profiles, sex ratios, and season of death, he shows that Late Pleistocene mastodont remains separate out into two distinct death groups, a winter–spring group consisting of equal numbers of males and females and lacking evidence of butchering and an autumn group consisting of young to prime adult males that appear to have been butchered.

Fisher's paper is also exciting because of its methodological innovations, especially the recognition of daily, fortnightly, and annual incremental growth lines in the dentine of mastodont tusks, which permit accurate assessment of season of death. According to Fisher, the same approach can be applied to the tusks of other proboscideans, opening the way to unraveling the hunting strategies of Clovis (Paleo-Indian) mammoth hunters in western North America and Upper Paleolithic mammoth hunters in Eastern Europe and the Soviet Union.

Though *The Evolution of Human Hunting* is a valuable contribution to the growing debate about human predation, it is strikingly incomplete in some ways. I have already mentioned the absence of any discussion of hunting and scavenging by the earliest hominids—the Australopithecines. Other obvious omissions are comparative studies of hunting among extant hunter-gatherers and non-human higher primates, both topics that figure prominently in current discussions of human hunting.

But there are other, larger omissions as well. The volume really addresses only the methodological side of the debate—for example, how we can distinguish human from non-human agents involved in the formation of a site or how we can discriminate between hunting and scavenging. The theoretical issues that underlie these and other questions are not represented. For example, why do humans (and non-human primates) hunt, or scavenge, in the first place? Is protein the principal target? or fat? or total calories? or are these nutritional aspects secondary to social factors? The answers to these questions are not as simple or obvious as they might at first seem. Why do we really want to know whether hominids hunted or scavenged? What really is at stake in this issue?

This is perhaps also the time to inject a note of caution. Though there have been a number of provocative and convincing arguments, several clearly articulated in the present volume, that suggest we have overemphasized man's organized, technologically aided hunting prowess in the Pleistocene, there is now a stampede toward the opposite pole, to a view of pre-modern humans as essentially opportunistic scavengers who lacked "planning depth," sophisticated "cognitive" skills, and perhaps even language and who wandered "irregularly," almost dumbly, over the landscape in search of food. I fear that the pendulum is swinging much faster and much farther than either current theory or data justify, and we will find ourselves a decade or so down the line wondering how we could ever have been so naïve or blind. Though we have learned a great deal in the last few years, and our data and models are undoubtedly vastly improved over what they were before, we still have a long way to go to properly understand the role that hunting played in making us what we are today.

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## Rationality and Risk

**Classical Probability in the Enlightenment.** LORRAINE DASTON. Princeton University Press, Princeton, NJ, 1988. xviii, 423 pp., illus. $49.50.

The ancient geometers were—by present standards—confused: they ran together the empirical problem of measuring the earth with the problem of the truth of axioms about points and lines. The errors of the ancients, however, were forgivable. They didn't distinguish between the validity of axioms and their application to their implicit model of space because they had only one model of space to think with. Similarly, the probabilists of the Enlightenment, who espoused what is now called the "classical" conception of probability, confused objective probability, a feature of the world, and probability as a subjective fact, a degree of judgment or certainty. Lorraine Daston in this volume argues that this failure on the part of the classical probabilists to grasp the difference is grounded in the cases they thought in terms of.

The strategy by which Daston develops her argument is illustrated by her treatment of the origins of the concept of equiprobability, a key theoretical idea for the classical probabilists. The idea actually derived from contemporary Continental legal thinking on "aleatory" contracts, that is, contracts involving risks, such as annuities or insurance policies. Contracts were made equitable by adjustments in the rate of return of the risk-bearer. The concern of the lawyers was to distinguish equitable, hence valid, contracts, from inequitable ones. Eighteenth-century probabilists simply took over this problem of equitability, indeed often followed the vocabulary of contractual law in their formulations. Huygens, for example, used the model of equitable exchange in his analyses of games. His reasoning inverted the modern way of thinking about the problem: for Huygens "expectations were equal when they could be fairly traded for one another," not, as one would say today, the game is "fair because the probabilities . . . are equal for all players" (p. 26). From a modern perspective, the difficulty with the classical formulation is this: how do we know a trade is fair? Today we would determine fairness from a determination of the numerical probabilities. A different kind of answer to this question was to be found in 18th-century legal practice: contracts were judged "by eye." The ability to make such judgments defined a mathematical task for the classical probabilists, but this task preserved the running together of subjective and objective probability. The classical probabilists understood their problem as one of formally describing the "implict and immutable calculations" (p. 52) of the minority of persons adept at judgments involving equity and risk, such as insurance men and gamblers. In the 18th century insurers and gamblers approached their tasks similarly, by intuitive assessments of good bets or risks.

