BRINGING THEORY BACK IN

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RATIONAL CHOICE THEORY AND RELIGION

Summary and Assessment

Edited by

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tem for a social scientific audience.

In 1967, I began a three-volume work on religious behavior with Charles Glock. The first volume appeared in 1968. It was a largely conceptual and descriptive book called *American Piety: The Nature of Religious Commitment*. The scheduled second volume was to focus on sources of religious commitment, and the third was to be concerned with the consequences of religious commitment. Neither the second nor the third volume ever appeared. And theory was a major reason.[2]

When I wrote the second volume, I began it with a deductive theory of religious commitment that was not much different from one included in the essay, "Towards a Theory of Religion: Religious Commitment," that William Sims Bainbridge and I (1980) published more than a decade later. I began with a rational choice axiom: *Humans seek what they perceive to be rewards and avoid what they perceive to be costs.* Another axiom explicitly introduced human cognition: *Human action is directed by a complex information-processing system that functions to identify problems and attempt solutions to them.* I pause here to point out that, from the very beginning, my theoretical work has always included an explicit and significant cognitive component, something frequently overlooked by those who worry that I rely too much on exchange theory (cf. Garrett, 1990).

Another axiom imposed scarcity upon the concept of rewards: *Some desired rewards are limited in supply, including some that simply do not exist (in the physical world).* I also introduced the notion of *compensators*. I have never liked that word. It carries unmeant negative connotations, but I have not yet found a more suitable alternative. Compensators are a sort of substitute for desired rewards. That is, they provide an explanation about how the desired reward (or an equivalent alternative) actually can be obtained, but propose a method for attaining the reward that is rather elaborate and lengthy. Often the actual attainment will be in the distant future or even in another reality, and the truth of the explanation will be very difficult, if not impossible, to ascertain in advance. When a child asks for a bike and a parent proposes that the child keep his or her room clean for a year and get no grade below B during the same period, whereupon the bike will appear, a compensator has been issued in lieu of the desired reward. We can distinguish compensators from rewards because one is the thing wanted, and the other is a proposal about gaining the reward.

As reward-seeking beings, humans will always prefer the reward to the compensator, but they often will have no choice because some things we want can't be had in sufficient supply by some people and some rewards cannot be had, here and now, by anyone. Compensators abound in all areas of life, but my primary interest has been in religious compensators. Let me note only the most obvious example. Most people desire immortality. No one knows how to achieve that here and now—the Fountain of Youth remains elusive. But many religions offer instructions about how that reward can be achieved over the longer term. When one's behavior is guided by such a set of instructions one has accepted a compensator. One also is exhibiting religious commitment, since the instructions always entail certain requirements vis-à-vis the divine. Indeed, it usually is necessary to enter into a long-term exchange relationship with the divine and with divinely inspired institutions, in order to follow the instructions: churches rest upon these underlying exchange relationships.

I want it to be clear that the theory does not, and should not, imply anything about the truth or falsity of religious compensators. It merely postulates the process of rational choices by which humans value and exchange these compensators.

By logically manipulating the axioms and definitions of the theory, I was able to reach what I thought were some striking deductions concerning the relationship between power and piety. Noting that religious compensators typically include both scarce and unavailable rewards, we can see that:

1. *The power of an individual or group will be negatively associated with accepting religious compensators for rewards that are only scarce.* That is, powerful people will simply pursue the rewards—material luxuries, for example. Less powerful people will tend to accept compensators that, for example, assure them that by forgoing luxury now they are piling up riches in the life to come. We might call this the sectlike form of religious commitment.

2. *The power of an individual or group will be positively associated with control of religious organizations and with gaining the rewards available from religious organizations.* Here I deduced what could be called the churchlike form of religious commitment.

But it is the third deduction that has always interested me most:

3. *Regardless of power, persons and groups will tend to accept religious compensators for rewards that do not exist in this life.* Here I noted that in some regards everyone is deprived and everyone has a motive for being religious—that since everyone faces death, doctrines of an afterlife appeal to all. We could call this the universal form of religious commitment.

Finally, I introduced the concept of socialization to condition human perceptions and actions, including their religious commitment.

