Annu. Rev. Sociol. 2001. 27:261–81

RELIGIOUS PLURALISM AND RELIGIOUS PARTICIPATION

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Key Words sociology of religion, rational choice, religious diversity, organizational competition, secularization

■ **Abstract** For more than a decade, sociologists of religion have been debating the answer to a basic question: What is the relationship between religious pluralism and religious vitality? The old wisdom was that the relationship was negative, that pluralism undermines vitality. This view has been challenged by advocates of a supply-side model of religious vitality. They argue that the relationship is positive—that pluralism increases vitality—and this empirical claim has become foundational to the larger project of applying economic theory to religion. We review the relevant evidence and reach a straightforward conclusion: The empirical evidence does not support the claim that religious pluralism is positively associated with religious participation in any general sense. We discuss this conclusion's theoretical implications, and we identify potentially productive directions for future research on religious pluralism, church-state relations, and religious competition.

> It appears that North Americans are religious in spite of, not because of, religious pluralism. (Olson 1998a:761).

> [R]eligious practice is strongly and positively associated with pluralism. (Finke & Stark 1998:762)

INTRODUCTION

These two diametrically opposed statements indicate the conflicted state of the literature on a basic question in the sociology of religion: What is the relationship between religious pluralism and religious participation? For years sociologists of religion agreed that the relationship was negative—that pluralism undermined participation. The best-known version of this theory was advanced by Peter Berger (1969). He argued that religious pluralism reduces religious vitality through its effect on plausibility: The more worldviews there are, the less plausible each seems, and the less religious belief and activity there will be. Over the last decade this wisdom has been challenged by advocates of a "religious economies" or a "supply-side" model of religious activity. Led by Roger Finke and Rodney Stark, the challengers have argued that the traditional view is backwards—that religious pluralism is *positively* associated with religious participation. For them, the key mechanism is not plausibility but competition. Starting from the assumption that "religious economies are like commercial economies," they argue that competition among religious groups increases the quantity and quality of religious products available to consumers and, consequently, the total amount of religion that is consumed (Finke & Stark 1988, p. 42).

This argument has drawn scholarly attention to the important phenomenon of competition among religious groups and to the possibility that such competition is a source of religious vitality. Although religious pluralism is not identical with religious competition, pluralism has commonly been treated as an indicator of competition, and analyses of the relationship between religious pluralism and religious participation have been the primary source of evidence in favor of the idea that religious competition leads to increased religious vitality. Furthermore, the empirical claim that pluralism and participation are positively associated has come to be considered the central discovery of a larger market model or rational choice approach to religion (Hechter & Kanazawa 1997, p. 198).

In this article we review existing evidence on the relationship between religious pluralism and religious participation. We begin with a comprehensive review of the large-N studies and a discussion of some of the methodological issues they raise. We then consider some of the relevant historical and comparative evidence and certain empirical and theoretical issues it raises. Our conclusion is simple: The empirical evidence does not support the claim that religious pluralism is positively associated with religious participation in any general sense. There may be times and places where increased religious pluralism produces increased levels of overall religious participation. But as Stark et al (1995, p. 436, emphasis in original) remind us, "the theory [including the proposition that pluralism increases overall levels of religious mobilization] is not about today, nor is it about the United States—it purports to be *general*." This aspiration to generality is not sustained by a comprehensive and dispassionate review of the empirical evidence. In the conclusion we discuss several theoretical implications of this fact, and we identify potentially productive directions for future research on religious pluralism, church-state relations, and religious competition.

THE LARGE-N QUANTITATIVE EVIDENCE

Our focus is on the empirical relationship between religious pluralism and religious participation, and we begin by specifying the boundaries of the relevant research. Several studies cited in the literature on pluralism and participation speak only indirectly, if at all, to the issue at hand. These include (*a*) two studies that use the number of congregations as the key independent variable (Finke & Stark 1992, p. 215; Welch 1993, p. 333); (*b*) one that uses religious switching as the dependent variable (Duke et al 1993); and (*c*) a set of studies that examine the relationship between a religious group's relative size—its market share—and involvement in that particular religion (Stark & McCann 1993, Zaleski & Zech 1995, Phillips 1998, Perl & Olson 2000). But the number of congregations does not indicate religious pluralism; religious switching is not a measure of religious participation levels; and a minority-religion effect should not be confused with a pluralism effect. Very few extant studies—three in all—use either the number of congregations as the independent variable or religious switching as the dependent variable. Additional comment is warranted, however, about the relevance of studies focused on a minority-religion effect.