The initial attempts to model reasonable judgment were failures, but interesting ones. The intuitions of risk-takers proved to be difficult to reconcile with any mathematical formulation, and it became apparent that the probabilistic rationality of the equity-seeking jurist and that of the prudent businessman were distinct. But the attempts had important consequences. Classifying problems together on mathematical grounds enabled the probabilists to distinguish the mathematical issues from the substantive problems of risk that had inspired earlier efforts and to extend the range of application of their mathematical ideas in new directions. New conceptual models of probability problems, such as the model of drawing black and white balls from an urn, replaced consideration of actual games of chance or risk situations, and the use of simple tables of rates redefined the practical domain of probability. Tables could be used as substitutes for the complicated internal weighing of numerous intuitive considerations that insurers of the older "betting" variety engaged in. But the new tabular methods were still crude, and probably not a genuine improvement. In any case, they had no effect on practice for some time. Annuities continued to be sold by governments without consideration of such basic risk factors as the age of the annuitant, and lotteries with absurd odds continued to flourish. "Luck" was treated, even by the rational, as a natural quality. Insurers continued to make decisions based on their practiced intuitions, not on tables and routinized lists of risk factors.

In the course of the 18th century, however, a change took place in public attitudes toward insurance. In the later part of the century, the purchase of life insurance came to be thought of as a conservative, risk-aversive choice rather than a bet. The change, Daston suggests, was not a simple case of increasing rationality. Purchasers of the new table-based insurance policies of the Equitable Society, which minimized the gambling element, paid dearly for the privilege: the participants would probably have done better with the old kind of insurance, in part because of the extreme fiscal conservativism of the Society, which resulted in a huge reserve and low payouts. But the character of the assumptions that intelligent people made about the world were changing in ways that fit with the Society's treatment of the probability of death as an objective feature of the world. In general "regularity" replaced "ignorance" as the key concept in probabilistic thinking. The urn model, accordingly, was replaced by the model of natural laws governing fixed or at least very orderly aggregate-level rates, such as the increasingly predictable death rate.

The subjective aspect of the classical interpretation had a different fate. The problem of weighing court testimony probabilistically, which had been a central concern of the classical probabilists, was abandoned: mathematicization, as Daston observes, is not irreversible. Also, informal probability concepts figured heavily in the associationist psychological theory shared by writers on the theory of knowledge in the 18th century. The effect of associationism, which posited a mechanism of belief or judgment formation by which repeated jointly occurring impressions were transformed into expectations, was to support the confusion of subjective and objective probability. Associationism faded, for many reasons. The experience of the French Revolution was hard on democratic ideas of universal rationality. The Lockean idea of common in-built cognitive mechanisms was replaced by the idea of social mathematics as a technique that an elite of experts could employ as an antidote to mob politics. The implicit practical model of probability from which the classical interpretation had departed and the psychological and social ideas that had propped it up thus disappeared, piece by piece; the classical interpretation disintegrated with them.

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## Lamarck in Context

**The Age of Lamarck**. Evolutionary Theories in France, 1790–1830. PIETRO CORSI. University of California Press, Berkeley, 1988. xiv, 360 pp. $42. Translated from the Italian edition (Bologna, 1983) by Jonathan Mandelbaum.

