GEOFFREY HARDING, *Opiate addiction, morality and medicine: from moral illness to pathological disease,* Basingstoke and London, Macmillan Press, 1988, 8vo, pp. vii, 98, £25.00.

In this slender volume, Dr Harding has set himself the task of explaining how a moral condemnation of opium use arose in Britain in the latter half of the nineteenth century, only to be supplanted by a medical concept of drug addiction as a pathological disease. He claims that he has diverged from "conventional histories" and, guided methodologically by Michel Foucault, has "tried to describe the social relations which made possible these descriptions and the way in which they have been taken seriously" (p. 85).

The change from a moral to a medical view of opium use in an interesting and topical subject. Despite two recent books that cover much of the same ground, there is ample room to see the phenomenon from a new perspective. Unfortunately, Dr Harding's book does not do so. On the one hand, the book is filled with errors, omissions, and misjudgements. On the other hand, some of the book's most critical chapters are simply borrowed, acknowledged or unacknowledged, from existing literature.

Dr Harding argues that the moral perspective on opium use was shaped during the last decades of the nineteenth century, largely through the efforts of the Quaker-dominated Society for the Suppression of the Opium Traffic. The Quakers were successful in defining and holding to their opposition to opium because they believed unequivocally that the drug "pathologically debilitated the moral faculty" of those who smoked it. Yet there was a considerable opposition to this view, culminating in the 1895 Indian Opium Report, a highly-publicized exoneration of opium by some of Britain's most respected medical men and colonial officials. Dr Harding has exaggerated the influence of the SSOT by omitting any reference to this considerable body of dissent. In his discussion of the passage of the 1906 parliamentary resolution that effectively ended the Indo-Chinese opium trade, Dr Harding fails to mention the substantial role of the Liberal Party. His overly-complex explanation for the changed perception of narcotic drugs in literature rests, implausibly, on an entirely atypical (and bad) novel by Aleister Crowley, *The diary of drug fiend* (1922). Finally, although Dr Harding claims to be inspired by Michel Foucault's methodology, Foucault is not mentioned until the end of the book, and then only in passing. Whatever methodological contribution Foucault's work may have made is not at all clear.

Much of Dr Harding's book is borrowed from other historians. His chapter on 'Opium and the British' is drawn almost entirely from the articles of Virginia Berridge. The Society for the Suppression of the Opium Trade is discussed more perceptively and accurately in Bruce Johnson's article, 'Righteousness before revenue: the forgotten crusade against the Indo-Chinese opium trade', *Journal of Drug Issues* (1975), which Dr Harding cites. Significant portions of his chapter, 'From moral illness to pathological disease', are unacknowledged, paraphrased versions of paragraphs from my book, *Secret passions, secret remedies: narcotic drugs in British society, 1820–1930* (1983). For example, the following paragraphs, dealing with the contribution of Dr T. D. Crothers to the theory of opiate addiction, are taken from Dr Harding's book and mine:

"T. D. Crothers made the most ambitious attempt to resolve this problem by distinguishing between 'morphinism' and 'morphinomania'. The former 'describes a condition following the prolonged use of morphin', while the latter designates 'the condition of persons in whom the the impulse to use morphin is of the nature of a mania, possessing the mind and dominating every thought, leaving but one supreme desire—to procure morphin and experience the pleasure it gives'. The morphinist, although he had contracted the physical habit, retains his moral sensibilities and sanity, whereas the morphinomaniac has lost both". (Parssinen, p. 93).

"He [T. D. Crothers] distinguished morphinism and morphinomania. Morphinism describes 'a condition following the prolonged use of morphin (sic)', while morphinomania is designated, 'the condition of persons in whom the impulse to use morphin, is the nature of a mania, possessing the mind and dominating every thought, leaving but one supreme desire—to procure morphin and experience the pleasure it gives'. The morphinist, having contracted the habit, was held to have retained his moral sensibilities and sanity and thereby wished to be cured; while the morphinomaniac had lost both and was irredeemably addicted". (Harding, pp. 61–2).

Other borrowed material includes paragraphs on the following pages: Parssinen, 86, Harding, 58; Parssinen, 87, Harding, 59; Parssinen, 88, Harding, 59. At a minimum, these errors, omissions, and borrowings raise questions about the depth of Dr Harding's research and the originality of his analysis.

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VIKTOR HAMBURGER, *The heritage of experimental embryology: Hans Spemann and the organizer,* Monographs on the History and Philosophy of Biology, New York and Oxford, Oxford University Press, 1988, 8vo, pp. xii, 196, illus., £22.50.

I came into embryology as a postdoctoral fellow in 1974, when the subject was rather unfashionable and neglected. Over my first few years I spent long hours in the library and read with fascination of an earlier "golden age" between the two World Wars. Slowly I pieced together the story of the organizer. It took time because most of the papers had to be retrieved from dusty stacks and because most were in German, a language I read poorly. How I would have loved to read Professor Hamburger's book then!

The organizer graft is a transplantation of tissue from the dorsal lip of the blastopore of an amphibian gastrula to the prospective ventral lip of another individual. When performed correctly, it yields a mirror symmetrical double dorsal embryo, rather like a pair of Siamese twins joined belly to belly. The lower half of the duplication, often called the secondary axis, consists of a notochord derived from the graft and the remainder, mainly somites and neural tube, is induced from the ventral tissues of the host. The organizer graft was first reported in a famous paper by Spemann and Mangold in 1924.

Hamburger was a graduate student in Spemann's department at Freiburg during the years that the organizer grafts were first being performed. In his book he describes the scientific background, the character of experimental work at the time, and something of the personality of the individuals involved. It is certainly sobering to be reminded of the experimental difficulties of the time. In my laboratory today we generate two or three batches of *Xenopus* eggs each week, we manipulate their developmental rate with incubators at different temperatures, and we protect our grafts and explant cultures from infection with antibiotics. During the 1920s, embryologists had to collect their eggs (usually newt eggs) from the wild during the brief breeding season in the spring. The whole year's experiments would be performed in a mad rush, with horrific mortality rates due to poor culture conditions and to infection. It is because of these difficulties that the famous organizer paper describes only six cases, of which only two show good double-dorsal duplications.

Hamburger goes on to describe the subsequent work on early amphibian development. Unfortunately the organizer, which we now regard as the source of a dorsalizing positional signal, was then seen mainly as an agent of neural induction. In 1932 three groups, all in Germany, simultaneously reported that *killed* organizer tissue had neural-inducing activity. This sparked off the famous "gold rush" for the chemical nature of the organizer, using neuralization of gastrula ectoderm as the assay. The hopelessness of such a task, with the biochemical techniques available over 50 years ago, may be assessed from the fact that we have only just succeeded in detecting a few picrograms per embryo of the mesoderm-inducing factor bFGF, using affinity HPLC and ultrasensitive immunochemical methods. In fact, neural induction, particularly in newts, is a rather unspecific process and many substances can trigger it, including some synthetic chemicals which do not occur in embryos at all. This realization caused much consternation, and the high morale and sense of excitement evident in the literature of the '20s and '30s faded rapidly. The field as a whole went into eclipse during the Second World War, partly because of the failure of the gold rush, but also because of the dispersal of the German scientists whose efforts had led the way throughout this period.

The real legacy of the period was not so much the work on the organizer, which, with the benefit of hindsight can be seen as largely misdirected, but rather the formulation of a set of self-consistent concepts for the description and analysis of early development. For example, fate,