There is evidence that religious groups evoke more commitment from their people when they are a smaller proportion of the population (Stark & McCann 1993, Zaleski & Zech 1995, Perl & Olson 2000, but cf. Phillips 1998). Although there may be conceptualizations of religious competition under which this subliterature would be relevant to an assessment of a broader relationship between religious *competition* and religious participation, the fact that a religion is a minority group in a particular region says little about how much religious *diversity* that area contains. A religion that has 10% of the population, for example, might be one of 10 religions, each with 10% of the population, or one of two religions, one of which has 90% of the population. Minority religions may or may not exist in highly pluralistic settings. We will return in the conclusion to the implications of our review of research on religious pluralism for more general claims about the consequences of religious competition. For now, the important point is that the subliterature on market share effects is not relevant to debate about the relationship between religious pluralism and religious participation because the market share held by a particular religion in a given area is not equivalent to the extent to which that area is religiously pluralistic.

Because studies of these sorts do not speak directly to the issue at hand—the empirical relationship between religious pluralism and religious participation—we limit our focus to those studies that use measures of religious pluralism and religious participation as their independent and dependent variables. We find 26 published articles or chapters of this sort, and we believe these 26 constitute the entire universe, at this writing, of published articles and chapters containing quantitative analyses of the pluralism-participation relationship. Ten of these articles find mainly a positive relationship (Finke & Stark 1988, 1989, Iannaccone 1991, Finke 1992, Stark et al 1995, Finke et al 1996, Hamberg & Pettersson 1994, 1997, Johnson 1995, Pettersson & Hamberg 1997). Eleven find mainly a negative relationship (Breault 1989a,b, Land et al 1991, Blau et al 1992, Bruce 1992, Blau et al 1993, Blau et al 1997, Hull & Bold 1998, Phillips 1998, Olson 1999, Olson & Hadaway 1999). Five find mainly null effects (Christiano 1987, Chaves & Cann 1992, Zaleski & Zech 1995, Verweij et al 1997, Perl & Olson 2000).

More informative than counting articles, however, is an examination of the individual analyses *within* these articles and chapters. We found 193 analyses reporting bivariate or partial correlations between religious pluralism and religious participation. We entered these analyses into a database (available from the first author upon request), noting for each the unit of analysis, the year and location of the data, and the dependent variable. For reasons that become apparent below, we also noted certain features of model specification and data analysis, especially whether or not the analysis includes a control for any specific religious group (or groups) constituting a large proportion of the population in at least some of the units. Most of the relevant analyses are regressions of some measure of religious participation on some measure of religious pluralism and selected control variables. We count each result presented in a table, ignoring for the sake of clarity the very few additional analyses mentioned in the text but not reported in a table. We do not double count identical analyses reported in two different articles. Our finding: Sixty-nine percent (133) of the published analyses report either a significant negative (86) or a null (47) relationship between religious pluralism and religious participation, while only 31% (60) report a significant positive relationship. The null findings were about evenly split between nonsignificant positive (26) and nonsignificant negative (21) coefficients.

These 193 analyses are not, of course, independent of each other. Many use the same datasets, and many are offered by the same authors. We know of no reasonable way to adjust our summary percentages to take this nonindependence into account, but we also see no reason that the links among these analyses created by common datasets or authors would favor one side or the other in the relevant debates. Scholars using the same datasets have reached opposite conclusions about the relationship between pluralism and participation. Furthermore, as we will see, the key issues involve model specification rather than any particular feature of the data, such as level of analysis or year in which the data were collected.

