Psychiatric epidemiology, or the story of a divided discipline

Steeves Demazeux

Université Bordeaux Montaigne, Laboratoire SPH – Philosophy Bordeaux, France.
E-mail: sdemazeux@gmail.com

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Abstract

This article traces the historical decisions, concepts and key professional collaborations that laid the foundations for the formation of American psychiatric epidemiology during the 20th century, up to the discipline’s institutional consolidation, circa 1980, when the third edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-III) was published. Thomas Kuhn’s ‘disciplinary matrix’ is mobilized as a framework that allows the institutional and intellectual construction of a discipline to be analysed as separate but intertwined components, without assuming that the two evolve in tandem. The identification of the strengths as well as the frailties and internal divisions of the discipline as it developed reveals a paradoxical situation: a time lag between psychiatric epidemiology’s institutionalization and public recognition, on the one hand; and the weak coherence of its intellectual components, on the other hand. We briefly trace the origins of split among the discipline’s aetiological models of mental disorders and suggest that the lack of coherence among them has prevented psychiatric epidemiology from achieving the status of a normal scientific discipline, in the Kuhnian sense. Without a more explicit attention to the intellectual rationale of the discipline, psychiatric epidemiology will continue to maintain a strong institutional dimension and weak intellectual matrix.

Key Messages

• Compared to the history of general epidemiology and to that of most epidemiological sub-disciplines, psychiatric epidemiology has developed more slowly, both institutionally and intellectually;
• From the late 1950s on, psychiatric epidemiology in the USA was gaining institutional recognition, but it remained weak (or “pre-paradigmatic” in the Kuhnian sense) intellectually;
• By 1980, American psychiatric epidemiology was undergoing a profound conceptual crisis. There was a split in its disciplinary matrix between two radically opposite approaches: a medical-centred approach (as embodied by the DSM-III) and a socio-ecological approach (favored by some of the most influential actors in the history of psychiatric epidemiology).
Introduction

Since the 19th century, philosophers of science have utilized the term ‘discipline’ to characterize a specialized branch of knowledge with its specific objects, theories, methods, academic journals and degree courses in universities. A ‘discipline’ implies the existence of both a structured community of scholars (its institutional or organizational dimension) and a specific set of methods, concepts, practices, problems and solutions (its intellectual dimension). Compared with the history of general epidemiology and that of most epidemiological sub-disciplines, psychiatric epidemiology has developed more slowly, both institutionally and intellectually. By the 1980s, the institutional relevance of psychiatric epidemiology had been fully accepted in the USA. This paper questions, however, whether the intellectual coherence of psychiatry epidemiology was similarly assured by that period. Without such coherence, psychiatric epidemiology cannot be considered to have achieved the status of a strong, let alone fully developed, scientific discipline.

To follow this line of enquiry, this paper uses Thomas Kuhn’s concept of a ‘disciplinary matrix’ (defined below) as a relevant framework in that it captures both the institutional and the intellectual constructions of a discipline, without assuming that they must evolve in tandem. After introducing Kuhn’s concept, the paper traces the historical decisions, concepts and professional collaborations that laid the foundation for the formation of North American psychiatric epidemiology in the 20th century. It signals the relationship of psychiatric epidemiology to both psychiatry and general epidemiology, as well as the unique research partnership between psychiatrists, psychologists and sociologists, many of whom would become the pioneers of the discipline. It then evaluates these developments by comparing the institutional and the intellectual accomplishments, taking 1980 (the year of the publication of the American Psychiatric Association’s Diagnostic and Statistical Manual of Mental Disorders (DSM-III)) as both the culmination of and the turning point for the discipline’s development. The last section discusses reasons for a lag in the two dimensions of the psychiatric epidemiology’s disciplinary matrix, namely the split within the intellectual component (‘matrix’) of the discipline, whose coherence is nevertheless necessary to the achievement of what Kuhn considers a normal science. In conclusion, the relevance of this analysis to further research on the epistemological history of biomedical disciplines is outlined.

Thomas Kuhn’s elaboration of the scientific paradigm: the disciplinary matrix

Thomas Kuhn’s concept of ‘paradigm’, introduced in The Structure of Scientific Revolutions, has been very influential in philosophy of science. But it has also often been criticized for its ambiguity and multiple meanings. Kuhn himself was quite cognizant of these critiques by the time the second edition of The Structure of Scientific Revolutions appeared. In his 1969 Post-script to that edition and in further essays, he attempted to clarify the paradigm concept in at least three principal ways.

First, Kuhn envisioned the paradigm in its most global sense as that which a community of scientists share and to which they are committed. To capture this, he suggested replacing the term ‘paradigm’ with that of ‘disciplinary matrix’, which referred at once to that which is possessed in common by the practitioners of a particular profession (‘disciplinary’) and the ordering of a set of intellectual elements (‘matrix’) within the discipline.

Second, by breaking down the disciplinary matrix in this way, Kuhn allowed for the contrast between its institutional aspect, or ‘community structure’, and its intellectual or cognitive aspect. In the latter, he included: (i) symbolic generalizations; (ii) metaphysical models; (iii) shared values; and (iv) exemplars. One strength of Kuhn’s approach lies in its requirement that a ‘normal science’ can progress only if all the aspects of the disciplinary matrix are adequately developed. Or, as Alexander Bird puts it, if ‘there is the strong commitment by the relevant scientific community to their shared theoretical beliefs, values, instruments and techniques and even metaphysics’. Kuhn further insists that one can speak of a paradigm only when a discipline is fully constituted, that is when the institutional aspects (or organizational norms) and the theoretical-methodological aspects (or cognitive norms) of the discipline overlap in a dynamic way.

