The spirit of St Louis: the contributions of Lee N. Robins to North American psychiatric epidemiology

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This article takes up the history of North American psychiatric epidemiology with reference to production of knowledge concerning sociopathic or antisocial personality disorder and drug dependence, abuse, and/or addiction. These overlapping arenas provide a microcosm within which to explore the larger shift of postwar psychiatric epidemiology from community studies based on psychological scales to studies based on specific diagnostic criteria. This paper places the figure of sociologist Lee Nelken Robins within the context of the Department of Psychiatry in the School of Medicine at Washington University in St Louis, Missouri. The St Louis research group—to which Robins was both marginal and central—developed the basis for specific diagnostic criteria and was joined by Robert Spitzer, Jean Endicott and other architects of DSM-III in reorienting American psychiatry towards medical, biological and epidemiological models. Robins was a key linchpin working at the nexus of the psychiatric epidemiological and sociological drug addiction research networks. This article situates her work within the broader set of societal and governmental transformations leading to the technologically sophisticated turn in American psychiatric epidemiology and research on the aetiology of drug abuse and mental health and illness.

Knowledge formations reflect and reproduce the material conditions and cultural preoccupations of the specific moments from which they emerge. Mid-20th century American psychiatric epidemiology first emerged out of postwar community surveys designed to determine the prevalence of psychiatric symptoms within a particular community based on non-specific psychological scales (see March and Oppenheimer, this issue). This article charts the emergence of an approach reliant upon validated diagnostic criteria operationalized through detailed structured interviews conducted by non-psychiatrists. Building this practice into psychiatric epidemiology required an epistemological shift that propelled a set of conceptual and methodological shifts that effectively constituted a new knowledge formation. This paper is an analytical history based on archival and published materials relevant to studies undertaken by sociologist Lee N. Robins, a key figure active in the conceptual and methodological shift from non-specific survey to detailed structured interview.

This shift is traced through a discussion of Robins' use of psychiatric diagnostic criteria for research purposes in three major studies: Deviant Children Grown Up: A Sociological and Psychiatric Study of Sociopathic Personality¹ (1954–66); the Vietnam Experience Study² (1972–74); and the Epidemiological Catchment Area Program³ (1978–80). Her research programme unfolded within an interacting set of epistemological, material, symbolic and discursive forces at play within the Department of Psychiatry in the Washington University School of Medicine in St Louis, Missouri. Robins, whose graduate training in the Department of Sociology at Harvard University
during the 1950s, Lee Robins worked out proof of the centrality of sociological reasoning to psychiatric epidemiology through an intense engagement with the work of Lee Robins. Although trained as a sociologist, Robins played such a pivotal role in this group relegates Lee Robins to a supporting role among these ‘biological rebels’ who became so critical of the nosological approaches of DSM-I and DSM-II that they developed their own diagnostic criteria.

Although the figure of Lee Robins is central to my analysis, this article is not meant to be biographical, celebratory or even a comprehensive tribute. Rather, it argues for the centrality of sociological reasoning to psychiatric epidemiology through an intense engagement with the work of Lee Robins. Although trained as a sociologist, Robins played such a pivotal role in the elaboration of psychiatric epidemiology that by the 1970s her work had come to the attention of key figures in the national drug abuse and mental health leadership. She played a series of roles from which she could legitimate large-scale use of structured interviews and specific diagnostic criteria by shaping the methodologies through which modern psychiatric epidemiology was understood to generate knowledge. She developed and refined the instrument and techniques involved in utilizing detailed structured interviews, administered by trained cadres of lay interviewers, many of whom she personally trained but few of whom were themselves clinicians. Robins influenced the direction of psychiatric epidemiology towards adopting a new nosology reflecting the conceptual tenets of a biological psychiatry; the deployment of a set of statistically validated diagnostic criteria; and the consolidation of a ‘medical model’ in psychiatry.

‘Deviant children grown up’: Robins’ first landmark study in psychiatric epidemiology