The relatively weak prima facie support for the supply-side prediction—only 31% of analyses find the expected positive relationship between pluralism and participation—is weaker than it initially appears. Of the 60 analyses reporting a significant positive relationship, only 23 use overall religious participation as the dependent variable. The other 37 analyses are of two sorts. Thirty-four of them use as their dependent variable participation in some specific religious group, examining, for example, only Baptist and Presbyterian growth (Johnson 1995), or finding that Methodists have increased in pluralistic settings in the United States but Catholics have not (Blau et al 1997). Studies of this sort do not address the general thesis about positive effects of pluralism, which predicts that increased pluralism will raise the *overall* market penetration of religion and thus raise *overall* levels of religious participation in an area. These studies suggest that some religious groups might do better than others in pluralistic situations, but this finding is not inconsistent with the old wisdom regarding religious pluralism. If in pluralistic settings small groups thrive and large groups decline, then the relationship between pluralism and overall participation could still, on balance, be negative.

Another three analyses obtaining positive results are from a cross-national analysis of religious participation, and the positive relationship between pluralism and participation holds only for a subset of units: It is present for predominantly

Protestant countries, but is null for predominantly Catholic countries (Iannaccone 1991). Moreover, Chaves & Cann (1992) use these same data to show that, once variation in the institutional arrangements between church and state are taken into account, the relationship between pluralism and participation is null even for Protestant countries. Other cross-national research also finds no general relationship between religious pluralism and religious participation (Verweij et al 1997).

If we discount these 37 analyses, only 23 published analyses (12%) yield results that appear to support the religious-economies claim regarding the positive effects of religious pluralism. Of the 133 negative or null analyses, 40 use participation in some specific subgroup as the dependent variable. If we ignore these analyses as well and look exclusively at the 116 analyses using overall religious participation as the dependent variable, only 20% (23/116) of these are positive; 80% are negative (52%) or null (28%).

Methodological Issues in the Large-N Evidence

Contributors to this literature have, of course, noticed the conflicting results, and much of the debate has turned on arguments about why we should place more confidence in one set of results than in another. Three methodological issues have emerged from this debate: whether to control for the presence of large religious groups, especially Roman Catholics; which unit of analysis is the proper one; and whether to use datasets that do not contain complete membership information on all religious groups.

Since the first of these issues is the most important, we consider it the most extensively. It emerged in the seminal analysis by Finke & Stark (1988) that used American cities in 1906 as the unit of analysis. In these data, the bivariate correlation between religious pluralism and religious participation (the percentage of religious adherents in each city, based on reports from each denomination about their membership) was moderately negative (–0.40), and the relationship became positive only when Finke & Stark added a control for the percent Roman Catholic in each city.

This finding was quickly challenged on both substantive and methodological grounds. Using data from US counties in 1980, Breault (1989a,b) found a negative correlation between religious pluralism and religious participation. Noting that the correlation between the two key "independent" variables used by Finke & Stark—percentage of a city population that is Catholic and the pluralism measure—was highly negative (–0.88), Breault argued that the positive effect of pluralism was obtained only because including this control for percent Catholic destabilized the equation, making it illegitimate to interpret that effect as a true causal relationship between pluralism and participation.

In their reply to Breault, Finke & Stark conceded that their result depended on the control for percent Catholic in each city, but they argued that the very high correlation between two independent variables was unproblematic (Finke & Stark 1989, p. 1055). The main reason it should be considered unproblematic, as they later put it, is that "the capacity of the Catholics to dominate many urban markets was due, in part, to their ability to sustain internal diversity" (Finke et al 1996, p. 206). In other words, they argued that the apparent homogeneity of Catholic-dominated areas conceals a great deal of de facto pluralism (within the Church), and that this internal diversity explains the relatively high levels of religious adherence within Catholic-dominated areas of the United States. They also objected to Breault's unit of analysis (counties rather than cities), arguing that it was too large, and they impugned the factual correctness of one of his key results, implying that his work was not credible, an implication they would later state explicitly (Finke et al 1996, p. 206 n).

The foregoing exchange inspired a steady stream of articles and chapters that employed various units of analysis, used data from various time periods and locations, and specified models in different ways. Evidence mounted on both sides, and it seemed that the debate was a stalemate, turning on a technical specification issue about which reasonable people could disagree.