But whereas institutional and cognitive dimensions are intertwined, an actual overlap does not always occur: a gap may open up between the achievement of a normalized institutional framework (the community of scientists, with their academic departments, journals, professional societies and so forth) and a still ambiguous intellectual project, itself split into numerous definitions and models. For example, as the philosopher Jean Gayon has shown, this is the case for genetics: after the rediscovery of Mendel’s laws in 1900, genetics very quickly became a specialized discipline, and it underwent intense institutionalization in the 1940s and the 1950s. Yet today it would be difficult from the Kuhnian perspective to still grant genetics the status of a scientific ‘discipline’ in its own right, given its fissured theoretical identity. The reason for this loss of theoretical identity, at a time when the tools and methods of genetics now exist throughout the biological and medical sciences, can be traced to the concept of the gene itself. For, as Gayon demonstrates, the ‘gene’ evolved from a theoretically robust notion at the beginning of the 20th century into
a polymorphous and difficult-to-grasp notion by the century’s end. Third, Kuhn emphasized a subset of the disciplinary matrix, the exemplar, as being more important than the matrix’s other elements for attaining the status of a scientific discipline. He defined exemplars as standard examples of problem-solving within a specific discipline, more closely linked to practice than are other aspects of the disciplinary matrix, such as symbolic generalizations or models. As Kenneth F Schaffner has pointed out, the Kuhnian notion of ‘exemplar’ has ‘some interesting implications for the analysis of particularity in medicine’. As an exemplar commonly denotes in biomedicine a particularly successful example of an experiment (or the solving of a problem, whether or not experimentally), it inspires other members of the discipline as they take on new puzzles, and it plays an essential role in transmitting a discipline’s way of working, which always operates to a certain extent by analogy with previous problem-solving. Hence exemplars are crucial to the success of the overall disciplinary matrix. (It is useful for purposes of clarity to note Peter Godfrey-Smith’s qualification of the disciplinary matrix as a paradigm in the broad sense, and the exemplar as a paradigm in the narrow sense.)

Kuhn’s original conception of a ‘disciplinary matrix’ is particularly useful for questioning the intellectual coherence of a complex discipline with regards to its institutional functioning. We now turn to one such case: the evolution of psychiatric epidemiology; its intersection with general epidemiology, psychiatry and the social sciences; and its paradoxical development.

The multifaceted early history of psychiatric epidemiology: mental diseases in the fold of the ‘new epidemiology’

The conditions for the emergence of an epidemiology of mental diseases were established early on, but changes within general epidemiology created a conceptual space for psychiatric epidemiology to move beyond the application of statistics to treated cases of mental illness. The early developments that fostered the formation of psychiatric epidemiology can be divided into three stages.

Proto-epidemiology of mental diseases

The origins of psychiatric epidemiology are anchored in the administrative statistics tradition of 19th century asylums and the very early statistical studies that attempted to correlate mental diseases with various environmental factors. The British medical statistician, William Farr (1807–1883), contributed to the understanding of the relationship between disease and the environment; Sir Arthur Mitchell (1826–1909) in the UK and Edward Jarvis (1803-1884) in the USA applied statistics to examine the disease-environment relationship specifically for mental disease. Unlike keepers of asylum statistics, which for decades would constitute the major and often only source of data on cases of mental illness, Mitchell and Jarvis gathered their statistical data both within and outside the asylum. Thus, they laid very early the groundwork for psychiatric epidemiology and provided an early solution to the thorny methodological problem of whether and how to include both treated and untreated cases of mental illness.

The broadening of general epidemiology

By the first half of the 20th century, the study of mental diseases was potentially furthered by a broadening of the scope of general epidemiology to encompass phenomena beyond infectious diseases. In 1928, the British physician Sir William Hamer employed the term ‘new epidemiology’—possibly the first use of this expression—to address the considerable progress that had been made in the previous century in analysing and preventing epidemics. However, epidemiology was soon expanding to include new areas of enquiry that would redefine its most basic research object. Notably, the English epidemiologist and statistician Major Greenwood demonstrated the value of applying the epidemiological method to non-infectious diseases such as cancer in a key 1935 textbook. He called for nothing less than a theoretical shift of epidemiology’s centre of gravity, arguing that for the epidemiologist, it was no longer the contagiousness of a disease that mattered, but rather its status as a crowd disease, by which he meant all pathological conditions with a significant distribution in a given population.

Over 20 years after Hamer proclaimed the advent of a ‘new epidemiology’, John Gordon, a professor in the Department of Epidemiology at Harvard, used the term in a very different way. In a 1950 article, he pronounced epidemiology as poised to become ‘a discipline with implications more far-reaching than the study of epidemics’. Seemingly disparate public health problems, ranging from cancer and cardiovascular diseases to the prevention of road accidents, would soon warrant consideration as genuine topics for epidemiological study. Gordon’s ‘new epidemiology’ reflected how epidemiologists worldwide were coming to accept the transformation of the discipline into what Greenwood had earlier called ‘the study of the mass aspects of disease’. This change of focus met resistance from some camps. However, the community of North American researchers proved particularly open to the new epidemiology, partly because of institutional concerns with the problem of redefining the missions of the nation’s...
public health officers in the aftermath of World War II. At the same time, Gordon noted, people were witnessing ‘a single epidemiological universe’ in a ‘shrinking world’, as travelling became commonplace, trade between cities and countries intensified and the need for water and food reached global proportions.²⁰ Once tropical diseases could be transported more easily, former epidemiological sub-units, based on geographical criteria or the particularities of place, would lose their relevance. Moreover, the improvement of living conditions and the ageing of the population required a reordering of priorities, as infectious diseases became less prevalent and the burden of chronic diseases emerged as a major public health issue.²⁰

Psychiatry and the new epidemiology
Within the discipline of psychiatry, support for the idea that new types of statistical studies and new objects of enquiry might warrant the term ‘epidemiological’ developed more slowly. The issue was first raised explicitly in a 1937 paper, entitled ‘Is there an epidemiology of mental disease?’ delivered to members of the American Public Health Association by the Massachusetts physician, Henry Elkind. He claimed that, to his knowledge, no published texts in which mental diseases were clearly treated as epidemiological objects could be found in the psychiatric literature before the early 1920s. However, he did note that in a lecture published in 1923, Professor Milton J Rosenau, Director of the Department of Preventive Medicine and Epidemiology at Harvard University, had suggested the possibility of applying epidemiology to mental diseases. Elkind identified several sources of resistance that had impeded the development of a full-fledged epidemiology of mental diseases. At the top of his list was the abstract and intangible nature of psychiatric symptomatology. Elkind mentioned other obstacles, such as: the conflict between theoretically-based schools; the fact that the available statistical data, for the most part drawn from asylum statistics, were partial and often inexact; the ‘single aetiologically-minded’ character of most doctors; and, more generally, the infectious thicket in which epidemiology as a whole seemed trapped. Yet these obstacles did not prevent him from predicting that ‘during the next decade or two, mental disease [will] become an important part of epidemiological science.’²²

An original partnership between psychiatry and the social sciences
Beyond the proto-epidemiology period of asylum statistics, the growth of an epidemiological science of mental disease predicted by Elkind can be traced to the crucial contribution of social scientists, particularly in the USA.²³,²⁴ Three major types of contribution influenced the formation of psychiatric epidemiology as a discipline.