During the 1950s, Lee Robins worked out proof of concept for the validity of structured interviews used by non-psychiatrists for longitudinal psychiatric epidemiological studies in an intriguing retrospective study that brought her into direct confrontation with what constituted a psychiatric case for the purposes of epidemiological analysis, as well as confirming the limitations of the 1950s-era diagnostic criteria of DSM-I. Deviant Children Grown Up was a follow-up study based on over 500 cases of children referred to the St Louis Municipal Psychiatric Clinic in the early 1920s. Psychiatrist Patricia O’Neal brought the records from the defunct child guidance clinic at the Malcolm Bliss Psychiatric Hospital to Robins’ attention. Robins and O’Neal considered the clinic records a ‘treasure trove of research materials representing a first step in the study of the natural history of the development of adult antisocial behavior’. This study was unusual in that it relied upon the clinic’s historical records, but required location and a follow-up interview with each subject in the present. Robins and O’Neal sought to identify childhood antecedents for adult disorders at a time when children’s mental health was not typically considered significant in community studies. Dividing available records into three populations—‘antisocial’ children; a control group of ‘disturbed, non-antisocial’ children; and a matched control group of 100 ‘well, non-antisocial’ children—Robins and O’Neal defined a psychiatric syndrome using a longitudinal approach. Their stated goal was to provide data on which childhood behaviour problems signalled poor adult outcomes and which were ‘relatively transient and innocuous’. According to eminent Johns Hopkins child psychiatrist Leon Eisenberg, who wrote the preface to the book-length study published in 1966, Robins demonstrated the ‘shaky underpinnings of many of the “explanations” of behavior so uncritically accepted in our field’.

Chief among the problems that Robins and O’Neal set out to correct were the ‘ill-defined and fluctuating’ diagnostic categories used by the clinic in an era of non-standardized criteria. Despite this problem, the clinic had completed detailed socio-medical histories and a full battery of psychological tests for each child patient. Robins and O’Neal were able to locate 90% of surviving individuals who were child patients 30 years before. They accessed current-day court records and other corroborating evidence of adult behaviour, and interviewed each of them between 1955 and 1960. The detailed structured interview tightly bound to the official nomenclature of psychiatric diagnosis would become the hallmark of Robins’ career. Indeed, the 12th chapter of her first book is titled ‘The validity of the structured interview as a method of obtaining life history data and making a diagnosis’. Subjects were interviewed for 2 hours and 20 minutes on average, and interviewers were trained to summarize subjects’ intelligence, cooperativeness, talkativeness, frankness and mood, in addition to posing 222 questions. Three participating psychiatrists then met to ‘diagnose’ 150 subjects each. Diagnostic categories were assigned on the basis of a summary of all records that showed symptom patterns over time.
Not until the 1960s did the broader field of psychiatry consider lack of consensus on diagnosis and lack of precise diagnostic criteria to be an issue of reliability. Robins systematized the review process and established uniform criteria on the basis of the DSM-I (1952) criteria for ‘sociopathic personality disturbance’, subcategory ‘antisocial reaction’, which she felt most resembled the ‘sociopathic personalities’ in her study. Sociopathic personality was a syndrome characterized by ‘a gross, repetitive failure to conform to societal norms in many areas of life, in the absence of thought disturbance suggesting psychosis’, a definition aligned with the draft DSM-II description of ‘chronically antisocial individuals who are always in trouble’. Finding that the DSM-I criteria said too much—that ‘these persons profit neither from experience nor punishment, and maintain no real loyalties to any person, group or code’—Robins began to reduce criteria to those necessary and sufficient, and to winnow out criteria that could not be operationalized for research purposes. Such detailed sociological attention to psychiatric diagnostic categories was unusual. Robins used the retrospective diagnostic process to demonstrate that children referred to the clinic exhibited high prevalence of psychiatric disease as adults. She concluded that ‘childhood behavior disturbances [were] important prognosticators of certain adult psychiatric illnesses’. Interviews were structured and conducted so as to reveal enough information for a team of three psychiatrists to assign subjects to a diagnostic category—a validation process that required precise questions yielding answers understandable by any clinician as responsive to specific criteria. Occupying Robins for more than a decade, Deviant Children Grown Up gained her a faculty research position in 1962. Among Robins’ departmental contemporaries was her husband, Eli Robins (who chaired the department from 1963 to 1975). Recruited from his residency at Massachusetts General Hospital in 1949, Eli Robins had been mentored by Mandel Cohen, an active critic of the non-scientific basis of psychiatry in the 1940s and 1950s, to whom is credited the conceptual premise of ‘operational criteria’ for the purposes of psychiatric research and diagnosis. Biological psychiatrist Edwin F. Gildea, a close associate of Cohen’s, chaired the Washington University Department but allowed the ‘triumvirate’ to set the research agenda from the mid-1950s onward. This subgroup set out to overcome a negative impression of its research trajectory at the National Institute of Mental Health (NIMH), that did not bode well for its efforts to cultivate federal sponsorship. ‘[S]oon after 1955…Winokur, Robins and I suddenly realized that we were now in a position to try to shape the department in the direction we thought it should go. We didn’t want a psychoanalytic department, we wanted a broad research effort, and we wanted to put tremendous emphasis on improving the diagnostic system in psychiatry. That agreement came about from four or five years of informal discussions…For maybe seven or eight years, we had a lot of trouble getting grants from the NIMH’, Guze recalled in his oral history interview, ‘Had a lot of trouble getting grants to carry out what we would then have called clinical research. There was very little interest in family studies or diagnostic classification at NIMH. That didn’t change until we got into the 1970s’. Guze noted an important exception to the funding drought in Lee Robins’ foundation grant for a study of juvenile delinquency in the late 1950s. Aware of this success, the core group incorporated her help in bringing about the shift to ‘operational criteria’ through statistical validation and attention to sampling, reliability, validity and other sociological concepts and techniques. Indeed, Deviant Children Grown Up can be read as a demonstration of the validity of the structured clinical psychiatric interview research approach to estimating the prevalence of psychiatric disease in a population. As a sociologist among psychiatrists, Robins might have appeared marginal were it not for her centrality to the inner circle of Washington University psychiatrists and for the national and international external recognition that she garnered as her career proceeded. As Guze recounted:

Lee Robins’ career had the advantages and disadvantages of being married to Eli. The advantage mostly came from learning a lot about psychiatry without having any medical training herself. She early on saw the outlines of what might happen for her as a sociologist in a clinical department…Her original study of juvenile delinquents was very well received all over the world, and in a very short time she achieved national and international recognition. The study was very well done and very well described. And this is where being Eli’s spouse was an advantage because her approach was infused with the kind of thinking that we later started to call the ‘medical model’.

Guze’s later manifesto, Why Psychiatry Is a Branch of Medicine, elaborated on what applying a ‘medical model’ to psychiatric disease meant. He dismissed the ‘sociocultural’ model on grounds that it might lead the public to think that psychiatric disease was not ‘real’, although acknowledging the importance of social and environmental factors in the aetiology and progress of psychiatric illness: ‘Psychiatric illnesses, like all illnesses, are most comprehensively conceptualized within a broad epidemiological framework, where health and disease are seen as varying aspects of the organism’s efforts to adapt to its environmental circumstances and history’. The St Louis group entwined the ‘medical model’ of psychiatric disease with a biologically-based psychiatry in which the medical model was embedded...
within knowledge of evolution, neurobiology, cognitive science and genetics. In her summation of *Deviant Children Grown Up*, Robins wrote: 'We have argued that a psychiatric syndrome exists when a common set of symptoms appears in persons with similar age of onset, family histories of the illness, and course of the disease. By these criteria, sociopathic personality does appear to be a psychiatric disease'. Citing evidence that the symptoms of this disease run in families and follow a predictable course, Robins concluded that sociopathic personality was 'Genuinely a psychiatric disease and not simply a pseudo-explanatory, opprobrious term for people with unpopular behavior'. Robins and O’Neal published over a dozen articles between 1958 and the 1966 publication of the book-length study, considering sociological attributes such as health status, deviant behaviour and social class in relation to psychiatric diagnosis in childhood and adulthood. Adult diagnosis was accomplished through the team-based diagnostic classification of the structured interviews, a form of conceptual and methodological labour that displaced emphasis on psychopathology and comprised an early step towards the merging of classification and diagnostic criteria that came to characterize the ‘spirit of St Louis’. However, by the mid-1960s, the St Louis group was not the only research group seeking to loosen the hold of psychoanalytical and psychodynamic concepts of psychopathology on American psychiatry. Most notably, Robert L. Spitzer, from New York State Psychiatric Institute at Columbia University, was seeking to develop criteria for identifying cases of mental disorder to which social and cultural factors could be linked in order to improve the research value of clinical judgments. This project would lead to the 1980 publication of the DSM-III as discussed below. Additionally, the US addiction research network had also become deeply sceptical of psychoanalytical explanations for the phenomenon they were studying.

**Weeding out psychopathology and institutionalizing biological psychiatry: the role of research on drug dependence**

While serving as the first Chief Medical Officer at the US Public Health Service Narcotic Farm located in Lexington, Kentucky (one of two federal drug treatment hospitals operating from 1935 to the mid-1970s), Lawrence Kolb Sr developed the first diagnostic template for addiction, the K-classification system. Early 20th-century scientific investigators sought to identify physiological or psychological markers useful for distinguishing addicts from non-addicts. Kolb strategically adopted the language of psychopathology to protest stigmatization and criminalization of addicts.

The Lexington hospital contained a small, congressionally mandated research unit that included Kolb’s protégé, Robert H. Felix, who became the founding director of the NIMH in 1947. With Felix at its helm, the NIMH influenced both psychiatric epidemiology and the study of narcotic addiction or dependence. In 1948 the research unit at Lexington officially adopted the name of the NIMH Addiction Research Center (ARC).