The stalemate was decisively broken by Daniel Olson (1999). Olson did three things. First, he pointed out that, because the standard measure of religious pluralism uses the sum of the squared proportions of each religious group in an ecological unit (such as a city), the proportion Catholic in each unit is an arithmetic component of that measure, as is the proportion of every other group. Consequently, as any small religious group grows in size, pluralism necessarily increases—up to a point. Once a group reaches a certain proportion of the population, further increases in group size mean that the group is coming to be one of the largest in the area, and increasing its size necessarily decreases religious pluralism. Thus, for any group that varies across units from being a religious minority in some places to a religious majority in other places, there is a mathematically necessary strong curvilinear relationship between the proportion of the population belonging to that group and the standard measure of religious pluralism. If one limits analysis to units in which the proportion Catholic, for example, is large enough to be beyond the inflection point in this curve—as it is when analysis is limited to cities or urban counties in the United States—then a strong negative correlation between proportion Catholic and the usual pluralism measure is the automatic result.

Second, Olson observed that when the dependent variable is the overall proportion of religious adherents—the proportion of the population identified with *any* religious group—then the proportion of the population belonging to *each* religious group is also an arithmetic component of the dependent variable. This means that there is a mathematically necessary positive relationship between the proportion Catholic (or the proportion of any other group) in an area and the value of the usual dependent variable. If the group varies from very small to very large across units of analysis, that positive relationship will be strong.

The problematic situation, then, arises when percent Catholic is an arithmetic component of the other two variables *and* enough of the units have a Catholic population large enough to make the relationship between percent Catholic and pluralism strongly linearly negative rather than curvilinear. The combination of these features produces a strong negative correlation between the pluralism index (X1) and percent Catholic (X2), and a strong positive correlation between percent Catholic (X2) and the dependent variable (Y). This guarantees that controlling for percent Catholic will produce a positive relationship between pluralism and religious involvement. When X2 is a large enough component of both X1 and Y, this result will emerge no matter what the substantive variables involved.

The third part of Olson's demonstration drives home the point that it is not legitimate to defend controlling for percent Catholic in these models on the grounds that there is something substantively different about Catholics that warrants the control. He constructs an artificial pseudo-denomination—an aggregation of moderate Protestants, Jews, and Mormons—that makes no substantive sense but where the percent belonging to this bizarre denomination is statistically analogous to the percent Roman Catholic in the United States: It is negatively correlated with pluralism and positively correlated with religious participation. Both of these correlations, like correlations involving the percent Catholic, emerge as a matter of mathematical necessity because the percentage belonging to the pseudo-denomination (X2) is an arithmetic component of the other two variables, pluralism (X1) and the percentage of the population that are religious adherents (Y). Under these conditions, the pluralism relationship flips from negative to positive when this variable is controlled, just as it does when percent Catholic is controlled.

Olson (1999) clarified the most puzzling issue in this literature. He definitively showed that Breault was correct about the specification problem underlying Finke & Stark's original results, and his analysis undermined the defense that something substantively different about Catholics justifies the specification favored by Finke & Stark. Controlling for percent Catholic may give the impression of distinguishing between pluralism's effect and the separate effect of domination by a religious group that is somehow substantively different from the other religious groups that are present. But this impression is an illusion. In fact, when Roman Catholics are a large enough group in most of the units being analyzed, adding a statistical control for percent Catholic forces the pluralism effect to be positive because the percentage of Roman Catholics in an area is a major component both of the pluralism measure and of the usual dependent variable. Statistically controlling for the presence of any large religious group, whatever its internal substantive characteristics, would have the same effect on the model. In other words: Under these conditions the positive pluralism effect is a mathematical artifact without any substantive meaning. As of this writing, advocates of the supply-side arguments have not responded to this line of criticism in Olson (1999).

With hindsight sharpened by Olson, it is easier to see that debate about the basic question of whether, in general, pluralism enhances religious participation should have been settled with Breault's 1989 critique. The debate continued, however, fueled in part by a calculation error in Finke & Stark's (1989) attempt to replicate Breault's results. This error—the pluralism index was incorrectly calculated in a way that reversed the sign attached to it in regression models—was unrelated to the specification problem and had the unfortunate consequence of leading

Finke & Stark to assert (falsely) that a negative relationship between pluralism and participation reported by Breault was in fact positive (Finke & Stark 1989, p. 1054). Relying on this error, Finke & Stark successfully undermined Breault's critique by creating the false impression that his analyses were untrustworthy. The error was not discovered for another decade (Olson 1998a), and it is reasonable to wonder how research on this subject might have proceeded, and been received, had this error not been taken as fact.

