroleptics,” in *Harmonizing Drugs: Standards in 20th-Century Pharmaceutical History*, ed. Christian Bonah, Christophe Masutti, Anne Rasmussen, and Jonathan Simon [Paris, 2009]). See also Thomas A. Ban, “Fifty Years Chlorpromazine: A Historical Perspective,” *Neuropsychiat. Dis. Treat.* 3 (2007): 495–500; Andrew C. Leon, “Evolution of Psychopharmacology Trial Design and Analysis: Six Decades in the Making,” *J. Clin. Psychiat.* 72 (2011): 331–40.

¹⁷ On Cole’s central role in early psychopharmacological research in the United States, see Martin M. Katz, “Jonathan O. Cole,” *Neuropsychopharmacology* 35 (2010): 2647; Nina R. Schooler, “Jonathan O. Cole, MD (1925–2009): Innovator in Clinical Psychopharmacology and of the ECDEU/NCDEU Tradition,” *J. Clin. Psychiat.* 72 (2011): 286–7. See also Jonathan O. Cole and David Healy, “Jonathan Cole: The Evaluation of Psychotropic Drugs,” in *The Psychopharmacologists: Interviews by Dr. David Healy*, by David Healy, 3 vols. (London, 1996), 1:239–63.

ticular, were still very much in flux. Gerard, for example, in outlining to the audience the scope of the problems that the field of drug testing in human subjects was facing, emphasized how much work there was still to be done in determining “the selection of the experimental and of the control populations, the testing conditions, the criteria for evaluating change, the follow-up procedures, the quantitative judgments and the properties of reporting results.”¹⁸ How, he asked, should the timing and dose of the drug be determined? Where should the drug be administered? (In order to emphasize the importance of context for determining how substance and soma interact, Gerard quipped that “alcohol acts different [*sic*] in the presence of one’s boss or one’s blond.”)¹⁹ How do changes in the ward situation during the experiment affect drug action? Should one select control populations, and if so, on what grounds does one designate them as controls? From whom or from what should one gather and/or elicit information about any changes produced by the drug? Gerard’s questions pointed to the complex webs that connected the drug and the patient’s body to the various worlds in which she lived and within which the potential action of the drug might be observed and rendered visible. What, exactly, should be held stable for change to be both discerned and measured? How might the emergence or attenuation of particular affects in and through patients’ bodies be one conduit through which a claim for the effectiveness of a drug’s action might be lodged? And what kinds of practices, housed within which kinds of bodies and drawing on which kinds of observational skills, would be best placed to discern and measure those changes?

Let us zero in on Cole’s deliberations over which aspects of the patient needed to be monitored for signs of change, and which technologies and practices of observation needed to be mobilized in order to do so. Cole grappled with the fact that, at that moment, there was neither general agreement nor any unified codes of practice either in relation to describing, or indeed naming, changes in psychopathological behavior or in relation to identifying specific effects that the new drugs helped to bring about. It was on this muddled and muddied terrain—a terrain that featured the patient’s body, the psychopharmacological substance, the clinical scientist, other clinical care staff, and the wards in which they were emplaced—that Donald Klein, a few years later, would come to excavate what he argued were ontologically heterogeneous manifestations of anxiety. The crispness of Klein’s empirical and conceptual work was underpinned by what he maintained was the near-surgical precision of one particular drug as it functioned as a psychopharmacological dissection tool. As we recognize the boldness of Klein’s maneuver, we might do well simultaneously to keep in view the dense and heterogeneous landscape—material, methodological, and epistemological—with which such a maneuver had both to reckon and, ultimately, to dispense.

Cole, in his conference presentation, covered a gamut of tools and techniques that might be used—starting with clinical rating scales, and moving through interview content analysis (of use, he suggested, in fine-grained analyses of progress in psychotherapy, or for following the effects of other therapies by dint of frequent psychother-

¹⁸ Ralph W. Gerard, “Orientation: Analysis of Program,” in *Psychopharmacology: Problems in Evaluation: Proceedings of a Conference on The Evaluation of Pharmacotherapy in Mental Illness Sponsored by the National Institute of Mental Health, the National Academy of Sciences–National Research Council, and the American Psychiatric Association, Washington, D.C., September 18–22, 1956*, ed. Jonathan O. Cole and Ralph W. Gerard (Washington, D.C., 1959), 9–19, on 13.

¹⁹ *Ibid.*, 15.

apeutic interviews), psychological tests (which included the Rorschach, as well as personality tests used in clinical psychology), and physiological evaluation (such as estimating autonomic “reactivity” by injecting small doses of sympathetic or parasympathetic drugs to predict response to somatic therapy).²⁰ Cole was acutely aware of the problems posed by the contemporaneous scientific push to increase the scale of psychopharmacological experimentation and analysis. For example, he noted that in a study taking place in a large state hospital, a six-item rating scale may be superior to a fifty-item scale and concluded—in a sentence that accurately presaged the incipient arrival of many short psychiatric rating scales in psychiatry—that “*brief* rating scales for judgments of the patient’s psychopathology, to be filled out both by the admitting physician and by the nurses and aides, could be of considerable help in delimiting better the types of patients helped and in providing easily usable data amplifying the assigned diagnosis.”²¹ The conference as a whole was, indeed, filled with scientists’ and clinicians’ ambitions to push toward larger scales of analysis and evaluation, and with recognition of their need for better technologies to track and capture change across large vistas of clinical experimentation.

We see, then, how central geography was to the challenge of assessing affective and cognitive change. I use the term “geography” to connote both the physical spaces in which and through which drug action might be adjudicated and measured and the various socio-spatial imaginaries mobilized by clinical researchers. Should the evaluative terrain encompass the microgeographies of conversations between patient and psychoanalyst, or the slightly broader circuit between clinician, patient, and the technological device of a printed rating scale? Or should the focus rather be on the dynamic psychosociological topologies of spatial interactions and atmospheres within the ward, or on tracing out a temporally dislocated geography in which social workers are sent on unexpected visits to discharged patients, beyond the reaches of the hospital and of the locus of the treatment itself?²² Even if clarity could be acquired about the appropriate socio-spatial context in which to evaluate psychopharmacological action, there remained unresolved questions about the accuracy of the behavioral and affectively freighted material that would be acquired. For example, some psychoanalytically oriented researchers worried that assessing therapeutic change by analyzing interview content would result in fixating too much on conscious, verbal communication and occlude analysis of unconscious motivations and nonverbal modes of communication.²³

²⁰ Jonathan O. Cole, “The Evaluation of the Effectiveness of Treatment in Psychiatry,” in Cole and Gerard, *Psychopharmacology* (cit. n. 18), 92–107.

²¹ *Ibid.*, 104; emphasis added. A number of rating scales were developed in the late 1950s and early 1960s; those that became particularly influential in psychiatry include Max E. Hamilton, “The Assessment of Anxiety States by Rating,” *Brit. J. Med. Psychol.* 32 (1959): 50–5; Hamilton, “A Rating Scale for Depression,” *J. Neurol. Neurosurg. Psychiat.* 23 (1960): 56–62; A. T. Beck et al., “An Inventory for Measuring Depression,” *Arch. Gen. Psychiat.* 4 (1961): 561–71; John E. Overall and Donald R. Gorham, “The Brief Psychiatric Rating Scale,” *Psychol. Rep.* 10 (1962): 799–812. See also Michael Worboys, “The Hamilton Rating Scale for Depression: The Making of a ‘Gold Standard’ and the Unmaking of a Chronic Illness, 1960–1980,” *Chronic Illness* 9 (2013): 202–19.