Multiple pressures turned the older explanatory frameworks based on psychopathology toward a new biological and behavioural psychiatry. NIMH addiction researchers acted as acolytes of biological psychiatry in seeking to purge addiction of associations between psychopathology and immorality. They developed new psychometric methods and instruments such as the Addiction Research Center Inventory (ARCI), a roughly 500-question rating scale still used to translate subjective drug effects into quantitative, objective scales. As the instrument became a complex, multi-scalar, validated inventory, it displaced reliance on Kolb’s classification system. Whereas Kolb’s motivation had been to displace criminalization by advocating a more scientific mental disease paradigm, his successors pried addiction away from psychoanalytical accounts of psychopathic personality. Expanding socio-technical capacity enabled NIMH to sponsor longitudinal studies such as George Vaillant’s retrospective study of New York opiate addicts discharged from the Lexington Narcotics Hospital. Although Lexington researchers had long seen relapse as key to understanding addiction as a scientific and clinical problem, their research focus on long-term, institutionalized populations shifted to shorter-term users as a new generation of adolescents experimented with drugs in the late 1960s. Now the puzzle was individual or within-group variation—why some users, similarly exposed and from similar environments, became dependent in ways that precipitated relapse, but others did not.

A sociological outsider to the addiction research network, Lee N. Robins was among the first to study a non-treatment sample of opioid drug users, specifically 235 African-American men born in St Louis between 1930 and 1934. Robins and George T. Murphy found that although 10% of the sample had been addicted to heroin, only approximately 4% (nine) of the sample had been addicted to heroin, only approximately 4% (nine) had been treated. Yet only four subjects reported heroin use in the previous year (1964–65). Thus 78% of treated and 85% of untreated former addicts reported no heroin use for the previous year. Although drug addiction researchers greeted Robins’ findings with scepticism, by the spring of 1971 this question had become acute in relation to a specific population of opiate users—troops stationed in Vietnam.
The Vietnam drug user returns: crossroads for psychiatric epidemiology and drug abuse research

Fearing that the return of large numbers of heroin-addicted servicemen would present an acute public-relations problem, the Nixon administration consolidated federal drug programmes into a White House Special Action Office for Drug Abuse Prevention (SAODAP), which was given a 4-year deadline to visibly reduce the number of drug abusers. The new office was directed by Jerome H. Jaffe, a physician and neuropharmacologist who had worked in Lexington and New York City before starting the innovative, multi-modality Illinois Drug Abuse Program (IDAP) that brought him to Nixon’s attention. An unintended consequence of Nixon-era reforms was to wrest drug abuse research from NIMH hands. SAODAP phased into the National Institute on Drug Abuse (NIDA), which was created by legislative action in 1973 and placed under direction of Robert DuPont. Just as the mental health field lost its grip on drug abuse, a major federal role brought Lee N. Robins into cross-fertilization with addiction researchers and propelled her to the forefront of psychiatric epidemiology.

One of Jaffe’s first moves at SAODAP was recruiting Robins to conduct a study of all veterans returning from Vietnam.20 Approved in summer 1971, the first wave of the Vietnam Drug User Study was conducted that September; a follow-up of nearly 1000 participants occurred 2 years later. Issued in May 1974, Robins’ final report to SAODAP was titled *The Vietnam Drug User Returns.*2 Documenting rapid decline of so-called narcotic drug use by Vietnam veterans upon return stateside, Robins found a surprisingly high remission rate that suggested that few among those using heroin regularly in Vietnam remained opiate-addicted upon return. This unanticipated conclusion challenged the inevitability of relapse and catapulted Robins to national prominence. The original study was organized around two major questions. How many service members had used ‘narcotics’ (including amphetamines, barbiturates and opiates) in Vietnam? How many continued use after return? Robins’ unexpected answer was that there were few immediate after-effects of regular use of heroin and other narcotics in Vietnam. Fewer servicemen than expected relapsed; most were in ‘remission’. Importantly, the capacity for psychiatric epidemiology to firmly deliver accurate answers to policy questions reinforced the sense that it was instrumental for governance. Robins wrote with confidence in a 1973 Medical World News article, ‘How addicted are our Vietnam veterans’, that the ‘typical Vietnam drug user was the young soldier, single, and enlisted rather than drafted’.

