The discipline of history is only formally excluded from consideration for a Nobel Prize. Under the rubric of literature, a lawyer won the award for three volumes on “Roman History” at the beginning of the last century (and to the present reviewer that work is in fact best seen as fine writing in the style of its day).\(^1\) Near the end of the century another work of history received the prize, this time under the rubric of economics.\(^2\) This latter moment should perhaps be taken as the high water mark in modern historians’ long love affair with hard facts and quantifiability by which they aspired to raise their discipline to the level of a pure science. The tide turned. A clear sign was Daniel Kahneman’s prize in economics in 2002, recognizing his long, ingenious undermining of the rational-agent model through psychological studies as much as economic. This model, personified as \textit{homo economicus}, had long subserved a strictly rational understanding of market behavior. With Kahneman himself, its challenger, it was in the last few decades overtaken by a rising tide of findings in both psychology and neuroscience. They combined to change perceptions of the role of affect in decision-making. A consensus began to emerge from (let us say) 1980, that mental activity ordinarily taken as purely cognitive was or is in fact inseparably and intimately united with emo-

\(^{1}\) Theodor Mommsen was Laureate in 1902 for his \textit{Römische Geschichte} (Berlin, 1854–1856), 3v. It covered only the centuries before the common era; in my sixty years of reading in the field, I don’t recall ever seeing it cited.

tions. Thus in the new millennium something besides rational calculation and material benefit must be taken into account in historical explanation. In quantifiable terms, a sign of what has happened in this area of research appears in Google (April 2012): 155 million “results” for “affect in decision-making.”

At this juncture, and unfortunate in their timing, Ekelund and Tollison propose the forcing of history into the once-favored rational-agent template, as an aid to a better understanding. The title they have chosen for their work will likely mislead the ordinary reader, but its terms and their context are conveniently explained by Stark in his *Rise of Christianity*. As he declares, “a religious economy consists of all the religious activity going on in any society. Religious economies are like commercial economies,” with “markets,” “customers,” “firms,” “products,” and so forth.3 The same word-use is adopted by Ekelund and Tollison themselves and by others, too: for example, Laurence Iannaccone from the 1980s in working papers and subsequent publications, in which religion is treated as a commodity, a *bonum*, that can be chosen like shoes or soap or anything else offered to the public.4 All the vocabulary of analysis familiar to economists is applied to suitable aspects of the subject: behavioral changes are “costs” or “profits,” theology and liturgy constitute a “portfolio,” evangelization is “marketing,” and so forth.

One could describe many features of life in the same terms: for instance, gossip, as a *bonum* that people generally enjoy and seek to acquire by exchange, offered to them through instruments of marketing (Facebook, blogs, or any sort of cyberspace bulletin board) by local purveyors who can earn profits or suffer loss (in esteem or audience-size). The terminology of choice amounts to translation, applied in a novel way. In the circle of Ekelund and Tollison it has given rise in recent decades to a quite active social-scientific sub-specialty focused on religion, with its own dedicated journal, conferences, lots of publication, and leading authorities. Ekelund and Tollison brought its methods to bear in their first co-authored book treating the modern religious history of the West

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and then in a second, on religion in the Middle Ages. Here now is their third effort devoted to still earlier centuries of Christianity.

Notoriously, in translation much is lost. Why then resort to it as a tool of science? Ekelund and Tollison claim that “Economic theory has much to contribute to the understanding of religion and its primary products” (194). To test this proposition, in the first place, it is fair to ask if the two authors build on good data. Are the historical highlights and big names that they chose to translate rightly understood?

Since their subject is a story of conversion, at the start, demography is essential to show change across time. For this, Ekelund and Tollison rely on Stark’s Rise of Christianity and an article by Hopkins (47 and elsewhere). As Hopkins often and engagingly reminds his readers, he has no reliable figures for conversion to “play” with; even the most informed, those of the most admired scholar (Adolf Harnack’s), are “only a guess,” and Hopkins’ own method is throughout “unashamedly speculative.” But Stark in sharp comment on Hopkins protests that Hopkins essentially works with Stark’s own conjectures! Both attempts at demography are entirely theoretical—merely imagined. Ekelund and Tollison may be excused for not citing the only verifiable data published only a year or two prior to their own volume; but those data show Stark’s and other scholars’ guesses to be very greatly misleading.

To determine the accuracy of other matters translated by Ekelund and Tollison into the language of economics, I begin with the pagans that were the target of conversion. Ekelund and Tollison know, with bare mention, only the “Greeks,” “Romans,” and “Egyptians” (19), leaving out a majority of the Roman Empire’s peoples, and they suppose, incorrectly, that “moral rules” were a feature of paganism (80), as if Isiacism or Jupiter Dolichenus’ cult resembled Judaism. For Ekelund and Tollison, the most important characteristic of (all) paganism was that it was “complicated” as opposed to a far “simpler” Christian-

ity with “a common catechism” (49, 54, 59). But the contrast insisted on is not and cannot be supported in the face of known pagan cult practices as opposed to the turbulent confusion and conflicts in Christian theology of the time.