These three simple deductions, or propositions, seemed of great utility. First of all, they are very parsimonious. All human attributes related to variations in individual or group power, including all status attributes such as sex and race, are covered by these three propositions, with specific predictions and prohibitions being evident. Thus an immense literature of known correlations neatly fits beneath them. Second, by reference to the axioms, the propositions are *explained*. We know *why* things are this way.

With this deductive system as my guide, in the remainder of the book I tested these propositions with considerable success, using not only the large data bases Glock and I had collected, but also the pertinent empirical literature.

In 1969, when the book was done, I gave a copy to Glock, anticipating that I would get it back in a month or two with minor proof editing—that was the way we collaborated. But this time the months stretched on and on. Since I was busy writing a book on the police, I did not press him. Finally, he put a note in my box to the effect that as usual the book looked great, but it would not be wise to include the first chapter. He counselled me that theory, and especially deductive theory, seemed to put people off. Why didn't we just stick to testing well-known hypotheses about correlates of religious commitment such as "poor people tend to pray more than rich people" or "rich people are more likely to belong to and attend church than are poor people"? The problem with this approach is, of course,

that the hypotheses were intellectual orphans in that there was no higher order explanation of why these hypothesis should hold, or even why they should be formulated or tested. In fact, these "hypotheses" were derived from earlier empirical results (cf. Demerath, 1965) and therefore were in some sense *post hoc*. It seemed to me far more important to say why and how these differential patterns existed, for they turn up not just for income, but for other status-related variables such as IQ, education, and race. It seemed silly to continue to resort to dozens of *ad hoc* interpretations, one for each of these correlations, when a parsimonious explanatory model was at hand.

This second volume never appeared because I was not prepared to pull in my horns and Glock was not willing to stick out his neck. As it turned out, he was undoubtedly right about the wiser course over the short run. But, at that point in my career, I had published five books, not one of which was important. Enough of that. And, while I was at it, I decided I'd had enough of Berkeley, too. So I accepted an appointment at the University of Washington.

As soon as I had adjusted to my new role as professor, I spent a wonderfully rainy weekend in Seattle adding a summary of the pertinent empirical evidence to the end of the chapter containing the theory of commitment and submitted it as a paper to a major journal. It came back almost immediately by return mail with a lengthy explanation that deductive theories are inappropriate for the social sciences. When I pointed to Homan's presidential address, I was given to understand that presidential addresses are not subject to the review process. Just to make sure, I sent the essay off to the other major journal. This time, I was told that what was needed was empirical proof of the truth of the axioms before it would be appropriate to assess empirical data merely concerning the derived propositions. I despaired of bringing real theories into sociology. I put the essay on a shelf, along with the manuscript of the second volume, and on and off for the next several years I gave serious thought to leaving academia.

Fortunately, there were signs that some sociologists did know what theories were and were prepared to try to create them. In particular, Peter Blau and James S. Coleman were doing very impressive theoretical work, although neither seemed willing to try for the "big theory"—to take up where Homans had stopped and try to make social systems emerge from micro-axioms. Moreover, even these established stars seemed to be having hard going bringing theory back in. For example, in a major theoretical essay in the *American Sociological Review*, after a lengthy explanation of the logical structure of deductive theories and how one does not test the axioms, but rather tests the lower-order empirical predictions derived from the axioms, Blau (1970) then devoted the remainder of the paper to ignoring everything he had just written, as he tried to demonstrate that his axioms *could be induced* from empirical data. This led me to think that he had been ambushed by the same reviewers who had demanded that I induce my axioms, and that for some reason he had been willing to go along. It was as if David Hume had never lived. I also

noted that Blau and Coleman both did their best theoretical work in books, where reviewers could not meddle. From this I concluded that if I wanted to do theory, I should probably do books. I also concluded that I should avoid the hue and cry over Marxism, causal models, grounded theories, symbolic interaction, and all the rest by concentrating my theoretical efforts in a subfield where I might be able to exploit my skills at conceptualization and empirical research—and my early reputation—to force a forum for my theoretical work. And then, in 1975, William Sims Bainbridge came to Seattle.