Be that as it may, it now is clear that arithmetic relationships among the key variables compromise many of the analyses yielding a positive relationship between pluralism and participation. Of the 23 published analyses that yield a positive relationship between pluralism and participation, 12 obtain this result by controlling for a variable—usually the percent Catholic in each unit—under the problematic conditions described above. Since these results must be dismissed as artifactual, only 11 good positive results remain, less than 6% of the total.

About half of the analyses producing negative or null results also contain controls for percent Catholic or a similarly large group. But recall that the mere presence of such a control is not sufficient to create problems. That control is a problem only if it is an arithmetic component of both the pluralism measure and the dependent variable, and if the units under study are such that the first correlation is linearly negative rather than curvilinear. There are several ways to avoid this set of conditions, including using a more expansive set of units (such as all US counties rather than simply urban US counties) or operationalizing percent Catholic in a way that breaks the mathematical relationship between it and the dependent variable. Olson (1999), for example, recommends operationalizing percent Catholic as the percent of all religious adherents that are Catholic rather than as the percent of the total population that is Catholic. For us, the key point is that, although about half the analyses producing negative or null results contain controls for percent Catholic or a similarly large group, none of the negative results and only 5 of the 33 null results use this control under the full set of conditions that would make it problematic.

The bottom line: If we limit attention to the 93 analyses that use overall religious participation as the dependent variable, and for which the published articles contain enough information for us to be certain that the analysis is not compromised by problematic arithmetic dependencies among the key variables, only 12% yield a positive result and almost 90% yield negative (60%) or null (28%) results.

The second point of contention in this debate concerns the appropriate unit of analysis. Proponents of the market model argue that religious markets are local in scale, and that studies employing larger units of analysis (such as counties) may misrepresent the actual degree of pluralism faced by religious consumers insofar as the larger units aggregate a number of different local markets. There is one dataset, from New York State in 1865, in which the pluralism effect is positive when the unit is towns and null when the unit is counties (Finke et al 1996). But this pattern is not evident anywhere else in the literature. In an analysis of Canadian data, for example, Olson & Hadaway (1999) find a negative pluralism effect whether the unit is counties or cities. And in an analysis of data from England and Wales in 1851, Stark et al (1995) find that the pluralism effect is positive when the unit is counties but negative or null when the unit is cities—a pattern opposite that in the New York data and therefore unhelpful to their argument that smaller units are more appropriate.

Nor does increasing the level of magnification shift the general balance of evidence. Of the analyses in our database that focus on smaller units of analysis (such as cities or towns) and use overall religious participation as their dependent variable, only 41% yield a positive correlation between pluralism and vitality (37% are negative, 22% are null). And if we focus only on analyses that are not compromised by the specification problem described above, the figure drops to 27% positive (with 46% negative and 27% null). In sum, null or negative results occur more often than positive results even among analyses using the smaller units preferred by Finke & Stark (as in, for example, Christiano 1987, Stark et al 1995, or Olson & Hadaway 1999).

Olson & Hadaway (1999, p. 499) also point out that improved transportation makes larger units less problematic, even in theory, in more contemporary data. In addition, the potential aggregation problems when the pluralism index is applied to larger units cannot produce a negative relationship between religious pluralism and religious participation among larger units when the true relationship among smaller units is positive. Thus, for both empirical and theoretical reasons, criticizing negative or null results on the grounds that the unit of analysis is too big proves untenable.

This brings us to the third and final methodological issue in this debate. Finke & Stark (1989, p. 1054) have criticized one of the datasets commonly used in this literature—a 1980 county-level dataset of religious adherents in various denominations—on the grounds that it does not include congregations in many denominations. In particular, this dataset does not include congregations within several large African-American denominations and therefore yields a pluralism index that is too low in some settings. Breault (1989b, p. 1058) and Olson (1999, p. 159) have shown that this criticism is a red herring. Limiting analysis to units with few or no African Americans, rendering irrelevant the absence of black denominations from the dataset, changes nothing in the results. And even when all units are used, results would not be significantly altered if the dataset were more denominationally inclusive. Since the value of the pluralism measure is overwhelmingly determined by the largest few denominations in an area, the exclusion of many small groups, or even a few relatively large groups, has little effect on the pluralism index for a particular unit. And, in any event, there are many null or negative results for datasets not affected by this problem.