The essence of the Christian sales pitch and its superiority was the assurance of an afterlife, as Ekelund and Tollison often remind us (for example, 20, 34–41, 56, 84, 173, 177). They seem unaware that there is little or no mention of this benefit in conversion accounts or known prayers whereas pagan belief in survival after death is widely attested in almost all lands and in all periods of the Empire. They suppose also that during the crisis of the third and early fourth century Christianity offered “security” (48), without mention of the persecutions. They imagine a nonexistent “official” paganism (53), where in fact emperors like army commandants, provincial governors, or other great men patronized and publicized their personal favorites among the gods without imposing them. The notion of Christianity as a “state” religion for Constantine is quite mistaken, as is their notion of a “theocracy” at that time (39).

Ekelund and Tollison are wrong in other elementary matters, supposing that the composition of all the Gospels is “thought to have taken place” under Hadrian (95); that the scriptural canon was settled at Nicaea (65, 101, 173, 175); that the emperor had invited to Nicaea all bishops everywhere (98), who with their priests totaled some 1,800 in actual attendance; that “Pauline Christianity” was established at Nicaea (xi); that this same Paul had been Jesus’ disciple (38); and that “Constantine’s motives in establishing a quasi-homogeneous set of religious beliefs were fundamentally political as well as for economic aggrandizement” (99).

Ekelund and Tollison also believe, contrary to the evidence, that Constantine “established the organizational structure of the Holy See in Rome with papal primacy over the secular as well” (79), making this church a “cartel,” and the emperor thus “in large part ended” competition between sects in 325 (92). They quite incorrectly suppose that crime and the need for policing were diminished as a result of the imposition of Christian ethical rules (92, 106), since, to judge from enforcement legislation, the opposite was the case. To the same effect, and incorrectly, Ekelund and Tollison imagine that the need for policing agents and the size of government overall could be reduced as the numbers of Chris-
tians rose in the population (104, 119), though in fact in this same period the ranks of non-army government servants increased roughly a hundredfold. Two more of the authors’ assertions are untenable: “For church fathers, the ability to collect rents along with other motives (e.g., membership maximization) was a strong motive for cartelization” (86), and “the institution of Christianity” was “financed by taxing pagans, atheists, and followers of a non-orthodox Christianity” (99).

Toward the end of the account, covering the fifth and sixth centuries, Ekelund and Tollison explain how “cartelization” and “vertical integration” were achieved by the church, which was “oligopolist” before attaining a monopolist, Roman character. The period of change is described in a gallop of forty pages through five hundred years of church and state history. Restrictions of space prevent a run-down of the numerous errors and approximations.

Aside from data, and in the second place, the method applied by Ekelund and Tollison—“translation” as I term it—is underwritten by “rational choice theory,” the “efficient markets hypothesis,” and the notion of the perceived “utility” or bonum as the natural (and rational) object of human behavior. These three related ideas long reigned supreme among economists. Over recent decades in their publications, however, it has been the fashion to point out how remarkably irrational market behavior really is. Ekelund and Tollison thus find themselves casualties of a major shift in the governing assumptions within economics itself, as a consequence of the econometricians beginning to look a little beyond the boundaries of their own specialty.

This shift is perhaps overdue; for, in the third place, challenge to the rational-agent model has been widened by psychologists and neuroscientists to take in the whole idea of cognition as an activity separate and distinct from affect. Prominent in the research are Robert Zajonc, Antonio Damasio, and Jonathan Haidt, among a great many others. In the consensus that they have done much to

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establish, there is nothing unexpected in market ups and downs, nor astonishing even in panics. Man is not rational, at least not in the familiar Cartesian sense.

This fact is blandly accepted by Ekelund and Tollison when they conclude, “the principal good sold by Christian religious organizations is the assurance of eternal salvation in an afterlife. This good is a metacredence good whose qualities cannot be known [italics added]” (194). Here, however, protest must surely be heard from all those millions and millions across time who did indeed “know” that the assurances offered by faith could be relied on. Otherwise how to explain their behavior? Is the sensation of certainty in some given question, which we ourselves may feel, always and necessarily based on verifiable experiment? Are the standards and methods of experimental verification in our modern age the same as those that produced the same sensation of certainty in Joan of Arc’s day? Or Constantine’s? Such obviously rhetorical questions bring into play, just as obviously, other questions about the nature of cognition, of motivation, and of the powers of mind that observers should bring to bear on history in order to understand its flow. Ekelund and Tollison in limiting themselves to only a small part of our own modern-day minds in the hope of entering into the mind of the past seem to have chosen in religious history an area singularly ill-suited to their approach. As to the approach itself and the authors’ supporting data, these too invite the criticisms offered above.