When Bill arrived from Harvard as a brand new assistant professor, we hit it off immediately. Each of us has many interests that bore the other silly, but that has merely added to the fun (and breadth) of our collaboration. In the beginning, our discussions of religious behavior centered on the book he was completing about a new religious movement, in which he made considerable use of my work on religious conversion. With that book in press (*Satan's Power*, 1978), we began our first efforts at collaboration, and I soon gave him the essay containing my deductive theory of commitment. After careful study, he noted that much greater power and clarity could be gained by adding an additional axiom, which became axiom A1 in all of our subsequent work: *Human perception and action take place through time, from the past into the future.* He then defined the *past* as the universe of conditions which can be known but not influenced, and the *future* as the universe of conditions which can be influenced but not known. What could be more obvious? Yet, by explicitly placing human behavior in time, many of our subsequent efforts to explain religious phenomena were clarified and simplified. Thus did I discover Bill's gifts for abstract thought, and I knew I had found someone crazy enough to agree to collaborate on a full-length deductive theory of religion.

It has struck many as odd that we would collaborate on an activity so seemingly singular as deductive theorizing. But there are immense virtues in doing such work as a collaboration. Let me explain. When one is not able to formalize a theory, that is, express it in symbolic or mathematical form, the deductive process is fraught with risks, and it grows more difficult as the deductive chains lengthen. It is very easy to think one has deduced a set of propositions when, in fact, one has simply jumped to them without having reached them in an unbroken logical chain. Such gaps usually are relatively easy to spot when the chains are symbolic, but it is very difficult to spot them when the chains are in plain English. To combat this problem, we divided the labor. I did the first drafts. Then Bill worked through each step independently, seeing if he could retrace my path without hitting a gap in the chain. When he found what he thought was a gap, we would get together and go over the logical steps until they were closed—although he would often have provided the closure before we consulted. In addition, Bill often found implications I had entirely overlooked and he extended many branches from the main line to produce important propositions on a number of vital issues. In the end, neither of us knows for sure who did what.

But we do know that our colleagues were little interested in so-called "grand theories" about anything. Early on, we submitted the axiomatic theory of religious commitment to the *Journal of Scientific Study of Religion*. In Phillip Hammond we found a receptive editor, and no significant revisions were required.[3] That, however, does not alter the fact that this is the only paper Bill and I ever published for which we did not receive a single request for a reprint. In any event, by the time this first fragment of the theory was published in June 1980, Bill and I had a relatively complete draft of the whole theory. We submitted a copy to Grant Barnes at the University of California Press, a friend who had previously published books by each of us. The reviews he received were rather surprising. John Lofland, as true-blue a symbolic interactionist as has ever lived, wrote an astonishingly favorable review. His enthusiasm was not shared by two anonymous "theorists," one of whom condemned the whole approach on the grounds that it was well-known that theories aping the physical sciences could not deal with the ambiguities and nuances of social science. I believe Grant Barnes would have published the book anyway, but at that point Bill and I decided to adopt a Fabian strategy. We would delay sending our theory into a hostile world, meanwhile teasing our opponents and tempting possible supporters by revealing fragments of the theory in essays constituting empirical tests of some of its more significant propositions. Through these means we hoped to create a climate of opinion willing to give theory a chance.

From 1980 through 1983, Bainbridge and I published 19 co-authored journal articles, in addition to other articles that each of us wrote alone. And in time, our advertisements of coming theoretical attractions did begin to create interest among sociologists of religion. Indeed, some people began to write attacks on our theory despite never having read it (cf. Wallis and Bruce, 1984). So, in 1984 we turned 22 of these essays into a book which Grant Barnes arranged to publish the next year. *The Future of Religion* was well received—it even won the Society for the Scientific Study of Religion distinguished book award. So at last it seemed time to publish our theory of religion. We returned to the manuscript, which had been in a drawer for about five years, and gave it a final going through. We both were rusty and it was very hard going. Worse yet, since 1982 Bill had been back at Harvard which made our collaboration rather more cumbersome. But eventually the book was done.

A Theory of Religion appeared in 1987. It consists of seven axioms, each of which is a very simple statement about humans or the human condition. In addition to those reported earlier in this essay, others are such uncontroversial claims as:

Rewards vary in kind, value, and generality.

Most rewards sought by humans are destroyed when they are used.

It would be difficult to think of more obvious statements. But, when combined with the 104 concepts defined in our theoretical system, we managed to derive 344 contingent propositions, including the three derived for the theory of commitment. As a necessary preliminary to explaining religion, some of these propositions account for the emergence of norms and values; others explain the division of labor (or specialization); and others deduce stratification, cultural evolution and a whole host of primary social phenomena.