Excluding analyses that do not use overall religious participation as the dependent variable and those that we know to be compromised by arithmetical problems, we are left, then, with 82 nonpositive results and only 11 positive results. These 11 results are from four specific settings: New York State in 1865 (Finke et al 1996), Wales in 1851 (Stark et al 1995), contemporary Sweden (Hamberg & Pettersson

1994), and the 300 most unpopulated counties in the contemporary United States (Breault 1989b). While this body of evidence suggests that pluralism may stimulate participation in some times and places, it clearly does not support the proposition that pluralism and participation are positively associated in any general way.

We return in the conclusion to the question of how this evidence connects to the claim that religious competition (as opposed to religious pluralism) promotes higher levels of religious participation. We turn first, however, to historical and comparative evidence concerning the relationship between religious pluralism and religious participation.

THE HISTORICAL EVIDENCE

Most of the large-N studies in this literature have focused on nineteenth or twentieth century America, or have employed cross-sectional evidence from a single case such as Sweden or Wales. Only a few have attempted to assess the relationship between pluralism and participation from a cross-national or longitudinal perspective (Iannaccone 1991, Chaves & Cann 1992, Stark & Iannaccone 1994). Obviously, we cannot fill this gap in the literature here. We can, however, review existing comparative and historical evidence to see whether or not it supports the claim that pluralism and participation are, in general, positively related.

Let us begin by considering the survey evidence from post–World War II Europe and Canada (for example, Höllinger 1996, Dekker et al 1997, Cesareo et al 1995, Michelat et al 1991). It consistently and unequivocally shows two things: Orthodox religious beliefs and involvement in institutionalized religion (*a*) vary considerably from one country to another and (*b*) have steadily declined throughout Europe (and Canada), particularly since the 1960s. Are these variations associated with variations in religious pluralism? Relatedly, are they associated with variations in religious regulation—state intervention in the religious economy by means of legal repression of unwanted competitors or financial subsidy to official churches, either of which, by supply-side logic, will suppress religious pluralism and thereby decrease religious participation?

If one focused only on the Protestant-dominated polities of Northern and Western Europe, one might be tempted to support the religious economies proposition that pluralism and participation are positively related, since the lowest levels of religious vitality are to be found in the confessionally homogeneous and state-dominated religious economies of Scandinavia, with the more pluralistic and unregulated economies of Britain, the Netherlands, and Germany exhibiting higher levels of participation. When one turns to the Catholic-dominated countries of Southern and Eastern Europe, however, the picture is different. Despite their homogeneity, these countries display levels of religious belief and church attendance that are consistently higher than those found in the Protestant countries. Nor can these divergences be attributed solely to the intertwining of religious and political conflict, for they obtain not only in Ireland and Poland, where Catholicism and national liberation are historically connected, but also in Italy and Austria, where they are not. In Europe, as in the United States, the religious vitality of Catholic regions represents a troubling anomaly for the supply-side explanations.

What about longitudinal variations? Surveys show a steady decline of religious participation in postwar Europe and Canada, and other sources (religious censuses, church records) suggest that the decline began (or at least accelerated) during the late nineteenth century. Can this decline in vitality be traced either to a decline in pluralism or to an increase in regulation? That is not what the historical record suggests. Generally speaking, relations between church and state were becoming looser, rather than tighter, during this period, and new religious movements and denominations were emerging and expanding. Thus, the decline in vitality seems to have coincided with a growth in pluralism and a decrease in regulation, a picture confirmed in longitudinal studies of Canada, the Netherlands, England, Scotland, and Sweden (Beyer 1997, Gorski & Wilson 1998, Lechner 1996, Bruce 1999, Petterson & Hamberg 1997, p. 68). These trends in religious pluralism, regulation, and participation seem to have been general in Europe (Mola 1993, Helmreich 1979).

Nor do recent religious trends in post-Soviet societies provide much support for the supply-side approach, which would lead one to expect a major upswing in religious vitality following the demise of Communism. Although increases in religious affiliation occurred in Russia during the 1990s, declines occurred in other post-Soviet countries, such as Hungary and Slovenia. Still others, such as Poland, have been stable. The picture is similarly mixed with regard to church attendance. Like the longer-term pattern in the rest of Europe (and Canada), the recent trends in the post-Soviet countries for which we have data cannot be construed as showing a positive relationship between either deregulation or pluralism, on the one hand, and participation, on the other. At best, the relationship here is null (Inglehart & Baker 2000, Greeley 2000).