Only 7% had prior experience with heroin; only 5% had been treated since return, most in compulsory army treatment programmes. Few voluntarily sought treatment. Yet a ‘surprisingly high proportion’ reported no continuation once home.21

Surprise is a constant feature of Robins’ writings on this subject—typically, she interpreted her results to indicate that drug dependence was not as permanent as previous studies had found and that percentages of re-addiction were lower (in the range of 1%) than previously found. This finding contradicted longtime researchers who sought to make relapse integral to the diagnostic criteria for drug dependence. Robins restated her surprise in paper after paper—claiming the study revealed things to be much better than expected given the hue and cry that had compelled the study.22 For a discussion of the rhetorical power of such claims (similar to those made about the DSM-III field trials yielding ‘surprising’ results that were ‘so much better than we had expected’), see Kirk and Kutchins, 1992.22

What would not have surprised Robins in light of her analysis in *Deviant Children Grown Up* was that veterans who continued to use were ‘more deviant’ than their fellows who discontinued use upon return. Veterans who continued drug use had prior drug use histories; used amphetamines and barbiturates in Vietnam in addition to opiates; and used less common opiates while in Vietnam—such as morphine, codeine or methadone.

The Robins research group portrayed the Vietnam experience as a natural experiment in the exposure of masses of young men to narcotic drugs in a ‘foreign’ or ‘frightening’ context.23 Most veterans claimed to have been offered heroin within a month of their arrival in Vietnam, where there were few social controls limiting use, and thus almost half of soldiers surveyed had tried heroin or opium and 20% had used it regularly while in Vietnam.2 Three-quarters of those who had used heroin in Vietnam agreed that it had ‘good’ effects such as ‘euphoria, [increasing] tolerance for army regulations, easing boredom and depression and making time go faster’, and half agreed it ‘made them less afraid and helped them feel part of the group’.2 Although Robins did not suggest that governmental and popular concerns were misplaced, she did not corroborate the existence of a crisis, writing: ‘Surprisingly, in the light of the common belief that dependence on narcotics is easily acquired and virtually impossible to rid oneself of, most of the men who used narcotics heavily in Vietnam stopped when they left and had not begun again 8 to 12 months later’.23 life (p. 249) The study found that ‘contrary to conventional belief, occasional use of narcotics without becoming addicted appears possible even for men who have previously been dependent on narcotics.’23 (p. 236) This finding contrasted to follow-up studies from the Fort Worth and Lexington hospitals, which stood as Robins’ examples of conventional thinking about the inevitability of relapse. Those who apply the term ‘chronic’ to
cases of relapse do not agree it fits all cases. For instance, Jaffe’s chapter on drug addiction and drug abuse for the third edition of Goodman and Gilman’s classic textbook, The Pharmacological Basis of Therapeutics (1965), uses the term ‘chronic, relapsing disease.’ Jaffe noted in a 2007 interview with the author that he did not say ‘This is a permanent relapsing brain disease [but that] the loss of flexibility with respect to a drug exists on a continuum. At one extreme, you have addiction, you have a compulsive drug-using disorder which begins to resemble a chronic relapsing disorder. There are milder forms that aren’t chronic. People recover…I just know too many people in complete recovery for me to view them as having a permanent brain disease…there are lots and lots of complexities here that I think are glossed over and minimized when you say it’s a brain disease. I think it’s wrong to label every case of drug disorder as a chronic relapsing disorder…to me, the idea that addiction is a chronic, relapsing brain disorder is both counter-intuitive and counter to all observations’.

Robins emphasized the unprecedented nature of narcotic use in Southeast Asia, which was ‘truly unlike anything prior in the American experience’. Recognizing that the unique setting could account for her findings, Robins took aim at ARC researchers who also studied a unique institutional setting but showed much higher rates of relapse than remission. The narcotics hospital operated as a revolving door for invertebrate narcotic addicts, some readmitted for treatment dozens of times despite professing little interest in quitting. Robins was intrigued with differences between those returning from Vietnam and long-term civilian addicts hospitalized for treatment. Whereas the authoritative definition of drug dependence as a chronic, progressive disorder characterized by high relapse rates might be suitable for long-term addicts, Robins showed it was not applicable to returning veterans whose use had occurred outside the country and in the context of war.