A major source of satisfaction was our ability to deduce within our axiomatic system propositions embodying the social scientific study of religion's most respected theories of the middle range.

Consider Malinowski's celebrated proposition that "primitives" never resort to magic when they possess means for achieving their goals directly. For example, they never resort to magic to remove weeds from their fields or to fix their fences. But there are forces, such as bad weather or plant blight, that thwart their best efforts and exceed their knowledge. "To control these influences and only these," the primitive agriculturist "employs magic" (Malinowski, [1925] 1964: 29). In our deductive system, Malinowski's proposition is generalized beyond magic and beyond primitive cultures and takes the form of Proposition 58: *People will not exchange with the gods when a cheaper or more efficient alternative is known and available.*

Another example is Durkheim's (1915: 44) famous claim that "*There is no church of magic.*" Since I regard this as by far the most original and important insight in Durkheim's otherwise quite overvalued work on religion, let me trace his argument. He began by distinguishing between religion and magic. While both are made up of beliefs and rites, myths and dogmas, magic differs by stressing technical and utilitarian ends and fails to address basic theological concerns: "[magic] does not waste its time in pure speculation" (p.42). Moreover, Durkheim noted the "marked repugnance of religion for magic, and in return, the hostility of the second for the first. Magic takes a sort of professional pleasure in profaning holy things...On its side, religion, when it has not condemned and prohibited magic rites, has always looked upon them with disfavor" (p. 43). Finally, Durkheim noted that "Between the magician and the individuals who consult him, as between these individuals themselves, there are no lasting bonds...The magician has a clientele and not a church, and it is very possible that his clients have no other relations between each other, or even do not know each other; even the relations which they have with him are generally accidental and transient; they are just like those of a sick man with his physician" (p. 44).

Here is how these insights appeared in our theory. First were some key definitions:

> Definition 18: *Compensators* are postulations of reward according to explanations that are not readily susceptible to unambiguous evaluation.
>
> Definition 19: Compensators which substitute for single, specific rewards are called *specific compensators.*
>
> Definition 20: Compensators which substitute for a cluster of many rewards and for rewards of great scope and value are called *general compensators.*
>
> Definition 22: *Religion* refers to systems of general compensators based on supernatural assumptions.

Let me note here that among the rewards of greatest scope are explanations of the human condition: Does life have purpose? Why are we here? What can we hope? Is death the end? Why do we suffer? Does justice exist? How did the universe come into being? Moreover, answers to such questions constitute what are often called theologies.

> Definition 52: *Magic* refers to specific compensators that promise to provide desired rewards without regard for evidence concerning the designated means.
>
> Definition 53: Cultural specialists whose main activity is providing specific compensators are *magicians.*

These definitions are consistent with Durkheim. Now see how the rest of Durkheim's assertions about religion and magic fall out as deductions within our system:

> Proposition 91: *Magic is more vulnerable than religion to disconfirmation.*
>
> Proposition 92: *It is not in the interest of religious specialists to risk disconfirmation of the compensators they supply.*
>
> Proposition 93: *Religious specialists will, over time, tend to reduce the amount of magic they supply.*
>
> Proposition 94: *To the extent that the demand for magic continues after religious specialists have ceased providing it, others will specialize in providing it.*
>
> Proposition 95: *The roles of religious specialist and magician will tend to be differentiated, as will religious and magical culture generally.*
>
> Proposition 96: *Magicians cannot require others to engage in long-term, stable patterns of exchange* (an earlier proposition asserted that religious specialists *can* require such patterns of exchange).

Proposition 97: *In the absence of long-term, stable patterns of exchange, an organization composed of magicians and a committed laity cannot be sustained.*

Proposition 98: *Magicians will serve individual clients, not lead an organization.*

Proposition 99: *Magicians are much less powerful than religious specialists.*

We then proceeded to explain Durkheim's perception of antagonism between religion and magic and also to show why this antagonism is often very minor. If, for whatever reason, the prevailing religious institutions in a society continue to offer their own brand of magic (as was the case with the medieval church) we deduced in Proposition 104: *...religion...will tend to oppose magic outside its system.* This explains witchcraft trials. But, in Proposition 105, we deduced that when religious institutions do not deal in magic, they will tolerate magic outside their system. This is the pattern found throughout most of the East where priests and magicians live in symbiotic balance.