A first strand developed within sociology. In fact, the particular tenor of the first US studies of mental disease outside the asylum, characterized by a concern for community, place and social status, came from sociologists—the University of Chicago’s Department of Sociology, founded in 1892, playing a central role in this endeavour. Within the field of psychiatry, the Chicago School exerted a direct influence on community studies, particularly through the seminal research of Chicago-trained sociologists Robert Faris and Warren Dunham. Their study, published in 1939, examined the medical records of over 30 000 hospitalized psychiatric patients and concluded that the rates of schizophrenia in Chicago were higher in more deteriorated urban areas.²⁵ (For the influence of the Dunham and Faris study on French sociology of mental disease, see Henckes²⁶ in this issue of IJE.) During the interwar years, the National Institutes of Health sponsored community studies in various areas of non-infectious chronic disease, particularly cardiovascular disease. However, after World War II, Faris and Dunham’s ecological perspective provided the impetus for a new kind of socio-medical enquiry on mental health, spearheaded by sociologists like August Hollingshead, Leo Srole, Lee Robins, Bruce Dohrenwend and the social psychologist, Barbara Snell Dohrenwend. These scholars pioneered the development of psychiatric epidemiology as a discipline, each in their own way.²⁷

On the whole, their research illustrated the application of sociology in medicine, in contrast to the contemporaneous field of the sociology of medicine.²⁸ It intersected with the more general concerns in the 1950s and 1960s of physicians and epidemiologists working both inside and outside mental health, including John C Cassel and Mervyn Susser, whose research in South Africa contributed to the development of epidemiological thinking focused on social determinants,²⁹,³⁰ and the British epidemiologist Jerry Morris, who wrote the first post-World War II textbook for the burgeoning discipline of epidemiology.³¹ Cassel, Susser, Morris and their followers shared with the pioneering mental health sociologists a particular concern for the relationship of health and disease to socioeconomic position and environmental factors, a concern later identified with social epidemiology.³²

Social science thinking within psychiatry, notably in the area of social psychiatry, constituted a second strand in the construction of American psychiatric epidemiology. Social psychiatry exerted a significant influence on the promotion of epidemiological studies in Europe and in North America. However, social psychiatry as it was understood after World War II differed radically from its prewar definitions. Until the 1940s, no clearly defined school of
thought characterized social psychiatry, and the orientations defended under its label varied widely from one country to the next. In Germany, for instance, social psychiatry took on the quite broad project of studying, as historian Michael Neve has noted, ‘the mental health of the whole population’ in order to promote ‘an active programme as to how to prevent mental pathology [from] increasing within [the] population’. German social psychiatrists, like some of their European counterparts, mobilized the idea of heredity, as it had been expounded in theories of degeneration, as a target of prevention. A similar notion of prevention developed in the USA under the impetus of the mental hygiene movement founded by the former asylum patient, Clifford Beers, and the Swiss-born psychiatrist, Adolf Meyer. Whereas this movement was anchored in the combination of self-help practices, system reform and community-based interventions with which historians of psychiatry are more familiar, its principles of prevention were nevertheless relayed and reinterpreted by eugenicists in the 1920s.

After World War II, social psychiatry acquired a sounder identity, partly due to the development of the mental health paradigm in the USA and the influence of the World Health Organization (WHO). At that stage, it became more specifically defined as ‘a theoretical field that postulates the relevance of social variables to the causes of mental illnesses and their treatment and prevention’. From a theoretical point of view, social psychiatry in the USA resembled more closely the ‘social medicine’ defended by the Englishman John Ryle, and aimed at effecting the transition towards an epidemiology of non-infectious diseases.

A third strand, running alongside the contributions of sociology and social psychiatry, involved another professional group, namely psychologists. Working with psychiatrists, psychologists exerted a discreet but decisive influence on the nascent discipline of psychiatric epidemiology. As several historical accounts have noted, psychologists, working as assistants or technicians, affected the organization of epidemiological studies in psychiatry and were crucial to the design of the first standardized tools for epidemiological use. They included eminent figures like John R Wittenborn, Maurice Lorr, John Overall, Paul E Meehl and Joseph Zubin, although the importance of these psychologists, most of whom held respectable academic positions, to the development of psychiatric epidemiology tends to be under-acknowledged. For example, John R Wittenborn, Maurice Lorr and John Overall made major contributions to the development of standardized instruments as well as to the use of factor analysis in psychiatric research. Their work notably influenced the case for using dimensional approaches in classification as opposed to the dominant neo-Kraepelinian categorical approach favoured by the designers of DSM-III.

Yet in the 1960s, at a time when US psychiatry was in intellectual turmoil because of its internal divisions, many of these psychologists were able to convince psychiatrists not only to introduce more rigour into their diagnostic approaches and quantitative analyses, but also to give population studies precedence over clinical ones. To give but one example, Joseph Zubin, a professor at Columbia University, founder of the Biometric Research Unit at New York State Psychiatric Institute and former President of the American Psychopathological Association (APPA), contributed significantly to conceptualizing what a psychiatric epidemiology might actually resemble. In a 1960 talk delivered to psychiatrists in Chicago, Zubin identified six dominant models of psychopathology: the socio-cultural, the developmental, the learning-centred, the internal environment-centred, the genetic and the cerebral. He highlighted a seventh model as superior, coining for it the label ‘epidemiological super-model’. For him, this was the only theoretically powerful model. Epidemiology thus comprised a ‘super-model’ rather than simply a method or a simple model because it integrated all of the other models, from the purely environmental to the purely biological. It could thus encompass the positive qualities of the range of theoretical models, while avoiding their respective shortcomings. Finally, the super-model proved to be the only truly inclusive one, Zubin argued, because it alone could provide the researcher with ‘the whole picture’ of the particular causes of mental diseases.