²² Such evaluative methods were used by the psychiatrist Maxwell Jones, pioneer of the concept of the therapeutic community. Jones, *The Therapeutic Community: A New Treatment Method in Psychiatry* (New York, 1953).

²³ See Cole, “Evaluation” (cit. n. 20), 100.

Perhaps it was not surprising, then, that Cole finished his address by noting that, in spite of the clinical and scientific evaluative innovations offered by various technologies, by the use of diverse socio-spatial settings, and by the push toward assessing larger numbers of patients across multiple sites, “The detailed study of the response to treatment of the individual patient under the experienced observer will still, no doubt, provide leads to be tested on a larger group of patients.”²⁴ That statement appears to be a call to order grounded on straightforwardness and simplicity: Find one trained observer who can focus on the individual patient! And, indeed, as we shall see, much of the potency of Donald Klein’s early scientific innovations arose precisely from the act of a small number of experienced observers studying the response to treatment of a small number of patients. There is no doubt that Klein’s achievement in installing panic disorder as a new nosological entity was significantly dependent on small-scale, intimate geographies and was not born from the large, multisite experiments that were fantasized about at the 1956 drug evaluation conference. But we should not be hoodwinked by the apparent simplicity of Cole’s injunction. What constitutes, in practice, the actions and interpretations of any one “experienced observer” in psychiatric research is anything but straightforward.²⁵

CREATIVE EXPERIMENTATION IN HILLSIDE HOSPITAL

Historians of US psychiatry have spent much analytical and empirical energy detailing the shift from psychodynamic models (prominent, e.g., in the second edition of the *DSM*) to the classificatory logics of *DSM-III*.²⁶ Central to that shift toward new classificatory logics was the proliferation in the 1960s and 1970s of psychiatric rating scales in relation to both symptoms and diagnoses. This emphasis on ratings in the service of standardization, in particular as regards the imperative to ensure reliability in relation to psychiatric diagnoses, has had consequences beyond that of directing our focal gaze toward 1980, the watershed year in which *DSM-III* was published. One consequence comprises the tight bonds that have been drawn between the emergence of psychopharmacology, the development of robust clinical protocols, and the overall push toward standardization. This narrative of methodological transformation has made it more difficult, perhaps, to keep in focus some of the most imaginative and influential early psychopharmacological studies, which departed significantly from the driving logic of rating scales with their impetus toward a standardization of symptomatology and an elaboration of target symptoms.²⁷ My focus here will be on the

²⁴ *Ibid.*, 104.

²⁵ As Lorraine Daston and Elizabeth Lunbeck have made clear, “Observation is a highly contrived and disciplined form of experience that requires training of the body and mind, material props, techniques of description and visualization, networks of communication and transmission, canons of evidence, and specialized forms of reasoning.” See Daston and Lunbeck, “Observation Observed,” in *Histories of Scientific Observation*, ed. Daston and Lunbeck (Chicago, 2011), 1–9, on 3. In my essay, I want to draw attention to the authority accruing to a certain kind of clinical observation, and to its importance in foregrounding a particular psychopathological affect. For other examinations of the complex relationship between the clinical and/or scientific observer and that which she observes, see Lunbeck, “Empathy as a Psychoanalytic Mode of Observation: Between Sentiment and Science,” and Otniel Dror, “Seeing the Blush: Feeling Emotions,” both in Daston and Lunbeck, *Histories*, 255–75, 326–48; Tiffany Watt Smith, *On Flinching: Theatricality and Scientific Looking from Darwin to Shell-Shock* (Oxford, 2014).

²⁶ Decker, *The Making of DSM-III* (cit. n. 4).

²⁷ Two important exceptions include scholarship by David Healy and by Viola Balz and colleagues (who emphasize the importance in early German psychopharmacology of long-term clinical observa-

creative experimentation that entangled drugs, bodies, minds, and affects in a far-from-standardized early set of experiments that took place in one small psychiatric hospital in the United States in the late 1950s. I am locating our analytic gaze, in other words, on a historical-geographical site that is significantly different from the perhaps more settled, and perhaps epistemologically less lively, clinical and research landscape that would come to be installed via the logics of the psychiatric randomized controlled trial and, subsequently, the framework of *DSM-III*.

What kinds of scientific and clinical observation of research patients were sanctioned in Klein and Fink's early psychopharmacological experiments, and how did they help consolidate new ontologies of psychopathological anxiety, as well as new kinds of interpretations of anxious bodies?²⁸ As I emphasized at the start of this article, the affect of anxiety was not front and center in the investigators' field of vision as these experiments commenced. The clinical researchers did not start with a series of questions about how to understand the phenomenology of agoraphobic anxiety; rather, the hinge that shifted the analytical plane and that served to open up the problematic of anxiety was the introduction of the psychopharmacological substances themselves.

Hillside Hospital, where those experiments took place, is located in Glen Oaks, Long Island, New York.²⁹ In 1954, the hospital established research programs "devoted to an understanding of the mode of action of the psychiatric therapies of the hospital."³⁰ Hillside Hospital was a Jewish hospital—which, in that period, meant being located beyond the orbit and the sphere of influence of the large, university-affiliated research hospitals—and was, in the late 1950s, largely focused around psychoanalytic therapies. Max Fink, who headed the experimental psychiatry research program, and his colleague Donald Klein (who at that time was a research associate and a mental health career investigator funded through NIMH) were developing new methodologies through which to investigate the mode of action of drug therapies. And if they were preoccupied with exactly the problems enumerated in the 1956 conference on the evaluation of pharmacotherapy—"Do the drugs act? How do the drugs act? What if the drugs act?"—they were particularly interested in figuring out the answer to a fourth question: In relation to which kinds of patients?

tion and psychopathological theory, as opposed to a focus on standardized rating scales and target symptoms). Healy, *The Antidepressant Era* (cit. n. 15); Healy, *Creation* (cit. n. 16); Balz, "Terra Incognita" (cit. n. 15); Balz and Hess, "Psychopathology" (cit. n. 16); Viola Balz and Matthias Hoheisel, "East-Side Story: The Standardisation of Psychotropic Drugs at the Charité Psychiatric Clinic, 1955–1970," *Stud. Hist. Phil. Biol. Biomed. Sci.* 42 (2011): 453–66.

²⁸ See Deborah Blythe Doroshov on Fink's therapeutic practices as regards insulin coma therapy. Doroshov, "Performing a Cure for Schizophrenia: Insulin Coma Therapy on the Wards," *J. Hist. Med. Allied Sci.* 62 (2007): 213–43.