In similar fashion we deduced the evolution of the gods. Many scholars have noted the tendency for religions to evolve in the direction of monotheism (Swanson, 1960; Bellah, 1964; Lenski, 1970). We thus deduced:

Proposition 61: *As societies become older, larger, and more cosmopolitan they will worship fewer gods of greater scope.*

Here, however, is an instance when the logical processes of deduction produced novelty. For we discovered that, given our axioms and definitions, the end product of this evolution is not *monotheism*, defined as belief in only one god (supernatural being) of infinite scope. Viewed within our system, such a god would necessarily be conceived of either as almost wholly remote from human concerns and affairs (as exemplified by Unitarianism and the versions of Buddhism sustained by Chinese court philosophers) or as dangerously capricious in the manner of the Greek pantheon. Here the issue is rationality, not only on the part of believers, but on the part of the gods. And we deduced that evil supernatural forces (such as Satan) are essential to the most rational conception of divinity.

Proposition 107: *Explanations that assume the gods are rational offer greater certainty of reward than explanations that assume the gods are irrational.*

Definition 54: *Rationality* is marked by consistent, goal-oriented activity.

Proposition 108: *Distinguishing the supernatural into two classes—good and evil—offers a rational portrait of the gods.*

Definition 55: *Good* and *evil* refer to the intentions of the gods in their exchanges with humans. *Good* consists of the intention to allow humans to profit from exchanges. *Evil* consists of the intention to inflict coercive exchanges or deceptions upon humans, leading to losses for the humans.

Thus we deduced the necessity either to conceive of a single god who is above the question of good or evil by virtue of being remote from any exchanges with humans (the Tao is not a fit exchange partner), or to admit the existence of more than one supernatural being. Thus we see that good and evil reflect the possible goal-orientations of the gods—to give more than they take, or to take more than they give. A god holding either of these intentions is more rational than a god who holds both intentions.

We therefore deduced:

Proposition 109: *The more complex the culture, the clearer the distinction drawn between good and evil gods.*

Proposition 110: *The older, larger, and more cosmopolitan societies become, the clearer the distinction drawn between good and evil gods.*

Proposition 111: *Humans seek to exchange with good gods, and to avoid exchanging with evil gods.*

Proposition 112: *Good gods will be preferred who are thought to protect humans from exchanges with evil gods.*

Proposition 113: *The more complex the culture, the more likely is belief in good gods that are more powerful than evil gods.*

Proposition 114: *The older, larger, and more cosmopolitan societies become, the more likely they are to believe in good gods that are more powerful than evil gods.*

In other propositions, we were able to deduce a fully-articulated control theory of deviance as our mechanism for explaining why and how people create, convert to, and defect from religious movements. We also produced propositions to account for the birth of sects and the conditions under which sects evolve into churches, for the occurrence of religious revivals and for dozens of other things about which much was already known to our fellow scholars. What we tried to do was not so much discover new knowledge, but to carefully codify the wealth of what already was known and unite it under one theoretical system in order to discover the connections among all of these insights.

It may be appropriate here to mention briefly the implications of theorizing about religion for the plausibility of religious doctrines. It would be entirely wrong to claim that by offering rational explanations of why religions will, for example, tend to conceive of the gods in rational terms, the truth of religious doctrines is called into question—that religious phenomena are reduced thereby to naturalistic explanations. On the contrary, if the supernatural is as described by traditional Jewish–Christian–Moslem theology, then we live in a reality in which our theory about the nature of the gods *ought* to hold. That is, the God of Abraham is presented as the merciful and forgiving creator of an orderly, lawful universe. The idea that social science can comprehend religion *because* God is rational is entirely parallel with the notion that the laws of physics are susceptible to reason and discovery, because, as Einstein reminded the world, "God does not play at dice." Moreover, our deductions about the need for a separation of good and evil are entirely consistent with millennia of theological thought.

I recognize that many omissions and shortcomings mark our first effort at a theory of religion, but I am proud that we finally got serious theoretical activity going again in the sociology of religion. Suddenly, there are a number of other people, especially young scholars, doing creative and original work on theories of religion and many more who are testing and refining pieces of these theories. For me, the most satisfying part of these developments has been the privilege to encourage, learn from, and often collaborate with these younger scholars.