Owing largely to the efforts of influential scholars like Zubin himself, the conditions were ripe for a disciplinary consensus among epidemiologists, sociologists and clinicians. All of them stressed the necessity of establishing an original partnership between the biological sciences and the social sciences as a foundation for psychiatric epidemiology.

The 1960s: psychiatric epidemiology’s institutional success

Neither an emerging consensus between theoreticians nor the constitution of an original partnership between social scientists and psychiatrists sufficed to establish psychiatric epidemiology as a new discipline. For this to happen, Kuhn reminds us, all the aspects of a disciplinary matrix must be shared by a structured community of scholars. More specifically, the medical historian and physician George Rosen has emphasized the degree to which professional and scientific societies were essential to providing an identity for epidemiologists in general. In his review of John R Paul’s book on the history of the American
Epidemiological Society, Rosen noted that: ‘Specialized knowledge and techniques may exist and be used by physicians, and yet there may be no sense of a separate identity. Only when there are bonds between such practitioners which take shape within an association based upon like interests and common problems does an identifiable specialized group appear’.  

Rosen’s general conclusion is equally applicable to psychiatric epidemiology. In fact, an identifiable, specialized group was gradually emerging from epidemiologists, physicians, social scientists and statisticians working on mental health issues, and its members came to be publicly designated as ‘psychiatric epidemiologists’. In 1959, the American Psychopathological Association devoted its Forty-ninth Annual Meeting to the epidemiology of mental disorders. Three years later, a reviewer of the just-published proceedings of this important event, writing in the Journal of the American Medical Association, humorously remarked that ‘there is now an epidemic of investigations of the epidemiology of psychiatric disorders’. Indeed from the late 1950s on, the private health policy foundation, Milbank Memorial Fund, and the National Institute of Mental Health, funded studies specifically designated as psychiatric epidemiology. Among the first journals to devote space to psychiatric epidemiology was Social Psychiatry, founded in 1966, although it did not change its title to Social Psychiatry and Psychiatric Epidemiology until 1998. In 1968, Mervyn Susser, by then working in the USA, published Community Psychiatry. Epidemiological and Social Themes, which reviewed relevant studies to date, including his own, and discussed issues raised by the application of epidemiological methodology to the measurement of mental illness in the community. Psychiatric epidemiology was gaining institutional recognition through its presence in North American universities. Columbia University founded the first psychiatric epidemiology training programme in 1967 and obtained funding for it from the National Institute of Mental Health (NIMH) Program to Enhance Diversity in Institutional Training (T32 Program) in 1972. From the late 1970s on, NIMH funded Psychiatric Epidemiology Training (PET) programs at several other universities as well. Meanwhile, the first psychiatric epidemiology textbook, by British authors Brian Cooper and Howard Morgan, appeared in 1973.

Psychiatrists had also turned to psychiatric epidemiology as it confronted the institutional watershed represented by the second revision of the American Psychiatric Association’s Diagnostic and Statistical Manual of Mental Disorders (DSM-II). For the first time, the DSM revision task force integrated a large number of recognized epidemiologists and epidemiologically-oriented researchers, including Ernst Gruenberg (the first psychiatrist in the USA to hold a doctorate in epidemiology), the biostatistician and epidemiologist Morton Kramer, the pioneer mental health researcher Benjamin Pasamanick and the researcher and psychiatric administrator Lawrence Kolb.

It is beyond the scope of this paper to explore the various complex socio-cultural forces that led to the institutional recognition of psychiatric epidemiology. From the 1960s on, psychiatric epidemiology clearly enjoyed all the attributes of a young discipline, including the formation of a critical mass of internationally renowned specialists. Barbara and Bruce Dohrenwend’s categorization of the discipline into three generations of community studies fits nicely with the gradual institutionalization of psychiatric epidemiology in the USA and elsewhere—even if their schema represents only one aspect of the history of psychiatric epidemiology, given that the field was not limited to community studies. The first generation, which for the Dohrenwends ended with World War II, consisted primarily of asylum statistics. At that time, psychiatric epidemiology had yet to be identified as an autonomous scientific discipline and, somewhat strikingly, the term ‘epidemiology’ was virtually absent from the 16 studies which the Dohrenwends included in that first generation. It was only with the second generation, comprising about 60 studies undertaken between 1945 and 1980, that the term ‘epidemiology’ began to appear in the definition and the self-designation of many of the community studies in psychiatry.

The third generation described by the Dohrenwends encompasses all studies undertaken after the 1980 publication of the third edition of the DSM (DSM-III), that is with the version of DSM which for the first time aimed to standardize all diagnostic criteria. With this new generation of studies, psychiatric epidemiology moved into a new phase, characterized by a profusion of epidemiological studies based on categorical measures of mental illness, rather than on the vaguer, continuous indicators of mental health used in earlier studies. This generation also paid attention to the inter-rater reliability of diagnoses, which in part compensated for the absence of biological markers and for other problems concerning the validity of what was defined as mental disorders. Finally, some methods used in third-generation psychiatric epidemiology resembled those of general epidemiology.

Yet despite methodological breakthroughs and an increased institutional stability, psychiatric epidemiology was undergoing a profound conceptual crisis. As we shall see, this involved some of the most fundamental convictions embraced by the discipline’s disciplinary matrix. The result was a widening rift between the discipline’s
institutional consolidation and its intellectual foundations, a problem to which we now turn.

1980: the consolidation of a discipline? Institutional stability and conceptual controversy

By providing psychiatrists with operational criteria for each mental disorder it covered, the DSM-III brought about a major transformation in the manner in which epidemiological studies of mental disorders were carried out. But as North American researchers had begun to develop operational criteria for diagnostic measurements to be used in epidemiological and other types of study before DSM-III appeared in 1980, that year can be seen as a point of culmination as well as a turning point. In the previous decade, sociologist Lee N Robins and the then-NIMH psychiatrist and researcher, Darrel Regier, had developed the Diagnostic Interview Schedule (DIS), a structured instrument capable of generating diagnoses according to the then-future DSM-III criteria, for the Epidemiological Catchment Area (ECA) Study. That study, which they spearheaded, was initiated in 1978 and launched 2 years later in five areas of the USA.