²⁹ "Hillside Hospital is a 196-bed, open ward, voluntary psychiatric facility for the treatment of patients with early and acute mental disorders whose stay is independent of their ability to pay. All patients are seen in individual psychotherapy, with the expectation that psychotherapy should be given a trial prior to other measures. Somatic therapies are employed by joint decision of the resident therapist and supervising psychiatrist, with the management of medication restricted to the research staff"; Donald F. Klein and Max Fink, "Psychiatric Reaction Patterns to Imipramine," *Am J. Psychiat.* 119 (1962): 432–8, on 432. See also Irving J. Sands, "The First Twenty-Five Years of Hillside Hospital: A Voluntary Psychiatric Hospital," *J. Hillside Hospital* 2 (1953): 199–206; Robert L. Kahn, Max Pollack, and Max Fink, "Social Factors in the Selection of Therapy," *J. Hillside Hospital* 6 (1957): 216–28.

³⁰ Max Fink, "Experimental Psychiatric Research at Hillside: Review and Prospect," *J. Hillside Hospital* 10 (1961): 159–69.

They therefore created a laboratory within the psychiatric hospital and established a system through which to control the prescription of psychotropic drugs throughout the hospital.³¹ They put procedures in place: all prescriptions were dispensed by a psychiatrist—Klein—within the Department of Experimental Psychiatry, who responded to a request made by the therapist of a particular patient and interviewed the patient prior to dispensing the drug. During the period of drug therapy, the patient's response was assessed weekly, and in a variety of ways, from the perspective of various individuals—by the patients themselves, by ward staff, by the patient's therapist, and by the therapist's supervisor. (We shall return shortly to how those perspectives were weighted on the basis of authority and clinical importance.) The dosage and the type of medication could be altered. From October 1958 to October 1959, Klein and Fink treated 120 patients with chlorpromazine, promazine, or prochlorperazine (all phenothiazines) and eighty-seven patients with imipramine. Imipramine was, at that point, a new drug that had emerged through the Swiss psychiatrist Roland Kuhn experimentally examining the effects of a Geigy compound. (This compound was similar in structure to that of chlorpromazine; it did not appear to have much effect on psychotic symptoms but did appear to reduce the depression of patients diagnosed with schizophrenia. Imipramine is now commonly described as the first tricyclic antidepressant.)³² In an associated study, a total of 215 patients received only imipramine between October 1959 and July 1961; Klein and Fink published two seminal papers relating to those two studies.³³ Subsequently, after the therapeutic success of those early, open experiments, a randomized placebo trial was carried out.³⁴ In assessing drug action, Klein and Fink paid attention to what they designated “changes in mental status and hospital adjustment,” “progress in psychotherapy,” and “utilization of hospital facilities.” Crucially, the patient's diagnosis was not at stake in the decision over which drug to prescribe, and Klein and Fink also argued that current psychodynamic formulations were of no help in predicting course of illness or treatment. They therefore jettisoned both existing diagnostic classifications and all psychodynamic formulations; instead, they aimed to set aside commonly used frameworks of adjudication the better to attend to the bodies and actions of those patients who had received drugs from the prescribing physician. “Present techniques of evaluating therapies by global improvement scores, imprecise diagnostic classification, and

³¹ Psychoanalysts at that time were not keen to prescribe drugs, seeing them as disruptive of the transference relationship between analyst and patient. In many hospitals, one doctor would be designated the “druggist”: he or she would prescribe drugs, leaving the remaining psychiatrists free to conduct psychotherapy/psychoanalysis (see Healy, *Antidepressant Era* [cit. n. 15], 191). My account of Klein and Fink's experiments is greatly indebted to their own two journal publications documenting those early studies, as well as Klein's retrospective reflections on these experiments. See Donald F. Klein and Max Fink, “Behavioral Reaction Patterns with Phenothiazine,” *Arch. Gen. Psychiat.* 7 (1962): 449–59; Klein and Fink, “Psychiatric Reaction Patterns” (cit. n. 29); Klein, “Anxiety Reconceptualized” (cit. n. 6); Donald Klein and David Healy, “Donald Klein: Reaction Patterns to Psychotropic Drugs and the Discovery of Panic Disorder,” in Healy, *The Psychopharmacologists* (cit. n. 17), 1:329–52.

³² Walter A. Brown and Maria Rosdolsky, “The Clinical Discovery of Imipramine,” *Amer. J. Psychiat.* 172 (2015): 426–9.

³³ Klein and Fink, “Psychiatric Reaction Patterns” (cit. n. 29); Klein and Fink, “Behavioral Reaction Patterns” (cit. n. 31).

³⁴ My focus here is purely on the early experiments that operated outside of the logic of a controlled, placebo trial. The later fixed dosage, double-blind study was reported in Donald F. Klein, “Importance of Psychiatric Diagnosis in Prediction of Clinical Drug Effects,” *Arch. Gen. Psychiat.* 16 (1967): 118–26.

target symptoms abstracted from their context were,” they emphasized, “felt to be methodologically inadequate.”³⁵ Klein and Fink documented eight distinct patterns of “behavior change” for those treated with the phenothiazines and seven for those treated with imipramine. Their underlying claim was that the interaction of particular patients with particular drugs allowed the identification of distinct “reaction patterns.” The “descriptive behavioral typology” that allowed those reaction patterns to become visible was produced through three research psychiatrists reviewing the patients’ detailed records and coming to a consensus “concerning the patient’s behavioral reaction during the medication period.”³⁶

For our purposes, what is crucial is the pattern that Klein and Fink identified as relating to “episodic anxiety”: it was this pattern that would, in time, become renamed and reimagined as “panic disorder.”³⁷ Those patients grouped under episodic anxiety were variously characterized, before treatment, as experiencing “episodes of felt anxiety and helplessness, associated with fearful clinging and urgent demands for aid,”³⁸ or as experiencing “the sudden onset of inexplicable ‘panic’ attacks, accompanied by rapid breathing, palpitations, weakness, and a feeling of impending death.”³⁹ Notably, they defined the “hallmark” of those patients’ condition as “expectant fear of lack of support when overwhelmed” (though they also noted that “their condition was often referred to as agoraphobia”).⁴⁰ With phenothiazine (“major tranquilizer”) treatment, these patients’ “episodic anxiety” was unaffected: while the tension they experienced might sometimes be reduced, “depressive complaints were not alleviated and phobic limitations on activity continued.”⁴¹ With imipramine, in notable contrast, “the ‘panic’ attacks ceased, . . . [although] the patients were reluctant to change their phobic behavior pattern and required much persuasion, direction and support.”⁴² The psychiatrists noted, furthermore, in those patients showing a positive reaction to imipramine treatment, “a surprising rise in aggressive self-assertion and rejection in domination”; response to imipramine showed “no special relationship to age or sex.”⁴³

Episodic anxiety patients provoked particular analytic attention from Klein and Fink “because of the apparently paradoxical nature of their drug response”: while the patients were clearly very anxious, the phenothiazines—tranquilizers—strangely had no effect, either on the particular quality of their anxiety or on their “phobic limitations.” (These limitations included the behaviors of some patients who, “between episodes [of anxiety] . . . manipulated the staff to enable them to remain within the

³⁵ Klein and Fink, “Psychiatric Reaction Patterns” (cit. n. 29), 432.

³⁶ Klein and Fink, “Behavioral Reaction Patterns” (cit. n. 31), 449.