As noted above, most addiction researchers had studied long-term, institutionalized male addicts who were either treatment-seeking or incarcerated. Robins argued, ‘We can no longer justify applying policies to every narcotics user that are based only on information about the careers of those addicts whose appearance in treatment facilities as volunteers or in lieu of prison sentences shows an inability to terminate their addiction on their own’. Given her results in Deviant Children Grown Up,1 which enabled her to question the immutability of ‘sociopathic personality disorder’ after finding that one-third of sociopaths gave up antisocial behaviours whereas two-thirds retained them into adulthood,1 Robins may well have expected to find that the best predictors of continuing drug abuse among returning Vietnam veterans were antisocial ‘deviance’, particularly injection drug use prior to deployment, and parents with records of drinking or drug problems and arrests. This conclusion was supported by another large-scale epidemiological study confirming that Vietnam-specific military service was not an important causal factor in increased heroin use by young men between the late 1960s and early 1970s:

Those who were exposed to drug use in the military or in Vietnam may have begun to use drugs earlier than they otherwise would have done, but not many more began to use than would have been expected to do so without experience in the military. When the focus is on men who were in Vietnam in 1970 and 1971, drug use rates are high. However, when the focus is broadened to encompass all of the young men in the sample, the effects of military service on drug use are invisible, and the effect of service in Vietnam is little more than a ripple in a stream.

Causation was not here considered a psychiatric problem or syndrome; the analytical separation between drug use and abuse was adopted to purge the field of moralism. Self-identified biological psychiatrists had so pressured the old relationship between psychopathology and narcotic addiction that the emerging science of neuroparmacology posited that neuroadaptation and conditioning to social and environmental cues better explained the chronic, relapsing nature of the condition than did psychiatric disorder. When Robins et al. argued that their study of Vietnam veterans suggested that beliefs about the chronicity of heroin addiction had been exaggerated, they confronted core assumptions bolstered by decades of empirical addiction research issuing from the ARC.

‘Civilian addicts’ was a new category constituted by Robins’ group, which speculated that Vietnam veterans differed from civilian addict counterparts. Robins identified a high-risk group of veterans that most closely resembled civilian addicts. Far from minimizing risk of relapse, she argued that ‘rates of relapse to addiction in certain subgroups of Vietnam veterans were by no means trivial. But what we have learned is that our traditional view of heroin addiction as so permanent and so intractable as to make the addict’s life virtually unsalvageable was too pessimistic a view’. Even in the highest-risk veterans, the relapse rate was still half that reported in civilian follow-ups from the narcotics hospitals. Robins argued that ‘Expectations for relapse among civilian addicts must be grossly pessimistic. It may be that the low rate of addiction since return reported by Vietnam veterans should have surprised us less than it did. Our beliefs about the chronicity of heroin addiction may be based on experience with highly unrepresentative civilian samples of addicts’. By contrast to civilian addicts admitted to the narcotics hospitals, Robins’ veteran population lacked a history of chronicity. Only in veterans with serious histories of drugs
Narcotic drug addiction was an ambiguously psychiatric condition that spawned tautological reasoning. Predictors of narcotic dependence were hard to find in the general population due to miniscule prevalence of narcotic addiction. Hence, limiting samples to those who had previously used narcotics improved predictive power immensely. Discussing Robins’ 1974 Vietnam follow-up sample of 943 returnees, her St Louis colleague, John E. Helzer, noted how hard it was to distinguish cause from effect in treated populations. Stronger predictors of alcoholism—most notably association between child and adolescent antisocial behaviour and adult alcoholism—had been identified by psychiatric researchers at Washington University. Antisocial behaviour during adolescence had been found by Lee Robins and John E. Helzer to be the best predictor of adult narcotic addiction in Vietnam. Nor would it surprise the St Louis group that social maladjustment after returning from Vietnam was predictive of later narcotic addiction.

Robin’s research team advanced a concept of vulnerability to addiction, finding that ‘adult antisocial behavior has its primary effect on vulnerability of users to addiction, while the demographic characteristics, race and age, each affect only the exposure to use.’ The conclusion was that prevention efforts should be focused on those who were at highest risk—the unemployed, the depressed or those who had a history of antisocial acts and alcohol abuse. Psychiatric diagnosis mattered little here; what mattered was behaviour within specific social contexts. Only those who came into conflict with law, medicine, educational institutions or social services—in short, only those authorities deemed ‘deviant’—later showed up as addicts. As de facto deviants they could be expected to differ from those who used drugs without coming to the attention of authorities. Did those whose drug use did not come to the attention of authorities devise one deviant act facilitates the next?

One overriding question for psychiatric epidemiology is whether or not substantial numbers of a population live with undiagnosed or untreated conditions. Assuming refined diagnostic technologies, would more people be categorized with diagnoses and presumed to need preventive or therapeutic care? The role of psychiatric epidemiology in shaping policy and hence in constituting populations for the sake of governance was about to expand radically. During the 1970s, epidemiological studies were increasingly considered authoritative mappings of psychiatric phenomena. Psychiatric epidemiology was becoming a basis from which to dislodge the stigmatization of the mentally ill, the addicted and the alcoholic. Yet diagnostic categories were still not stable, reliable or proven valid. Although concerns about reliability and validity of diagnostic criteria had surfaced as early as the 1950s, that step remained to be taken as late as the 1970s.