Although it would spur an abundance of epidemiological research, DSM-III nevertheless appeared on the crest of controversies and debates built up over the previous years around the direction psychiatric epidemiology should take. The controversy first arose during the 1960s and early 1970s, in the series of round tables organized by the Milbank Memorial Fund to promote psychiatric epidemiology as a basis for mental health policy. It crystallized more clearly—and before a nationwide public of psychiatric scientists—on the occasion of DSM-III’s publication. The essence of the controversy is illustrated by an exchange published in several issues of the major North American psychiatric research journal, Archives of General Psychiatry, between the Columbia University sociologists, Leo Srole and Anita K Fisher on the one hand, and Yale University researchers Myrna Weissman and Gerald Klerman on the other hand. The catalyst was Srole and Fischer’s now classic paper, ‘The Midtown Manhattan Longitudinal Study’ vs ‘The Mental Paradise Lost’ Doctrine’, published in the Archives in February 1980. Srole and Fischer used the data from their impressive socio-epidemiological survey, launched in the early 1950s, to attack a commonly held view among psychiatrists and psychoanalysts of the day, namely that mental health tends to deteriorate with the progress of civilization. As their study examined an area of Midtown Manhattan characteristic of large, modern cities, Srole and Fischer were well positioned to argue against the ‘mental paradise lost’ view of the modern era. The two sociologists attributed that view to a sexist bias on the part of psychiatrists seeking to show that women had more mental disease than men, especially in the modern era. They countered that claim with results from the Midtown Manhattan study to suggest the opposite tendency: women’s subjective well-being actually tended to improve as they became more emancipated.

The same issue of the Archives included a letter to the editor signed by Myrna Weissman, then professor of psychiatric epidemiology at Yale University, and her husband, the psychiatrist Gerald Klerman. Their letter did not criticize the conclusions of Srole and Fischer’s article but rather questioned the underlying assumptions of the Midtown study and the methodology which had produced the findings on which the two sociologists based their argument. For instance, Weissman and Klerman opposed the idea that there are no discrete mental diseases, but only a continuum between the normal and the pathological. This ‘continuum perspective’ undergirded the psychological scales and mental health measures which the operational criteria from DSM-III tried, at least partly, to displace, albeit with mixed success. But above all, Weissman and Klerman attacked the assumptions behind Srole and Fischer’s sociological hypotheses. They criticized the sociologists for perpetuating a style of epidemiological inquiry that they considered to be outdated and out of sync with the new epidemiology of ‘risk factors’ and the focus on specific, well-delineated diseases. In their view, the publication of the DSM-III with its operational criteria would rapidly foster a renewed psychiatric epidemiology.

This controversy foreshadowed the role that the DSM-III would come to play from a historical perspective. Writing in the following decade, Klerman characterized the advent of DSM-III as ushering in a radical transformation that was nothing less than a change of paradigm within psychiatry, in the technical sense Kuhn gave to this expression. That is, from an epidemiology centred on the idea of mental health, the DSM-III triggered the shift towards an epidemiology centred on specific psychiatric disorders. Klerman thus addressed some of the same concerns as the present paper, concerning the status of psychiatric epidemiology as a scientific discipline. However from a Kuhnian perspective as well as on the basis of the history of the discipline presented thus far, his argument faltered on several points.

First, Klerman addressed the problem of psychiatric epidemiology only from the point of view of psychiatry, as if the two disciplines could be superimposed, or as if psychiatric epidemiology were merely a branch of psychiatry. The paradigm change Klerman referred to did not involve epidemiology in general. Rather, he was implying that
with DSM-III, psychiatric epidemiology would be able to move closer to other types of medical research being practised at the time—in cardiology, oncology, and elsewhere—and which were turning to the search for risk factors. Second, although the DSM-III would profoundly change the nature of epidemiological studies in psychiatry, Klerman was not sensitive to the potential for resistance within the community of psychiatric epidemiology researchers itself. In fact, a profound resistance arose immediately, as we will show below. Third, and more fundamentally, even if many theoretical assumptions and methodological implications of DSM-III were to become influential within North American psychiatry during ensuing decades, it can hardly be concluded that DSM-III was leading (or has led) to a real ‘scientific revolution’ in the field, which Klerman’s use of the paradigm concept implied. In fact, the DSM continues to experience at best an ambivalent success from a scientific viewpoint. The last revision of the DSM, the DSM-5, published in 2013, has been criticized by many clinicians and researchers for its lack of validity. In any case, from a Kuhnian perspective, Klerman was misusing the paradigm concept by not referring to the discipline of psychiatric epidemiology as a whole. Rather, he tended to identify a current within psychiatry as a paradigm. Klerman himself was aware of this terminological problem. In a later article, he did recognize that the existence of five rival schools—biological, psychoanalytical, interpersonal, cognitive-behavioural and social—within psychiatry challenged the very concept of a paradigm in the Kuhnian sense, unless one were willing to admit the possibility that psychiatry as a whole was still in a ‘pre-paradigmatic’ age. Thus his observation inadvertently illustrated the point we are arguing in this paper. As we saw, Thomas Kuhn breaks down the disciplinary matrix into its institutional (disciplinary) and cognitive (intellectual) components. From the institutional or organizational perspective, psychiatric epidemiology had more or less consolidated by 1980. But what of the intellectual dimensions of the disciplinary matrix, or the conjuncture of its four cognitive components—symbolic generalizations, shared values, exemplars and metaphysical models? How cohesive was this aspect of psychiatric epidemiology by 1980?