³⁷ The term “panic disorder” did not emerge until the late 1970s; throughout the 1960s, Klein—while referring to attacks of panic—continued to use anxiety as the dominant nosological term. For example, in 1967, he referred to the “phobic-anxiety reaction”; Klein, “Importance of Psychiatric Diagnosis” (cit. n. 34), 121. In Klein’s book on diagnosis, coauthored by John M. Davis and published in the late 1960s, he and Davis defined “panic anxiety” as “the state of being suddenly overwhelmed by fearful sensations,” which is “accompanied by massive autonomic responses, both sympathetic and parasympathetic.” See Klein and Davis, *Diagnosis and Drug Treatment of Psychiatric Disorders* (Baltimore, 1969), 325.

³⁸ Klein and Fink, “Behavioral Reaction Patterns” (cit. n. 31), 456.

³⁹ Klein and Fink, “Psychiatric Reaction Patterns” (cit. n. 29), 435.

⁴⁰ *Ibid.*

⁴¹ Klein and Fink, “Behavioral Reaction Patterns” (cit. n. 31), 456.

⁴² Klein and Fink, “Psychiatric Reaction Patterns” (cit. n. 29), 436.

⁴³ *Ibid.*

phobically defined safe areas or to have constant attendance by aides.”)⁴⁴ Why were phenothiazines not effective for those patients, whereas imipramine was effective? If Klein and Fink had dramatically succeeded in rendering visible distinctive reaction patterns among the patients in their cohort, they still needed to explain why imipramine helped patients with episodic anxiety.

PANIC DISORDER’S ORIGIN STORY

Klein offered a number of retrospective reflections on his early experiments at Hillside Hospital, in which he set out, in characteristically vigorous prose, his explanation for the action of imipramine.⁴⁵ Notably, Klein chose to exemplify the stakes of his account by turning to clinical observations that were made regarding one particular male patient. Clinical descriptions of this patient’s suffering and treatment might, indeed, be classified as the origin story of the nosological category of panic disorder.⁴⁶ Through considering this origin tale in some detail, we will be able to discern the relation that was traced between the practice of observation and the phenomenon that was its object—the relation that underpinned Klein’s practice of psychopharmacological dissection.

The story begins in Hillside Hospital with the patient’s doctor calling Klein. The doctor described his patient as schizophrenic and told Klein that treatment with the phenothiazine chlorpromazine had made the patient worse. Klein was not convinced that the patient was schizophrenic: he was neither delusional nor hallucinating and manifested no thought disorder or restriction of affect. He was, however, Klein emphasized, “hideously anxious, extremely dependent, extremely demanding.”⁴⁷ Klein described this experimental and clinical situation as one that allowed him to bring into the same terrain a “patient we didn’t know what to do with” and “a drug [imipramine] . . . we were unsure what it did.” He “mixed them together” through a process that he characterized as “pure empiricism.”⁴⁸ After a couple of weeks of imipramine treatment, there appeared to be no change in the patient’s symptoms and the patient was complaining bitterly of his continuing anguish. After the third or fourth week, however, the nurses in the wards felt that something had altered, though they were unable to put their finger on quite what that was; neither the patient nor his therapist nor the therapist’s supervisor believed there to be any change. Finally, one nurse—whom Klein described a number of times as a “good observer”—pointed out that the patient was no longer running to the nurses every few hours wanting help and feeling as though he were dying.⁴⁹ After several more weeks, Klein averred that those improve-

⁴⁴ Donald F. Klein, “Delineation of Two Drug-Responsive Anxiety Syndromes,” *Psychopharmacologia* 5 (1964): 397, 398.

⁴⁵ Klein’s theoretical architecture would become more elaborate over time, though he has not departed from the basic logic of his early accounts.

⁴⁶ The story of this “originary” patient is elaborated in Klein, “Anxiety Reconceptualized” (cit. n. 6); Klein and Healy, “Donald Klein” (cit. n. 31), 311. Klein also spoke at some length about this patient in his interview with me (Donald Klein, interview by Felicity Callard, digital recording, The Hamptons, New York, 28–30 July 2011). In “Anxiety Reconceptualized,” the one patient becomes a group of patients, although the structure of the narrative is the same. I refer to a singular patient.

⁴⁷ Klein and Healy, “Donald Klein” (cit. n. 31), 331.

⁴⁸ *Ibid.*

⁴⁹ Klein, in his interview with me, said “one of [the ward staff] who’s a good observer said, ‘You know, this guy’s been coming to the nursing station four times a day for the past nine months saying he’s dying.’” (Klein, interview by Callard [cit. n. 46]).

ments could not be discounted—even though the patient’s own explanation for his behavior was that he had finally realized the nurses could do nothing for him and that he was therefore no longer running to them. Indeed, the good doctor is required to point out changed behavior to the unknowing patient: according to Klein’s account, the patient was “stunned” since “he had no idea he had changed his behavior.”⁵⁰

Klein described how he and his colleagues were initially puzzled by the strange turn of events. Was the patient primarily depressed rather than anxious or phobic, such that the imipramine, with its antidepressant qualities, was lifting his depression and simultaneously alleviating his anxiety symptoms? Klein noted, however, that “most of the patients [within the episodic anxiety group] neither looked nor acted depressed,” and “thoughts of suicide, guilt, and depressive ideas of reference were conspicuously absent.”⁵¹

What Klein characterized as a scene of “pure empiricism” became a scene that—through the interlocking actions of a drug whose action was uncertain and of observers who were not sure what they might be on the lookout for—unfurled two distinct ontologies of psychopathological anxiety. Klein argued that observations of this ordinary patient allowed him to parse anxiety into two kinds, in contradistinction to the prevailing psychoanalytic model of anxiety.⁵² Klein installed “a physiological discontinuity” between what he came to term “paroxysmal anxiety” (which was manifested, e.g., when the patient ran to the nurses) and “chronic anxiety” (from which the patients suffered most of the time).⁵³ He interpreted imipramine as acting on the paroxysmal anxiety but having no effect on the chronic anxiety because that anxiety was of a different order. Now that Klein had divided anxiety into two phenomenological and physiological kinds, he was able—in subsequent research and publications—to clarify the link between them. The various phobias that beset patients like the “ordinary” patient, as well as those patients’ chronic and anticipatory anxiety, were all directed toward the avoidance of panic attacks:

In other words, what they feared was having a panic attack, particularly having one while in a helpless situation. We began to understand why such patients would not drive over a bridge or into a tunnel. The simple answer, without resort to psychoanalytic symbolism, was that they realized that once they had committed themselves to a bridge or a tunnel there would be no way to stop, so that if a panic attack occurred, they would be completely helpless and isolated.⁵⁴

Klein emphasized that patients’ intense attacks of anxiety came first and were subsequently followed by the patterns of phobic avoidance, general anxiety, and depressed mood. The patients, he claimed, did not realize the difference between the two kinds

⁵⁰ Klein and Healy, “Donald Klein” (cit. n. 31), 331.

⁵¹ Klein, “Anxiety Reconceptualized” (cit. n. 6), 239.

⁵² For a classic psychoanalytic account of anxiety advocated at that moment, see Leo Rangell, “On the Psychoanalytic Theory of Anxiety: A Statement of a Unitary Theory,” *J. Amer. Psychoanal. Assoc.* 3 (1955): 389–414.

⁵³ Klein, “Anxiety Reconceptualized” (cit. n. 6), 239.