One of these scholars is, of course, Laurence Iannaccone. Just before Christmas in 1985 I received a letter from an assistant professor of economics at Santa Clara University. He explained that the enclosed essay, which consisted of a formal model of church and sect, had been on his shelf since the summer of 1980 because neither his dissertation advisor nor "anyone else in Chicago's economics department deemed the subject worthy of an economist's attention." He asked what I thought of his model and where I thought he might send it. I wrote him immediate encouragement because his paper was wonderful—not the least of its virtues being that it was entirely compatible with the work Bainbridge and I were about to publish. My judgment of the paper was soon ratified by others, and it appeared in a special issue of the *American Journal of Sociology* devoted to economic sociology.

Since then, Larry Iannacconne has become a well-known figure in the social scientific study of religion, and his applications of rational choice theory as developed in micro-economics have been major contributions to very basic issues, such as why strict churches are strong. Moreover, Larry's work has already closed very serious theoretical gaps at the micro level that were left by Bainbridge and myself. The truth is that, although from the very first I have constructed deductive theories from what primarily are micro-axioms and have derived macro sociology from micro origins, I always have been more interested in the macro level of analysis.

Hence, beginning in 1985, with the theory book drafted and launched on its nearly interminable journey into print, I began to extend and refine the theory at

the most macro level of analysis. Things rapidly began to come together when I struck upon the notion of religious economies. A *religious economy* consists of all the religious activity going on in any society. Religious economies are like commercial economies in that they consist of a market of current and potential customers, a set of firms seeking to serve that market, and the religious "product lines" offered by the various firms (Stark, 1985). The use of market language to discuss things often thought to be sacred was not, and is not, meant to offend, but to enable me to import some basic insights from economics to help explain religious phenomena.

Among the many innovations made possible by this approach is the capacity to focus on the behavior of religious *firms* rather than only upon religious *consumers*. Let me give an example of what this shift in focus offers. Past discussions of secularization usually postulate a decline in the *demand* for religion, claiming that potential consumers in a modern, enlightened age no longer find a need for faith in the supernatural. In contrast, in new essays written with Larry Iannaccone (1993; 1994), we focus not so much on religious consumers as on religious *suppliers*. We ask, under what conditions are religious firms able to *create a demand* Or, what happens when only a few, lazy religious firms confront the potential religious consumer? More concretely, does the low level of religious mobilization in Scandinavia, for instance, primarily reflect weak demand, or an unattractive product, badly marketed, within a highly regulated and distorted religious economy?

As I pondered the workings of religious economies I soon recognized that the most decisive factor involved is whether they are free markets or whether the government regulates the economy in the direction of monopoly. Bainbridge and I already had deduced that a religious organization would be motivated to seek monopoly standing and that often the state finds that its interests are best served by supporting a religious monopoly. Starting anew, I extended the theory thus:

> Proposition 1: *The capacity of a single religious firm to monopolize a religious economy depends upon the degree to which the state uses coercive force to regulate the religious economy.*

> Proposition 2: *To the degree that a religious economy is unregulated, it will tend to be very pluralistic.*

Pluralism refers to the number of firms active in the economy: the more firms having a significant market-share, the greater the degree of pluralism.

I shall not deal here with why pluralism must arise in free markets. Rather I shall focus on my realization that competitive pluralism is not the evil force that saps the vigor from religion. To the contrary, where there is greater pluralism and competition, religious organizations are stronger, and the overall level of religious participation is higher (Stark, 1985). This led me to formulate the next two propositions:

Proposition 3: *To the degree that a religious economy is pluralistic, firms will specialize.*

To *specialize*, a firm caters to the special needs and tastes of specific market segments.

Proposition 4: *To the degree that a religious economy is competitive and pluralistic, overall levels of religious participation will tend to be high. Conversely, to the degree that a religious economy is monopolized by one or two state-supported firms, overall levels of participation will tend to be low.*

Economists take it for granted that a set of specialized firms will, together, be able to appeal to a far greater proportion of consumers than can a solitary unspecialized firm. The same principle applies to religion. Moreover, because so much of the religious product necessarily is intangible and concerns the far distant future, vigorous marketing activity is needed to achieve high levels of consumption. But that is not how state-supported monopoly firms function. It is a major proposition of economics that such firms tend to be inefficient. Writing in 1776 about established religions in general and the Church of England in particular, Adam Smith noted their lack of "exertion" and "zeal":