With regard to the first type, symbolic generalizations and all forms of expressions ‘deployed without questions or dissent by group members’, psychiatric epidemiology adopted the vocabulary and statistical toolkit of general epidemiology very early on, with notions such as morbidity, continuum, density, incidence, validity, reliability and different study designs, such as experimental, quasi-experimental, longitudinal etc. The second type of cognitive element, or values of psychiatric epidemiology, are also those of epidemiologists in general, and they contributed strongly to defining the scientific ambition of psychiatric epidemiology as a new discipline with regard to the uses to which psychiatric statistics had been put in the past. These values include scientific rigour in data analysis as well as the humanitarian vocation of epidemiology as promoted, for example by the WHO, with the primary objectives of human welfare and the respect of individuals. This could not contrast more with North American and European eugenic projects between the two world wars, in which statistical research on mental health was used to promote the existence of one kind of individual over another, and often through coercive means.

With regard to the third type, exemplars or paradigmatic examples available in textbooks and systematically cited by the members of the discipline, psychiatric epidemiology took shape around several studies which became canonical references, despite the tremendous differences in their scope and methodologies. Examples include Joseph Goldberger’s work on pellagra in the mid 1910s, 1966 and Dunham’s study of urban areas and schizophrenia in 1939, 1939 Hollingshead and Redlich’s study of mental illness and social class in 1958, the Stirling County study by Leighton and his colleagues in 1959, the Midtown Manhattan Project initiated in 1962, the International Pilot Study of Schizophrenia initiated by the WHO in 1966 and the Epidemiological Catchment Area Study launched in 1978. Yet the results yielded by most of these studies are difficult to reproduce, and their methodological

The disciplinary matrix of psychiatric epidemiology before 1980

What did the disciplinary matrix of psychiatric epidemiology actually resemble in 1980? Contrary to Klerman and Susser and Stein, who sum up the history of epidemiology as the succession of several paradigms, we propose that by 1980, psychiatric epidemiology in the USA had not even reached the status of a ‘normal science’. That is, the theoretical framework of psychiatric epidemiological studies was neither coherent nor unitary, hence not paradigmatic. And paradoxically, from the 1980s on, the discipline continually strengthened its institutional dimension, but remained weak (or ‘pre-paradigmatic’) intellectually.

To elaborate this point, let us return to the framework presented at the beginning of this paper. As we saw, psychiatric epidemiology before 1980

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- Klerman was not sensitive to the potential for resistance within the community of psychiatric epidemiology researchers itself. In fact, a profound resistance arose immediately, as we will show below.
- The DSM continues to experience at best an ambivalent success from a scientific viewpoint. The last revision of the DSM, the DSM-5, published in 2013, has been criticized by many clinicians and researchers for its lack of validity. In any case, from a Kuhnian perspective, Klerman was misusing the paradigm concept by not referring to the discipline of psychiatric epidemiology as a whole. Rather, he tended to identify a current within psychiatry as a paradigm. Klerman himself was aware of this terminological problem. In a later article, he did recognize that the existence of five rival schools—biological, psychoanalytical, interpersonal, cognitive-behavioural and social—within psychiatry challenged the very concept of a paradigm in the Kuhnian sense, unless one were willing to admit the possibility that psychiatry as a whole was still in a ‘pre-paradigmatic’ age. Thus his observation inadvertently illustrated the point we are arguing in this paper. As we saw, Thomas Kuhn breaks down the disciplinary matrix into its institutional (disciplinary) and cognitive (intellectual) components. From the institutional or organizational perspective, psychiatric epidemiology had more or less consolidated by 1980. But what of the intellectual dimensions of the disciplinary matrix, or the conjuncture of its four cognitive components—symbolic generalizations, shared values, exemplars and metaphysical models? How cohesive was this aspect of psychiatric epidemiology by 1980?

With regard to the first type, symbolic generalizations and all forms of expressions ‘deployed without questions or dissent by group members’, psychiatric epidemiology adopted the vocabulary and statistical toolkit of general epidemiology very early on, with notions such as morbidity, continuum, density, incidence, validity, reliability and different study designs, such as experimental, quasi-experimental, longitudinal etc. The second type of cognitive element, or values of psychiatric epidemiology, are also those of epidemiologists in general, and they contributed strongly to defining the scientific ambition of psychiatric epidemiology as a new discipline with regard to the uses to which psychiatric statistics had been put in the past. These values include scientific rigour in data analysis as well as the humanitarian vocation of epidemiology as promoted, for example by the WHO, with the primary objectives of human welfare and the respect of individuals. This could not contrast more with North American and European eugenic projects between the two world wars, in which statistical research on mental health was used to promote the existence of one kind of individual over another, and often through coercive means.

With regard to the third type, exemplars or paradigmatic examples available in textbooks and systematically cited by the members of the discipline, psychiatric epidemiology took shape around several studies which became canonical references, despite the tremendous differences in their scope and methodologies. Examples include Joseph Goldberger’s work on pellagra in the mid 1910s, and Dunham’s study of urban areas and schizophrenia in 1939, Hollingshead and Redlich’s study of mental illness and social class in 1958, the Stirling County study by Leighton and his colleagues in 1959, the Midtown Manhattan Project initiated in 1962, the International Pilot Study of Schizophrenia initiated by the WHO in 1966 and the Epidemiological Catchment Area Study launched in 1978. Yet the results yielded by most of these studies are difficult to reproduce, and their methodological
biases are commonly critiqued. Few epidemiologists would claim that the studies conducted in psychiatric epidemiology before the 1980s can be summarized as a series of successive discoveries. This absence of a cumulative history reveals an inherent fragility of the matrix of psychiatric epidemiology.

The final type of element in a disciplinary matrix is what Kuhn calls its ‘metaphysical part’, by which he means the adhesion of researchers to certain privileged theoretical models. In the history of psychiatric epidemiology, three important concepts have functioned as ‘preferred or permissible analogies and metaphors’ that enable researchers as a group to adhere to common models: infectious contagion, hereditary transmission and moral epidemics. Interestingly, even if they did not contribute to decisive scientific progress, these three models polarized the research enterprise of epidemiological puzzle-solving during the entire past century.