⁵⁴ *Ibid.*, 240–1. Notably, *DSM-III* carried a specific emphasis on helplessness in the description for Panic Disorder (300.01): “A common complication of this disorder is the development of an anticipatory fear of helplessness or loss of control during a panic attack, so that the individual becomes reluctant to be alone or in public places away from home” (American Psychiatric Association, *Diagnostic and Statistical Manual* [cit. n. 4], 230).

of anxiety because the chronic anxiety submerged the particularity of the panic attacks. (The originary patient's claim, then, that he was no longer running to the nurses because he realized they could do nothing for him, was, on Klein's account, an erroneous post hoc explanation.) Chronic anxiety remained, Klein explained, because although imipramine brought patients' panic under control, the patients did not know or believe that this would remain the case. Thus their anticipatory anxiety remained and kept in place their avoidant mechanisms (the phobic limitations). Klein's formulations would, in time, assist in establishing panic as a central topic for research and treatment. Klein came to understand panic as a kind of "spontaneous" attack resulting from a dysfunctional somatic mechanism; he argued that imipramine normalized this dysfunctionality.⁵⁵ This, I argue, helped to transform the locus of clinical intervention in cases of panic: consideration of the situations or places in which paroxysmal anxiety had occurred was of secondary interest since the primary question was how to cure the defective somatic mechanism—which produced the panic—pharmacologically.⁵⁶

Klein's formulations—first developed in his articles from the 1960s, though continuing to this day⁵⁷—turned upside down established psychiatric wisdom concerning the development of paroxysmal anxiety out of chronic anxiety (a formulation that had loosely followed Freud's understanding of anxiety neurosis).⁵⁸ They also shifted the mise-en-scène of agoraphobia that had been in place since Westphal's first inquiries into agoraphobia in the 1870s. The scene of Klein's pharmacological dissections—the hospital ward and a panic-stricken inpatient running to his nurses—moved the spatial imaginary of the disorder away from the streets and squares that had until then formed the primary stage for agoraphobic behavior. That Klein's originary panic disorder patient was male rather than female also marked a break with many psychiatric and psychoanalytic commonplaces concerning women and agoraphobia.⁵⁹ Klein's model replaced the backdrop of public space with the drama of a terrified figure running to be comforted in the closeted space of a hospital ward: at the center of the dis-

⁵⁵ See, e.g., Donald F. Klein et al., *Diagnosis and Drug Treatment of Psychiatric Disorders: Adults and Children* (Baltimore, 1980).

⁵⁶ Klein, "Anxiety Reconceptualized" (cit. n. 6), 239. Klein, in the latter part of the 1960s, buttressed his own results by citing F. N. Pitts and J. N. McClure, "Lactate Metabolism in Anxiety Neurosis," *New Engl. J. Med.* 277 (1967): 1329–36. Pitts and McClure had run a study showing that intravenous lactate infusions bring on a panic attack in those people who suffer from spontaneous panic attacks but rarely have any effect on normal individuals. For Klein, the lactate-induced panic seemed to mirror, and provide grounded confirmation of, his model of the spontaneous panic attack. Orr has argued that Klein's "panic-disordered body is defined by an absence of relation to any social reason for the force or the timing of its terror. Even within its classificatory family of 'anxiety disorders,' panic disorder stands out as the psychic response to no discernible stimulus" (Orr, *Panic Diaries* [cit. n. 13], 174; emphasis in the original).

⁵⁷ Klein remains a prolific scientific author. In more recent years, he has championed "serendipity" in psychopharmacology, which he sees as central to the psychopharmacological successes of the 1950s and 1960s, and which he believes to have been wrongly pushed to the side by the logic of rational drug development. See, e.g., Donald F. Klein, "The Loss of Serendipity in Psychopharmacology," *J. Amer. Med. Assoc.* 299 (2008): 1063–5.

⁵⁸ Sigmund Freud, "On the Grounds for Detaching a Particular Syndrome from Neurasthenia under the Description 'Anxiety Neurosis,'" in *The Standard Edition of the Complete Psychological Works of Sigmund Freud (1893–1899)*, vol. 3, ed. James Strachey (London, 1895), 85–115.

⁵⁹ Many post-World War II approaches to agoraphobia associated it with particular kinds of femininity; see, e.g., D. Buglass et al., "A Study of Agoraphobic Housewives," *Psychol. Med.* 7 (1977): 73–86.

order lay not a problem in negotiating public spaces of exchange and sociality but a problem of dependency and need for a substitute mother figure. The tumult of the city receded; a small-scale, intimate parent-child drama took its place.

* * *

In the remainder of this article I want to consider in greater detail the practices of clinical observation—as well as what I shall call their “intimate geographies”—that accompanied those early, creative experiments by Klein and Fink at Hillside Hospital. These are, I believe, central to understanding how those experiments helped focus attention on a psychopathological manifestation of affect that would, in time, allow the inauguration of the new nosological category of panic disorder. My interest lies in understanding how Klein and Fink responded to the challenges posed in the 1956 conference on the evaluation of pharmacotherapy, and how their experiments mobilized particular formulations of psychopathological affect. The ordinary scene that Klein described under a rubric of “pure empiricism” drew together a complex network of material objects (e.g., the drug imipramine), socio-spatial settings (spaces of psychotherapeutic consultation vs. the regular space of the ward), discursive elements (the speech of patients, therapists, ward staff), bodily movements (patients running, or not running, to their nurses), and changes in affective rhythms and demeanors (e.g., increases in patients’ “aggressive self-assertion” upon taking imipramine). For psychopathological anxiety to be transformed from one into two ontologically distinct kinds, which elements within this network were prioritized and valorized, and which, ultimately, were ignored? The 1956 conference had set out multiple ways of traversing and mapping a dense and heterogeneous landscape so as to determine whether, how, and with what consequences drugs might “act.” But how widely did Klein and Fink’s map extend? How did it end up validating some elements within that landscape and occluding others? And is the concept of “purity” (namely, Klein’s claim of “pure empiricism”) apposite in characterizing that scientific and clinical scene?

OBSERVING BEHAVIOR

Central to Klein and Fink’s framework for adjudicating drug action was their notion of a “behavioral reaction pattern.”⁶⁰ How did they conceptualize behavior, and what role did affect play? Notably, changes in affect were one of the five criteria—alongside changes in symptoms, patterns of communication, and participation in psychotherapy and social activity—they used to divide patients into groups.⁶¹ But what was meant by affect? The researchers set great store on “gain[ing] a *broad image* of the patient’s behavior”: not only did they shy away from the enumeration of “simple lists of traits and symptoms,” but they also deemed batteries of psychological, psychiatric, and behavioral indices to be of little use in assisting in the carving out of relevant patient subpopulations.⁶² Of the eight behavior change clusters that Klein and Fink

⁶⁰ Klein and Fink’s collaborative work followed earlier work by Fink in which he had examined behavioral patterns to explore the effects of convulsive therapy. See Max Fink, “A Unified Theory of the Action of Physiodynamic Therapies,” *J. Hillside Hospital* 6 (1957): 197–206; Fink, R. L. Kahn, and M. A. Green, “Psychological Factors Affecting Individual Differences in Behavioral Response to Convulsive Therapy,” *J. Nerv. Mental Disease* 128 (1959): 243–8. Fink would go on to become one of the world’s leading electroconvulsive therapy researchers.