‘Hardening of the categories’: the Epidemiological Catchment Area (ECA) study’s role in reinforcing DSM-III as a diagnostic technology

At the heart of biological psychiatry lodged the goal of destigmatizing formerly stigmatized disorders. The democratization of psychiatric disorders through claims about prevalence, frequency and population distribution was helpful to the destigmatization project. Adopting this goal, Robins co-authored with Darrel A. Regier, then director of the Division of Clinical Sciences at the NIMH, the now-classic Psychiatric Disorders in America: the Epidemiologic Catchment Area Study. Using laypersons to administer extensive psychiatric surveys in five regional ‘catchment areas’, they interpreted the ECA findings as a demonstration that rates of mental illness in the USA were higher than previously presumed and that co-morbidity was common. Soon after Robins concluded the second wave of the Vietnam Experience Study, the NIMH asked her to develop survey instruments based on soon-to-be-published diagnostic criteria from DSM-III. The DSM-III architects were a close-knit group; Robert L. Spitzer was a frequent guest of Eli and Lee Robins in St Louis due to a strategic introduction by Martin M. Katz, director of the NIMH Clinical Research Branch. Nancy Andreasen characterized the close-knit band of ‘neo-Kraepelinians’ as the ‘Mid-Atlantic counterrevolution’, identifying Hopkins, the New York State Psychiatric Institute (NYSPI), Iowa Psychiatric Hospital in Iowa City and Washington University in St Louis as ‘the Mid-Atlantics.’ Despite relative isolation from mainstream American psychiatry, the Mid-Atlantics mounted a revolution in American psychiatry. Andreasen argues that the principal players in the DSM-III—almost all of whom were from the institutions she named as Mid-Atlantics—had
no idea that the release of DSM-III would have such major impact.

In the late 1960s Katz convened an advisory group on the psychobiology of affective disorders, through which he became aware of the concurrent project at Washington University to develop diagnostic criteria for affective disorders. Known as the Feighner Criteria, these were published in January 1972 but had been in development since the late 1960s. Spitzer and Endicott adopted the so-called Feighner Criteria, also known as the ‘St Louis Criteria’ and the ‘criteria developed by the Renard Hospital Group in St Louis, Missouri’, as the basis for a semi-structured psychiatric interview schedule, the Schedule for Affective Disorders and Schizophrenia (SADS). The Feighner Criteria were based on a painstaking, 9-month review process of all extant criteria for depression, antisocial personality disorder and alcoholism that took place in St Louis in the late 1960s. The Feighner or St Louis Criteria later expanded to 15 conditions where there was sufficient evidence of validity, consistency over time, clinical description and increased familial incidence. The process involved six Washington University psychiatrists, including Eli Robins and psychiatry resident John Feighner, with cameo appearances from Lee Robins and others. With the exception of Lee Robins’ criteria for sociopathic or antisocial personality disorder, antecedent criteria had not been statistically validated or standardized. The Feighner Criteria paper itself became a ‘citation classic’, numbering among Eli Robins’ top three most cited papers. The Renard Diagnostic Interview was then developed at Washington University to operationalize the St Louis or Feighner Criteria.

Finally, the Research Diagnostic Criteria (RDC), also based on the Feighner Criteria, were developed with sponsorship by the Clinical Research Branch of NIMH. The RDC covered 25 conditions considered for inclusion in DSM-III, and advanced as a solution to the problem of low reliability prevailing routine clinical psychiatric diagnosis through the first two editions of the DSM. The stated problem to which the RDC replied was that diagnosticians did not use statistically validated or standardized diagnostic criteria, but brought to bear idiosyncratic clinical concepts. Additionally, the criteria generated by the St Louis group emphasized longitudinal factors such as the course or progression of disease or disorder over time in ways that other diagnostic criteria did not.

NIMH propagated wide reliance on the RDC in sponsored research. Common diagnostic criteria were designed with an eye towards clinical acceptance, but also for research so that investigators could select a relatively homogeneous group of patients who met both inclusion and exclusion criteria. By the late 1970s, Endicott, Spitzer, and Eli Robins could triumphantly proclaim that: ‘The use of operational criteria for psychiatric disorder is an idea whose time has come’. Despite the convergence of these efforts, by the time of Carter’s President’s Commission on Mental Health in 1978, there was still no standardized diagnostic interview based on then-developing DSM-III criteria. NIMH staff selected the RDC for incorporation into the DSM-III, and Lee N. Robins’ group in St Louis designed the NIMH Diagnostic Interview Schedule (DIS) used for ECAP.