Infectious contagion

The idea that insanity could be transmitted by germs, or that it stemmed from an infection, has recurred repeatedly in the history of psychiatry. The theory of focal infection, which goes back to antiquity, postulated that a local and persistent, even slight, infection could be at the origin of the most serious systemic disorders. In the early 20th century, the heyday of bacteriology and the hygienist movement, this theory met with considerable success throughout the medical field. Many doctors and dentists maintained that by treating dental infections, cleaning sinuses and removing tonsils, diseases ranging from rheumatism to gastric problems, anaemia, severe depression and psychosis could be fought. For instance Henry Cotton, medical director of the Trenton Psychiatric Hospital in New Jersey, became famous in the 1920s when he undertook to cure patients by removing all sorts of organs: teeth, tonsils, gall bladder, stomach and spleen. He defended the idea of an insidious transmission of mental diseases, notably within the family, through contact between individuals and perhaps even, in the case of pregnant women, through in utero contamination. Similarly, examples of contagion in the case of mental illnesses were presented in the 1950s and 1960s to support the scientific rationale for applying epidemiological methods to mental illnesses, as Anne Lovell points out in this issue of IJE. Although not dominant, the infectious contagion model survives today in some psychiatric thinking (for example, the idea that an ‘invisible plague’ explains the rise of mental illness from the 18th century to the present) and in the mass media.

Even if this model has proved harmful in the past, the possibility does exist that some mental disorders may be due to the transmission of an unknown infectious agent. (Think for example of the history of general paresis, which psychiatrists discovered to be caused by syphilis.) Yet, in the scientific literature, it is sometimes difficult to understand whether authors are referring to the infectious contagion model in the literal sense of the term (contagion as a transmission through actual contact) or in a metaphorical sense. A preferable alternative to mere ‘behavioural’ contagions (i.e. without hypothesizing the spread of an infectious agent) is ‘moral epidemics’, which will be discussed below.

Hereditary transmission

With the rediscovery of Mendel’s laws in the early 20th century, hereditary research in psychiatry proliferated. For many psychiatrists, the idea of a hereditary transmission of insanity was soon considered as a contagion risk far more insidious than any infection caused by a germ. In the USA, figures like: the lawyers Prescott Hall and Madison Grant; the biologist Charles Davenport; the director of the Eugenics Record Office, Harry Laughlin; as well as two physicians often considered important precursors of psychiatric epidemiology, Horatio Pollock and Thomas Salmon; contributed significantly to spreading the idea that psychiatrists were responsible for protecting the ‘genetic vitality’ of the North American population, notably against immigration, which was considered a vehicle for spreading mental diseases. As an alternative model of contagion, heredity proved more worrisome than the infectious model, in so far as a recessive gene could be transmitted in an inconspicuous way and might take several generations before being expressed. If insanity could be transmitted by recessive genes, and not merely by dominant genes, psychiatric examination of new immigrants entering the country no longer sufficed to prevent mental illness. Rather, investigations would have to include information on a family’s ancestors, or at least a sufficiently long period of psychiatric surveillance of new arrivals in North America. Far from focusing on the individual, eugenicists thus understood very early on that new statistical methods and family studies would need to be mobilized so that society could defend itself against the proliferation of mental diseases. From the 1930s on, the development of family and twin studies exerted an enormous influence within psychiatric epidemiology.

Moral epidemics

Emile Durkheim’s sociology exerted a profound intellectual influence on the development of psychiatric epidemiology, although the discipline has not always
acknowledged this. His work on suicide, published in Paris in 1897, quickly earned him international renown in the social sciences. The first English translation, however, was not published until 1951; and even though his ideas were frequently opposed to those developed by the Chicago School of sociologists, certain commentators have noted significant similarities, especially with the earlier Chicago School members.78,80

One of the most original features of Durkheim’s work was his conceptualization of a phenomenon that he characterized as a moral epidemic. Holding that the sociologist’s job was to bring attention to the social norms and pressures likely to explain the variation in rates of suicide according to geographical distribution, social origin or other factors, Durkheim defended an original conception of epidemics. He explicitly contrasted epidemic to contagion as two very different forms of normative pressure on the individual. Behavioural contagion is inter-individual and based on effects of imitation. It involves a limited number of individuals. A moral epidemic, according to Durkheim, is essentially of a sociological nature, characteristic of the normative pressures on a group. He summed this contrast as follows: ‘A [moral] epidemic is a social fact, produced by social causes; contagion consists only in more or less repeated repercussions of individual phenomena’.79 This notion enabled him to defend a sociological approach to disease and mental health in which the latter were characterized as social facts, given that their distribution in a population followed patterns correlated with social characteristics such as gender, marital status and urban vs rural, as well as social norms. Such a distribution provided immunity against merely psychological interpretations.

Durkheim’s conception bears a family resemblance with an important theoretical assumption that stimulated all medical and sociological reflection in the early 20th century: the holistic perspective. In the 1920s USA, the first partisans of constitutional medicine were inspired by a holistic approach.81 Later, pioneering epidemiologists like Major Greenwood and John Gordon agreed that the epidemiological perspective should take the group as its unit of analysis. In so doing, they assumed that the whole could not be reduced to the sum of its parts. Epidemiology was therefore defined as a population-based style of thinking that was explicitly opposed to the clinical viewpoint of the doctors.20,43

Until the 1980s, the disciplinary matrix of psychiatric epidemiology attempted to integrate these three metaphysical guiding models. The spread of a noxious agent, the presence of a gene and the distribution of social factors were considered the three main candidates for explaining high rates of mental disorders in the general population. Within psychiatric epidemiology, these ideas nurtured an ideological common ground based on a multi-causal and holistic theoretical framework, which in turn contributed to the vitality of the institutional partnership between social scientists and physicians within psychiatric epidemiology. But it was not to last.

**A split in the matrix of the discipline**

Partly in reaction to the sociological criticism of psychiatry in the 1970s,82 many psychiatrists began to adamantly reject the idea that true mental diseases could spread like moral epidemics. A notable example is Samuel Guze, who deplored the many ‘easy generalizations about the effects of social factors on psychiatric disorders’ which he thought posed a threat to the medical model in psychiatry.83 Others claimed that the disciplinary role of sociology inside and outside mental health was becoming ever more confused,84 and sociologists were becoming ‘deskilled’ in epidemiology.82 The result was an ‘exhaustion’ of the original interdisciplinary perspective adopted during the past decades.82

In this light DSM-III, which as we noted earlier reinforced the medical model in a field influenced by sociological models, represents a turning point in psychiatric epidemiology’s development. Psychiatric epidemiology was henceforth torn in two radically opposite directions: a medical-centred approach, which attempted to treat mental disorders as if they were diseases like any other; and a socio-epidemiological10,12,63 or, better still, an ecological approach.10,12,63 The split between these two broad approaches at the beginning of the 1980s still has implications for today’s psychiatric epidemiology.