⁶¹ Klein and Fink, “Behavioral Reaction Patterns” (cit. n. 31), 449, 457.

⁶² *Ibid.*; emphasis added.

enumerated, several centered on affect (e.g., “reduction of anger,” “affective stability,” and “unaffected episodic anxiety”). Affect, then, was embedded within and helped to constitute the “broad image” of behavior—and was addressed via patient demeanor, gestures, actions, and expressions. It appeared in a variety of forms and was underwritten by different kinds of evidence that was gathered via different kinds of observations by different kinds of people. Those patients placed by Klein and Fink in the reaction pattern group “Suppressive Denial,” for example, were distinguished by “a fearful suspiciousness accompanied by derogatory ideas of reference.” Claims that they are “fearful, agitated, and panicky” were grounded in references to their speech being “evasive” or “guarded,” and their social interactions being “hostile, fearfully demanding, and leading to mutual withdrawal.” Crucially, one source of observational evidence was the affective reactions in those staff interacting with them: these patients were described as “engender[ing] uncomfortable feelings in staff personnel, with fears of assaultive behavior.”⁶³ In comparison, the “somatizing” group was characterized not only by patients’ “chronic use of bodily complaints” but by “manipulation as a basis for interpersonal relatedness.” Manipulation was evidenced by a fascinating range of affectively tinged behaviors that were interpreted through a contrast between patients’ outward expression and their “inner states.” Before treatment with phenothiazines, for example, somatizing patients were deemed to be

friendly during those interactions where they felt that they were about to get their way, and depressed, fearful, reproachful, sulky, and covertly angry when their demands were denied. Their symptoms and affective upheavals were most prominent in relation to the medical staff, appearing to be role-playing devices rather than expressions of inner states.⁶⁴

After treatment with phenothiazines, these patients responded with “heightened manipulation”: the evidence that Klein and Fink marshaled here included “histrionic demonstrations of physical distress such as slumping slowly to the floor, wearing a wet towel around the head, walking around the corridors leaning against or touching the wall or using both hands on the stair bannister,” and the abandoning of hospital activities “as another gesture of helpless distress.” Klein and Fink concluded that these patients’ somatic and affective complaints seemed “best understood as manipulative communications rather than the direct expression of anxiety or depression”; they claimed, furthermore, that “secondary gain is marked, and their illness is utilized in an attempt to maintain a protected dependent status.”⁶⁵

What is noticeable in these descriptions is the range of different frameworks used to characterize and interpret both patients’ actions and their displays of affect. Descriptions of affect frequently embedded affect within an account of social interaction (either between patients or between a patient and a member of clinical staff) or a verbal exchange (between a patient and his/her psychotherapist or with a member of ward staff). Not infrequently, evidence was given that was not necessarily about affect witnessed in the patient, but that comprised feelings invoked by the patient in the attending clinical staff. (Clinical staff might have been turning, here, to psychoana-

⁶³ Ibid., 451.

⁶⁴ Ibid., 454.

⁶⁵ Ibid.

lytic principles concerning countertransference, or to other models in which personal feeling was relied upon to assist with diagnosis—such as those indebted to Rümke’s “Praecox Gefühl,” which was used to identify schizophrenia.)⁶⁶ Sometimes there was the implication that interpretations were being made of patients’ bodily and/or facial demeanor, or of the affective timbre of their speech (e.g., “patients now approached the interviewer in an ingratiating manner,” or patients “expressed boredom with hospital routine”).⁶⁷ Sometimes, an affectively tinged descriptor—a patient appearing “fearful” or “helpless”—was associated with (inauthentic) “role-play” that ran counter to the inner state, and at other times it was invoked as an apparent endorsement of the patient’s authentic affective state. Affective displays were often linked to particular kinds of encounters in particular socio-spatial contexts (e.g., differences were noted between how the patient might behave in the context of a psychotherapeutic encounter, in comparison with social interactions on the wards). This array of frameworks and modes of gathering evidence makes us aware of how heterogeneous the practices of observing, assessing, and interpreting patient behavior and affect appear to have been within the psychiatric hospital at that moment.⁶⁸ Eric J. Engstrom, in his analysis of Kraepelin’s late nineteenth- and early twentieth-century interest in the role of emotions in psychiatric illness, emphasizes Kraepelin’s desire—as manifested in his diagnostic cards [*Zählkarten*—to develop “reliable diagnostic techniques that, in turn, would lead the way toward greater prognostic certainty in day-to-day clinical practice.”⁶⁹ How to identify and document details regarding a patient’s behavior, emotions, and cognitive abilities—and how to relate these to a diagnosis—remained a challenge for psychiatry through the course of the twentieth century. Nosological schemas and modes of identifying and classifying symptoms, behaviors, and affects remained labile and heterogeneous. Indeed, the fact that there was no uniformly accepted method through which to evaluate the patient—and the effect of the drugs on him or her—was one strong impetus behind the 1956 conference on “The Evaluation of Pharmacotherapy in Mental Illness” discussed earlier.

We see how Klein’s and Fink’s published texts intermingle psychoanalytic principles and techniques (e.g., the concept of “secondary gain”), both so-called folk and scientific descriptors of affect, and various clinical frameworks used to describe phenomenology, symptomatology, and psychopathology (e.g., the concept of “ideas of reference”). They always inserted affect into a broader hermeneutic matrix through which to assess changes in the patient as a whole. Klein and Fink critiqued the use of “target symptoms” in relation to psychopharmacological research—arguing that such a model, by erroneously assuming that each manifestation of affect (in the context of a psychopathological symptom) was “identical in nature from patient to pa-

⁶⁶ H. C. Rümke, “Das Kernsymptom der Schizophrenie und das ‘Praecox Gefühl,’” *Zentralbl. Gesam. Neurol. Psychiat.* 102 (1942): 168–9.

⁶⁷ Klein and Fink, “Behavioral Reaction Patterns” (cit. n. 31), 451.

⁶⁸ For additional insights into how Klein is likely to have been assessing patient behavior (including affective behavior), see Loring L. Burnett and Donald F. Klein, “A Guide for the Psychiatric Case Study,” *J. Hillside Hospital* 14 (1965): 54–68. In this guide, “Affect” (which was considered under the heading “Direct Observations”) included such diverse subheadings as “Tension: level, fluctuation, startle reactions”; “Anxiety: fidgeting, blushing, wet palms”; “Mood: apathy, expansiveness, depression, exhilaration, fluctuation”; “Expressiveness: inhibited, spontaneous, impulsive”; and “Genuineness vs. simulation: exaggerated, feigned.”