The ECA study was a multi-sited, collaborative approach designed to capture diversity within and between large, geographically defined catchment areas populated by at least 200,000 people, of whom 3500 subjects (3000 household members and 500 institutional residents) were interviewed. The ECA study was hailed as an ‘unprecedented atlas’ and a ‘modern-day voyage of the Beagle’ when the landmark final report was published as Psychiatric Disorders in America. Credited to the presidential commission, the ECA helped shift the field towards objective diagnostic methods and standardized operational criteria. Until Carter’s commission, there had been no national commission on mental health since Kennedy’s 1961 Joint Commission on Mental Health and Illness. NIMH had overseen changes in governance of mental health, none of which had been directly responsive to the move towards diagnostic criteria then well underway.

The goal of the study, according to its authors, was to reduce the ‘ignorance surrounding these much stigmatized mental and substance abuse disorders’. The twin problems of lack of comparability and proliferating diagnostic systems were overcome by relying on the DSM-III and statistical calculations of prevalence rates for specific psychiatric symptoms, which were referred to as the ‘building blocks of mental disorders’. The building blocks are unlikely to change over time, although they will be reassembled in different patterns to create new diagnoses…Thus the ECA study can be expected to be a prime source of psychiatric epidemiological information for years to come. The stated goal was not to simply provide prevalence information, but to reduce stigma: ‘By shedding scientific light on mental disorders that have so often been shrouded by shame and stigma, we hope to make possible a more objective and optimistic approach to mental illness’. This approach, the authors predicted, could eventually be linked synergistically to research efforts in molecular genetics, the neurosciences, psychopharmacology and the behavioral sciences. Harnessed to ‘real’ science, psychiatric epidemiology thus became a ‘real’ science itself.

For all Robins’ influence over statistical validation and stabilization of techniques for ensuring the reliability of psychiatric diagnosis in epidemiological and sociological studies, psychiatric epidemiology did not become sociological—that is, it did not become a conceptual framework that dealt adequately with the social and cultural norms at work in the social worlds within which psychiatric symptoms are formed and psychiatric syndromes are lived out by subjects. Permeated by the ‘medical model’ of what
psychiatry should be and do, psychiatric epidemiology was made a narrowly technical discipline constrained to statistical rather than conceptual explanation. Yet the conclusion to Psychiatric Disorders in America contained important clues for anyone charting the conceptual course of psychiatric epidemiology. The first of these concerned the practical, theoretical and nosological implications of co-occurrence of disorders, the ‘presence of other disorders [that] may defeat the therapeutic efforts or increase risks of relapse of the treated disorder’. Epidemiological investigation established that certain disorders occurred together at rates well above chance—but did these disorders have a common aetiology or did they function as risk factors for other disorders? Secondly, to the expressed surprise of Robins and Regier, nosology intruded them despite their efforts to ‘take the 1980 nomenclature as a given and operationalize it’. Instead, the ECA findings pressured almost immediate changes in official nomenclature and diagnostic criteria that led to the DSM-III-R. The study—and its authors—gained the imprimatur of credibility and far more cultural authority than any previous study in the field. To this day the ECA stands as a singular landmark in the history of psychiatric epidemiology in the United States, whereas the DSM became an ever-more contentious conceptual object. However, the concepts and methodological practices characterized by Robins’ approach have been stabilized and consolidated within psychiatric epidemiology, assisted by enhanced computational power, the proliferation of specialized diagnoses and increased understanding of the neurobiological basis of mental health and disorder. The ‘spirit of St Louis’ has been disinterred from the local context in which it took shape, and the unique cultural preoccupations and professional aspirations of those who threw their conceptual weight behind it have become widely shared and even global preoccupations of psychiatric epidemiology. Well into the 21st century, efforts to fully standardize diagnostic interviews within the official nomenclature of the DSM continue.

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KEY MESSAGES
- Knowledge practices central to American psychiatric epidemiology—such as the use of structured interviews tightly fitted to the nomenclature of diagnostic criteria—required an epistemological shift propelled by the new set of conceptual and methodological practices central to the emerging knowledge formation.
- Sociologist Lee N. Robins was a key figure in the reconceptualization of the methodological shift from non-specific survey to detailed structured interview.
- Robins’ influence on the use of psychiatric diagnostic criteria for research purposes can be traced in three major sociological studies: Deviant Children Grown Up (1966); two waves of the Vietnam Experience Study (1972 and 1974); and the Epidemiological Catchment Area Program (ECAP) (1980).

References