> [T]he clergy, reposing themselves upon their benefices, had neglected to keep up the fervour of faith and devotion of the great body of the people; and having given themselves up to indolence, were incapable of making vigorous exertion in defence even of their own establishment. ([1776] 1937: 741)

Having begun to use economic language and to apply basic economic principles, my acquaintance with Larry allowed me to discuss these things with a trained economist. Interestingly enough, this economist was more interested in micro issues that might have been of greater interest to sociologists and psychologists. During the past several years Larry and I have collaborated, and often our papers move from micro to macro theorizing. I have contributed an occasional point and examples to the micro portions of these essays and Larry has done the same to the macro portions.

In any event, the extension of the macro level of theorizing about religious economies has yielded many quite controversial results. For example, the theory forces the conclusion that the so-called secularization thesis is simply wrong—that levels of religious mobilization vary in response to pluralism, not to the spread of modernity and scientific sophistication. The deduction that religious mobilization must be low when a religious economy is essentially monopolized required us to examine history and to discover that the received wisdom about the universal piety

of medieval Europe is mythical and that the medieval masses were scarcely religious at all. The theory even predicts the churching of Europe, should the religious economies of those nations be effectively deregulated.

These new theoretical developments at the macro level have also been important in my collaboration with another of the gifted young scholars who have recently taken up the social scientific study of religion—Roger Finke.

I have been blessed with some good graduate students, but for many years I never had one of appreciable talent who had much interest in religion. Mostly, I have trained criminologists, and in the beginning Roger worked with me on criminological topics. In 1983, for instance, he was one of my co-authors on an essay called "Crime and Delinquency in the Roaring Twenties." But Roger soon decided he wanted to specialize in the sociology of religion. Instantly, whatever have been my failings in terms of the quantity of sociologists of religion I have trained, I became beyond reproach in terms of their quality.

My collaboration with Roger was not initially focused on theory. Together we had begun to explore the wonderful and neglected religious census studies. When it turned out to be possible to construct equations to predict church membership in 1850, 1860, and 1870 from data on the seating capacity and finances of each congregation, Roger realized he could do his dissertation on the churching of America. *Churching* is the appropriate verb because in 1850 only a third of Americans actually belonged to a church, while by 1980 almost two-thirds belonged. In addition to describing these changes and comparing the relative fate of various religious denominations during the process, Roger made the first major effort to test the deduction that pluralism invigorates religious "firms" and thus results in higher overall levels of religious participation. His findings were very strongly positive: as pluralism grew, levels of American church membership grew accordingly. With his dissertation completed, Roger left Washington and our future collaboration depended upon long distance communication. But, continue we did and over the next several years, as we worked together, what had once seemed a carefully delimited dissertation project sprawled into a major undertaking in which we used quantification and theory to challenge many chapters in the standard narrative of American religious history. In the end, we both had to read a lot of musty books and dig in a lot of diaries and documents to extend our grasp of American religious history in order to be careful to get things right. So, *The Churching of America, 1776–1990: Winners and Losers in Our Religious Economy* did not appear until 1992. In the meantime, Roger, Larry and I have been involved in a number of three-way and two-way collaborations.

In their essays, Larry Iannaccone and Roger Finke summarize their own work in far greater detail. Moreover, they mention others who have, and are, contributing to the task of bringing theory back in. As for me, I am continuing to analyze religious economies. I hope soon to offer a theory of religious conflict and civility in which I try to explain why and when religious economies will be torn by

religious strife and to discover the basis for peaceful civility among distinctly different religions. I am also building a theory of the dynamics and stability in religious economies by close examination of the demand side. My aim is to identify the natural and relatively stable set of religious market niches that exist in societies—that is, segments of potential adherents sharing particular religious needs, tastes and expectations.