⁶⁹ Eric J. Engstrom, “Tempering Madness: Emil Kraepelin’s Research on Affective Disorders,” in this volume.

tient,”⁷⁰ ended up “implicitly promot[ing] a mosaic view of psychopathology.” They went to some lengths to convey how any particular affect might be exacerbated or attenuated in very different ways according to the distinctive behavioral typology in which it appeared. (For example, phenothiazine treatment had, they argued, very different effects on behaviors understood as “anxiety,” depending on the overall behavioral reaction group of which anxiety was one part: in the “somatizing” group, anxiety after phenothiazine treatment became “markedly accentuated with much dramatic expressiveness, when dealing with psychiatric staff but it was not apparent during the patient’s social intercourse,” whereas “anxiety” in the “episodic anxiety” was entirely unchanged.)⁷¹ They concluded that “each symptom represents a prominent facet of various complex adaptations . . . which can be most fruitfully described in a patterned multivariate context.”⁷²

What allowed the identification of those temporally and spatially patterned accounts of changes in patients’ affective demeanors, gestures, and expressions was, Klein and Fink made clear, long-term, “expert observation” by psychiatric researchers who knew the patients. They disparaged the approach taken in many large hospital programs, where many patients were examined and tested by several raters who were not able to have prolonged clinical contact with patients, arguing that “the experienced clinician is our most sensitive cluster analytic device, given the opportunity to use his skills.” (“Cluster analysis” emerged in the 1950s, and clustering algorithms began to be used in psychiatry in an attempt to cluster different groups of patients according to symptomatology. That Klein and Fink believed the individual, highly trained observer to trump the technological potency of clustering algorithms emphasized how sophisticated they believed that observer’s techniques of parsing, amalgamating, and discerning to be.)⁷³ In contrast to the contemporaneous use of rating scales, such as Max Hamilton’s “Assessment of Anxiety States,” which was published in 1959, Klein and Fink’s approach constituted the patient’s body as a distinct and complex entity that existed in relation to other bodies in particular social settings: it was not something that could be dismantled and disaggregated into a tessellation of target symptoms.⁷⁴

But not all “expert observation” was regarded with equal esteem by Klein and Fink. The observers and practices documented in their early publications made up a collectivity—including Klein and Fink themselves, the ward staff, the psychoanalytic psychotherapists and their supervisors, the nursing aides, and the patients—in which certain kinds of observation were privileged over others. The impact of this privileging became clear when Klein and Fink’s favored approach and another mode of observation yielded different judgments about a patient. For example, Klein and Fink, in critiquing the reliance in many psychopharmacological evaluations on sim-

⁷⁰ Klein and Fink, “Behavioral Reaction Patterns” (cit. n. 31), 457.

⁷¹ *Ibid.*, 458.

⁷² *Ibid.*

⁷³ Fionn Murtagh, “The History of Cluster Analysis,” in *Visualization and Verbalization of Data*, ed. Jörg Blasius and Michael Greenacre (Boca Raton, Fla., 2014). See also Harvey A. Skinner and Roger K. Blashfield, “Increasing the Impact of Cluster Analysis Research: The Case of Psychiatric Classification,” *J. Consult. Clin. Psychol.* 50 (1982): 727–35.

⁷⁴ Hamilton, “Assessment”; Worboys, “Hamilton Rating Scale” (both cit. n. 21). For a summary of the use of scales and checklists in the 1950s, see Richard L. Jenkins and Maurice Lorr, “Symptom Scales and Check Lists for Determining Symptomatic Improvement in Psychotic Patients,” in Cole and Gerard, *Psychopharmacology* (cit. n. 18), 469–77.

plistic psychotherapeutic notions of “cured” or “improved,” argued for a means by which the “rich complexity of behavioral change” might be registered. Such complexity would not, they implied, be recognized by psychotherapists, who might rate behavior change negatively because it interfered with the progress of psychotherapy, or by ward staff (who might rate the change as “positive” because they perceived the nursing burden to be alleviated).⁷⁵ Their research also downplayed the robustness of much of the evidence gleaned via linguistic utterances (remember Klein’s account of the originary patient—in which the therapist discerned no difference in therapy after the ingestion of imipramine, and the patient himself was interpreted as providing erroneous explanations for his own actions). Theodore Porter’s historical research on different forms of scientific objectivity is helpful here in allowing us to discern the professional and disciplinary jostling over when and how individual expertise ought to be trusted over forms of “mechanical objectivity” (such as scales or checklists). Klein and Fink are implicitly defending one kind of clinical observation as far more epistemologically robust than another kind of clinical observation (poor expertise in the form of psychoanalytic techniques of observation), and also more robust than the use of symptom checklists (a poor example of mechanical objectivity).⁷⁶

Klein and Fink attempted to position observations of behavioral changes as a way of sidestepping some of the difficulties attendant upon observing and adjudicating changes in patients. But behavior was not as pellucid a means of capturing the potential effects of drug action as Klein and Fink might have wished it to have been. As we have already seen, the means by which they brought attention to particular kinds of behaviors rather than others was inflected by their interpretations of how behavior emerged in the context of particular kinds of communicative actions. Klein and Fink appeared to interpret some behaviors as not possessing ambivalent psychic overlays (e.g., the “helplessness” of the patients who ran to the nurses when beset by panic attacks), whereas other behaviors were associated with complex psychic motivations (the “dependent façade” of the somaticizing group, who engaged in “role-playing” with the nurses, and whose “helpless distress” was seen as a manipulative “gesture”). In short, Klein and Fink worked with a complex hermeneutics that ended up pulling particular affects and behaviors into analytical visibility and left others, no doubt, in the shadows. This is perhaps particularly striking in relation to the prowess of that “keen clinical observer”⁷⁷—who was judged to have spotted the core of what was happening in relation to the “helpless” originary patient. For while there were surely multiple behavioral transformations that might have been noted after treatment with imipramine, what actually was foregrounded and endowed with the greatest significance was the fact that the patient was deemed to have stopped running to the nurses several times a day. It was the cessation of a particular kind of locomotor behavior—over and above the timbre and specifics of the affect of anxiety—that was privileged in Klein and Fink’s account. Paroxysmal anxiety became newly visible as a distinct kind of psychopathological affect by dint of the removal (after imipramine treatment) of a particular kind of socially communicative locomotor behavior.

⁷⁵ Klein and Fink, “Behavioral Reaction Patterns” (cit. n. 31), 457.

⁷⁶ Theodore M. Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, N.J., 1995).

⁷⁷ Klein, “Anxiety Reconceptualized” (cit. n. 6), 238.

What were Klein and Fink actually seeing? What did they privilege in this scenario of “pure empiricism”? While they stressed the importance of attending to behavior, their enumeration of particular bodily actions was buttressed by a theoretical framework that underpinned their descriptive typology. After all, the patient’s running was not documented simply as locomotor action but specifically as a manifestation of helplessness and “fearful clinging.” And it was here that the concept of “separation anxiety,” as formulated by the psychoanalyst and ethologist John Bowlby, haunted the empirical scene unfolding in Hillside Hospital.⁷⁸ Bowlby, dissatisfied with the accounts that Freud and later psychoanalysts had provided to explain the relationship between the child and mother, turned to ethology in order to frame attachment as serving a biological, prosurvival function of protection. For Bowlby, anxiety was “a primary response not reducible to other terms and due simply to the rupture of the attachment to [the] mother.”⁷⁹ Klein, borrowing from Bowlby,⁸⁰ decided that early separation anxiety might be a particular kind of evolutionary process. Furthermore, in his seminal paper on separation anxiety, Bowlby had described conditions of isolation for the baby as activating both “crying” and “clinging” in relation to the mother figure: “until he is in close proximity to his familiar mother-figure these instinctual response systems do not cease motivating him,” such that until this outcome is reached “his subjective experience is that of primary anxiety.”⁸¹ We can see here how Bowlby, in shifting the weight of interpretation away from agoraphobic anxiety concerning streets and squares, assisted in establishing a model of phobic anxiety in which attachment figures (particularly the mother) were equated with the environment of the home.⁸² Klein and Fink superimposed Bowlby’s small-scale dyadic scene featuring a crying, clinging child and a reassuring mother onto the figures of an adult male patient and a nurse within the space of the Hillside Hospital psychiatric ward.