If there ever was an "Age of Lamarck," it was the period 1890 to 1940, when, after neo-Darwinism had "outdarwined Darwin," to use the phrase of George John Romanes, those biologists who were not convinced by mutationism looked to neo-Lamarckism as the general theory of evolution. This is not the subject of this book. The title is not a misnomer, however, for the book stresses that Lamarck's endeavors make sense only when understood as embedded in the issues of and interwoven with the science of his own time. This may seem obvious, but it has not always been so. Indeed, as Corsi underlines, Lamarck himself, and Lamarckian historiography, built the myth of a romantic genius, isolated and ignored by his contemporaries. Even recent scholars have not always been immune to this fabrication. The purpose of this book is precisely to move beyond this myth, as the title of the original Italian edition, *Oltre il mito*, makes clear.

In congruence with this goal, Corsi's analysis—though he has written the most painstaking of internal exegeses—could not rest upon the assumption that Lamarck's theoretical choices and constructions can be accounted for simply as stages in his individual development. Among the major contributions of this book is a convincing elucidation of the positioning and the originality of Lamarck's approach in the midst of contending attempts to impose a working conception of what natural history is, the Buffonian tradition (kept alive up to 1815) and the variety of rival classificatory programs. Corsi also shows that Lamarck repeatedly encountered transformist hypotheses in the works of others, even before his first commitment to transformism in 1800. For his analysis of Lamarck's reactions to these innovations and their role in the transformation of his own views. Corsi seems to have virtually read all of French natural history books and papers published during the formative years of Lamarck's theorizing. In his reconstruction of the debates that followed Buffon's death, even specialists of the period are likely to come across names and works they have overlooked—not only J.-J. Virey, Bory de Saint-Vincent, J.-C. Delamétherie, and C. Prévost but also Sonnini de Manoncourt, P. Bertrand, F. Chaussier, Coquebert de Montbret, L. Cotte, F.-M. Daudin, C.-L. Dumas, F. Levaillant, D. Plan, J.-J. Sue, and many others.

This superb command of the literature does not lead Corsi to gratuitous erudition. It permits him to understand contextually the peculiarity and significance of Lamarck's own ambitious research program for a "terrestrial physics" encompassing the study of the atmosphere, of the changes of the surface of the earth, and of the organization of living beings. Through his broad synchronic reading of the literature and acute analysis Corsi brings an abundance of new data and interpretations bearing on all of the main historiographical issues, such as Lamarck's rejection of the occurrence of any extinction of species, his views on spontaneous generation, the relationship between his anti-Lavoisierian chemistry and his biology, or the place he occupied on the French scientific scene.

For instance, it had already been noted as a puzzling fact that, in the Inaugural lecture for the year 1800, his first presentation of a mechanism for the transformation of organized beings through the action of circumstances, Lamarck, whose own studies were focused on plants and invertebrates, used examples taken from the anatomy and behavior of birds. But it is from Corsi that for the first time we understand where these examples came from and why this is a far from trivial question: Indeed what we have here, at the birth of Lamarck's new theory, is a transformation operated upon statements that had just been made by the ornithologists François Levaillant and François-Marie Daudin. In his *Histoire naturelle des oiseaux d'Afrique*. Levaillant had stressed, against the Buffonian notion of some plasticity of species under the influence of food and climate, "that habits depend upon forms, and that nature modifies these when it wants to diversify the animal's habits"; Daudin in his *Traité élémentaire et complet d'ornithologie* had also emphasized that form controls the behavior of birds and the satisfaction of their needs. Corsi has detected that Lamarck in 1800 was making a "point-by-point reply" to such assertions. It is in this polemical mode, where he closely paraphrases these two ornithologists, that Lamarck, inversing the Levaillant sequence from forms to needs and habits, advances his own theory, according to which changes in circumstances will induce changes in habits and thence in forms. We may not yet fully understand the basis for that reverse transformation, but to have established its existence is a major breakthrough.

Other interpretations that Corsi puts forth are likely to generate some debate. A case in point is his contention that though Lamarck's *Recherches sur l'organisation des corps vivants* of 1802 seems to posit two distinct causes for the transformation of living be-