Meanwhile, I have used portions of the theory to sustain me in another foray into history. This time, I have tried to reconstruct the rise of Christianity. The project has served as a cherished hobby for about eight years and I have published parts of it as I have gone along. As a result, I have come to know a number of historians of the early church and of Greco-Roman times. What wonderful, dedicated scholars they are, and generous with both praise and help. I finished the book last month. I probably could have been done several years earlier, but I was reluctant to finish because now I will lack an excuse to continue reading the marvelous work being produced by scholars in this area. Lest you think I am referring primarily to works of textual analysis, I offer a recent essay by Roger Bagnall of Columbia who used a Gini index to calculate inequality in landholding in Roman Egypt, based on surviving tax records from the year 350. Bagnall is a classicist who specializes in ancient papyri. But his statistical sophistication is equal to anyone in sociology, and his respect for hypothesis testing (as opposed to raw empiricism) exceeds most. Indeed, Bagnall's results shed light on a major historical question: why did so few people from Egypt rise to the Roman aristocracy, as compared with people from other parts of the empire? In Bagnall's words: "Perhaps the most striking feature of all, in fact, is the absence of really great landed fortunes in the hands of the curial class, fortunes that might support a rise from municipal status to the aristocracy of the empire" (1992: 143).

These historians of the early church do not need me to teach them how to analyze quantitative data. So then, what was my role? To introduce them to real social scientific theories. And my favorable reception among them is the result of their ability to see that when concrete events can be cast as instances of a general class of phenomena, governed by general axioms, that is far more satisfactory than *ad hoc* explanations. Now, if we can just convince sociologists that science is a theory-driven enterprise.

Finally, given the focus of this conference, it should be asked, if sociology is to be theory-driven, must these be rational choice theories? That depends upon what it means to call something a rational choice theory. If it merely means that all efficient social theories will include an axiom postulating that humans seek to maximize, which is to say, humans will attempt to act rationally, then undoubtedly the future of sociological theory rests on a rational choice approach (Coleman, 1990). But if we place greater limits than that on what we will call Rational Choice Theories (as is implied when the words are capitalized), then the future of social

theory may be far broader. To conclude this essay, I would like to explore this matter in greater depth.

For far too many sociologists, theoretical "work" is a form of ancestor worship. That is, theory is believed to consist of the opinions, prejudices, insights, analyses, and metaphors about social life contained in the works of dead founders, especially Marx, Durkheim, and Weber. These collections of thought are often referred to as "perspectives" and most books and articles identified as "theory" or "theoretical" involve efforts to explicate or to compare these perspectives. Most sociologists experience some pressure to identify with a particular perspective. Unfortunately, once sociologists are identified with any given perspective they have, in a very important sense, been deactivated. That is, if a scholar is known to be a Weberian or a Marxist, he or she easily is placed within an array of distinctive perspectives and the bases for disputes among these perspectives are well-known and regarded as *beyond resolution*. Since, as Popper noted, perspectives do not give rise to contingent (falsifiable) predictions, they can never be disconfirmed and hence they endure in splendid, if hermetic, majesty. That is, each perspective is assumed to have a sufficient claim to validity so that all can and must exist in endless, sterile disputation. That being the case, no serious progress is possible beyond mere fiddling with details of a given perspective. Moreover, the fate of perspectives is governed not by the results of research, but by fashion, taste, esthetics, or effective moral exhortation.

We must be very careful not to let our efforts to bring real theories into sociology be compromised by being labelled as the Rational Choice "approach" or "perspective." For then our work becomes just another one of the field's "theoretical" sects. But, as Voltaire pointed out, "There are no sects in geometry." And there are no sects in real theory, either.

To test this assertion, examine any university catalogue. You will find no course on the thought of Newton or Einstein in any physics department. Nor will you find a course on the thought of Copernicus in an astronomy department. And no courses on the thought of Darwin appear in biology department listings. Truly theoretical fields remember their ancestors only in ceremonial ways—no matter how illustrious their achievements—because real theories continue to evolve and, therefore, ancestors are always out-of-date.

My goal is to bring real theories into sociology, not to found a new theoretical sect. So, rather than suggest that rational choice theories are the future of sociology, I would suggest instead that for the future of sociology, theory is the only rational choice!

NOTES

[1] He also claimed that when he was in a hurry he didn't read a paper, but just looked at the tables, since that's really all of interest that the author could report.

[2] For other reasons see my ASR presidential address (1984).

[3] Phil did ask that we cut the essay from the 32 pages we submitted to his new, non-negotiable limit of 25 pages. But in the days before people were familiar with the power of word-processing (the machines available in those pre-micro computer days cost $12,000 or more and required extensive operator training), we solved this problem by having the font slightly reduced in size and slightly decreasing the margins.

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Summary and Assessment

Edited by

Lawrence A. Young

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