⁷⁸ It is of course impossible to know from the published documentation whether Klein and Fink were already on the lookout for manifestations of “separation anxiety” as the early experiments began, or whether their theorizations took place subsequent to the “keen clinical observer” noticing the cessation of the running to the nurses’ station. Klein and Fink’s first paper on imipramine certainly referred to Bowlby’s “separation anxiety,” noting that for Bowlby, “separation anxiety has the biological function of evoking the retrieving and mothering response in a parent. . . . One may speculate that imipramine, in these patients, has some specific reparative effects upon this disordered emotion” (Klein and Fink, “Psychiatric Reaction Patterns” [cit. n. 29], 436).

⁷⁹ John Bowlby, “Separation Anxiety,” *Int. J. Psychoanal.* 41 (1960): 89–113, on 93.

⁸⁰ Klein retrospectively described his turn to the work of Bowlby as follows: he had noticed that many of his agoraphobic patients manifested dependent behavior, and that many had been clinging children, fearful of going to school. Subsequently, he and his colleague Rachel Gittelman-Klein conducted a double-blind, placebo-controlled study of “school phobic” children whose central problem was deemed to be separation anxiety; the results indicated that imipramine was very successful in helping with school phobia. The same drug that apparently blocked panic attacks in adults seemed also to diminish separation anxiety in children. Klein, therefore, again working backward from drug responses, began to consider whether “in some sense, an outbreak of separation anxiety was at the root of agoraphobia” (Klein, “Anxiety Reconceptualized” [cit. n. 6], 245). See Rachel Gittelman-Klein and Donald Klein, “Controlled Imipramine Treatment of School Phobia,” *Arch. Gen. Psychiat.* 25 (1971): 204–7; Gittelman-Klein and Klein, “School Phobia: Diagnostic Considerations in the Light of Imipramine Effects,” *J. Nerv. Mental Disease* 156 (1973): 199–215.

⁸¹ Bowlby, “Separation Anxiety” (cit. n. 79), 93.

⁸² Bowlby’s model of separation anxiety served several purposes for Klein. It manifested the same kind of distaste for “baroque structures” of symbolic interpretation as Klein’s theory of panic. For Bowlby, separation anxiety functioned as a kind of unmediated protest mechanism whose form was very similar to Klein’s understanding of panic as an autonomic discharge of paroxysmal anxiety.

CONCLUSION

Klein and Fink's early psychopharmacological experiments involved a small number of patients and a small number of "expert" clinical observers who inhabited a small psychiatric hospital away from the heart of mainstream large university research centers. In time, those experiments would come to have an impact that was both geographically and conceptually extensive, for Klein's work on pharmacological dissection in Hillside Hospital acted as the germinator for a diagnosis, panic disorder, that is now firmly embedded in multiple countries and across many psychiatric cultures.⁸³ Klein and Fink privileged particular observational practices as they traced a route through a hermeneutically dense terrain composed of heterogeneous patients, heterogeneous drugs, and all manner of "noise" vis-à-vis the behavioral features, linguistic utterances, and affective transformations that might, potentially, be of use in assessing whether and how imipramine acted. Those practices brought a particular manifestation of psychopathological anxiety to center stage. At a historical moment in which the clinical-evaluative drive was toward working with larger numbers of patients, larger research sites, and the use of target symptoms and the development of complex rating scales, Klein and Fink's experiments were characterized by their "intimate geographies."

Those geographies were centered on one research site and entailed the direct observation of patients' bodies—as entire, communicative, and spatially and temporally patterned entities—within the ward by clinical researchers and ward staff who knew those patients well. The intimacy of those geographies was perhaps dramatized most poignantly by the coming to life of Bowlby's separation-anxiety-disordered infant (in need of her mother) in the body of the "episodic anxiety"-disordered male adult patient (in need of the reassurance of the nurse). Whereas Westphal's case histories had referenced Berlin squares that piqued agoraphobics' fear, Klein's narrative of his originary patient was one that displaced the city and replaced it with the drama of a child-and-parent dyad. The agoraphobia of Westphal's patients was exemplified by their stuttering, stalled passage through the public sphere; the panic disorder of Klein's originary patient was exemplified by a frenzied running to the nurse/mother.

Klein and Fink mobilized a complex and creative experimental apparatus comprising heterogeneous bodies and heterogeneous drugs moving within a particular socio-spatial setting. They rendered visible and validated particular interpretations of affective behavior in their consolidation of distinct behavioral reaction patterns—which, in turn, led to Klein's powerful elaboration of the logics of psychopharmacological dissection. David Healy has argued that for Klein and Fink,

the new drugs were an experiment that would lead to new observations. The trick was to remain open-minded enough to see phenomena that available theories did not predict. New theories to explain these observations could be elaborated later. This was almost

⁸³ Admittedly, this was a long process. Many psychiatrists profoundly disagreed with Klein's interpretation of the imipramine findings. Some argued that he had mistaken his patients' symptoms and that the patients were actually suffering from depression; others (e.g., the behaviorist Isaac Marks who was based at the Maudsley Hospital in the United Kingdom) argued that Klein's panic disorder patients should instead be diagnosed as agoraphobic and saw no basis for Klein's new nosological category.

a new form of science, one that acknowledged that techniques drive progress as much as, if not more than, anything else.⁸⁴

But Klein himself has not claimed, in fact, to have made any actual new observations. (He has emphasized that Freud had, in fact, described panic attacks “beautifully” in 1895 but argued that Freud’s “theory prevented his observations.”)⁸⁵ “So, it’s not like it’s a new observation,” stated Klein: “What’s new is that I put it together a different way.”⁸⁶ What was new was a complex socio-spatial assemblage that Fink and Klein put together and set into motion. We need, I argue, to attend to the spatial as well as temporal specifics of this assemblage in order to discern how this new form of psychopathological affect—which would come to be termed panic disorder—emerged and then gained epistemological and ontological consistency. Through Klein and Fink’s experiments, one drug (imipramine) operated in combination with one “good clinical observer”—to which was added the compelling overlay of Bowlby’s figure of mother and child. Such were the elements that brought to center stage one small, affectively dramatic scene. Klein’s analysis of the patient running to the nurse allowed him to “singl[e] out panic attack as being the key variable that was changing with imipramine”⁸⁷ and led, in time, to the consolidation of a new nosology of anxiety.

⁸⁴ Healy, *Creation* (cit. n. 16), 282.

⁸⁵ Klein, “Anxiety Reconceptualized” (cit. n. 6), 245; Klein, interview by Callard (cit. n. 46).

⁸⁶ Klein, “Anxiety Reconceptualized” (cit. n. 6), 245.

⁸⁷ Klein and Healy, “Donald Klein” (cit. n. 31), 331.