The second direction in fact illustrates exactly where the crack had formed in psychiatric epidemiology’s disciplinary matrix. The key culprit was the by then archaic notion of ‘ecology’. In the 1920s, Chicago School sociologists had drawn inspiration from the botanist Frederic Clements’s seminal book on plant ecology, *Plant Succession: An Analysis of the Development of Vegetation* (1916).85 The sociologist Roderick D McKenzie was the first to justify the value of broadening the application of the ecology concept to human communities.86 This idea was also central to Faris and Dunham’s perspective when they undertook their study of the ‘ecological distribution of mental diseases’ in Chicago.85 The notion of human ecology rapidly spread to social medicine, notably in the writings of John Ryle. The chairman of the Royal Society of Medicine, Sheldon Dudley, even argued that the ecological method was equivalent to the epidemiological method.87 John Gordon reiterated this equivalence in 1950, when he defined epidemiology as a ‘medical
ecology. He touted the advantage of the ecological approach as allowing disease to be addressed as a mass phenomenon, taking into account the ‘dynamic balance’ existing between the triad of pathogenic agent, the host and the general environment. After Gordon, the ecological perspective in psychiatric epidemiology continued to be treated as an overarching perspective aimed at promoting: (i) the search for multiple causes; (ii) the emphasis on the complex entanglement of causes; and (iii) a shift in causality from attention to the pathogenic agent to emphasis on the host and the environment, with no a priori hierarchy between the two sets of determinants.

Early on, these three goals characterized the specificity of the ecological perspective, its intellectual content and its political agenda. However, the dominant position attained by the medical model in psychiatry at the beginning of the 1980s led many psychiatric epidemiologists to focus on individual-level factors in mental disorders. Social determinants were relegated to the hypothetical role of ‘modifying or facilitating causes’ rather than considered possible true causes of mental disorders.

The division among scholars between these two radically opposite approaches—the medical-centred approach the DSM-III embodied, and the ecological approach which several of the most influential actors in the history of psychiatric epidemiology preferred—split the disciplinary matrix of psychiatric epidemiology at the cognitive level. Hence the paradox of psychiatric epidemiology: it had flourished institutionally, but as an intellectual endeavour it would lag behind. For without cognitive coherence, the disciplinary matrix was not crystallized. Psychiatric epidemiology was stuck in a pre-paradigmatic phase. We can further hypothesize that it remained pre-paradigmatic during the following three decades, since the DSM medical approach, although dominant, has not contributed to notable scientific progress in psychiatric epidemiology so far.

**Conclusion**

With the publication of the DSM-III, a milestone in the history of psychiatry, the discipline of psychiatric epidemiology stood poised to move closer to general epidemiology by providing epidemiological studies with criteria for measuring specific mental disorders. Circa 1980, the DSM-III had set the stage for relegating the socio-epidemiological survey to medical sociology, thereby more or less excluding it from the disciplinary field of psychiatric epidemiology itself. The dominance of the medical model further relegated inquiry into distant causes of mental diseases to a domain beyond the ‘epidemiological’. These moves did not eliminate the tension between the two approaches. Thus, rather than constituting a veritable paradigm shift within the discipline of epistemology, DSM-III’s arrival revealed a split in the disciplinary matrix of psychiatric epidemiology that had been under way for decades, and that would remain discreet but persistent during the following decades.

The question of whether or not psychiatric epidemiology appears much more homogeneous today than it was when DSM-III was launched over 30 years ago cannot be examined within the scope of this paper. Whereas we would caution against assuming that the split has been replaced by a single model, given the discipline’s somewhat insecure theoretical foundations, only future studies can determine the extent to which the intellectual division remains within the discipline.

As a scientific enterprise—and not merely as a profession—psychiatric epidemiology is affected by the limitations of general epidemiology and by the theoretical conundrums of modern psychiatry. We suggest that the division between a socio-epidemiological approach and a medically centred approach described here characterizes general epidemiology as well, judging by the critique some epidemiologists make of the epidemiology of risk factors. However, this division is stronger in psychiatric epidemiology. Disagreements among researchers over how to use logical inference and integrate several levels of causation into one coherent picture of illness run through many epidemiological subspecialties other than psychiatric epidemiology. Within psychiatric epidemiology, such disagreements are intensified owing to the fact that the biological causes of mental disorders are still massively unknown. This is a non-negligible consideration, for the absence of a consensus about causation not only undermines the coherence of psychiatric epidemiology, it also undermines the coherence of the psychiatric field.

On the other hand, the increasing influence of non-reductive approaches in the psychiatric literature during the past decades may indicate that the field in general is experiencing a conceptual evolution towards a more pluralistic and complex discipline. Or, on the contrary, it may indicate that psychiatry in general—and not only psychiatric epidemiology—still lacks intellectual coherence and thus has not yet reached the status of a normal science, in Kuhn’s sense.

These tendencies need to be verified. The criteria of the Kuhnian framework concerning the status of a scientific discipline are quite demanding. One might even speculate whether the absence of coherence and convergence within a disciplinary matrix is specific to psychiatric epidemiology or is true for other epidemiological subspecialties (cardiovascular, genetic etc.) or even for general epidemiology—questions that are beyond the scope of this paper. What is certain is that nowadays researchers in psychiatric epidemiology are not merely confronted with ‘anomalies’ that
could be resolved by ‘normal means’. They still express hesitancy and uncertainty about the most fundamental conditions necessary for the full consolidation of their discipline. In conclusion, given the general ideological tendency in modern medicine to localize health problems within the individual rather than in society, debates on the relevance of the ecological perspective and its renewal within psychiatric epidemiology, could prove crucial in the next few years for reassessing causal explanation in that discipline, as well as in psychiatry more generally.

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