Market model advocates recognize that some of the cases mentioned above are problematic for their claims about the relationship between pluralism and participation, and they invoke various qualifications to account for the exceptions that are brought to their attention. They have argued, for example, that religious conflict can substitute for pluralism and competition as the energizing force behind religious vitality, and that decreasing conflict among religious groups has caused religious decline in the Netherlands and elsewhere (Stark & Iannaccone 1996). Leaving aside the issue of whether this substitute mechanism would apply to all the contrary historical cases, we note that a market explanation supplemented by qualifications of this sort is difficult to distinguish from more traditional explanations of religious change, which focus on the interaction between religious and political conflict, and cleavages of confession, class, and ideology (such as Martin 1978 and McLeod 1996).

What about the United States? Proponents of the market model have made much of the American case (Finke & Stark 1992, Finke 1990). They have argued that the opening of the religious market dramatically increased religious pluralism in the United States, and that pluralism dramatically increased religious involvement over time. The principal evidence for this claim is a long-term increase in church membership. But there is a problem with this indicator: Membership criteria have generally grown laxer over time. Today, formal membership levels are higher than attendance levels; in earlier periods, the opposite may have been true. That at least is the conclusion reached by one prominent historian of American religion who argues that "participation [as opposed to formal membership] in [US] congregations has probably remained relatively constant" since the seventeenth century (Holifield 1994, p. 24). Thus, an historic increase in formal church membership may not be a valid indicator of historic increase in religious participation. Whatever one decides about the specifics of the US historical case, however, it is of limited theoretical significance to debate about a general relationship between pluralism and participation. Even if we accept the claim that, in the United States, religious pluralism and participation are positively linked, that is an historical fact about a single case, not a basis for a theoretical claim about a general relationship between pluralism and participation.

Are there other times and places where the religious-economies propositions about pluralism, regulation, and participation fare better? At first blush, early modern Europe (ca. 1500–1750) might seem to fit the predictions of the supply-side approach quite well. The Protestant Reformation brought substantial increases in religious pluralism and religious competition. The Catholic monopoly was broken into three large multi-nationals (the Catholic, Lutheran, and Reformed Churches) and a host of smaller and more embattled religious suppliers, such as Baptists and Unitarians, which were all forced to battle for territory and people (Klueting 1989). Moreover, this increase in religious competition was accompanied by improvements in the quantity, quality, and availability of religious products—more priests and pastors, more cathedrals and churches, more schools and universities, more poor-houses and orphanages. Church-building and missionary campaigns of this period also brought religious services to many towns and villages that had been underserved or beyond the reach of the pre-Reformation Church. And, compared with their medieval predecessors, the post-Reformation clergy were better trained and probably more zealous as well.

Still, the fit between the religious-economies model and the early modern evidence is not as tight as it seems. While increased pluralism certainly brought increased competition, it did not create *market* competition in which local religious organizations competed to sell religious products to consumers. Outside a few privileged enclaves where a certain measure of religious toleration prevailed (a few German cities, most of the Netherlands, and, somewhat later, England), individuals were not really free to pick and choose the religious product that best matched their tastes (Grell & Scribner 1996, Monter 1984, Lecler 1955). The only religious product available to them, at least on the open market, was the one which their ruler(s) had chosen. Practically speaking, the only way to change religion, as a rule, was to change location—to move from Dresden to Munich, or from Paris to Edinburgh. Even in more tolerant areas, some religions were more privileged than others, and still others (such as Unitarianism) were banned altogether. With its complex intertwining of geopolitics, high ideology, and class struggle, the competition between Catholics, Lutherans, and Calvinists during the Confessional Era was more akin to the competition between communism and capitalism during the Cold War than to market competition between business firms.

Furthermore, we should be careful not to overestimate the actual increase in religious participation or vitality brought on by the increased religious competition of the Reformation era. The fact that medieval Christianity contained more pagan (that is, magical) elements than its post-Reformation counterparts indicates a change in the character of popular religiosity, but it does not necessarily imply an increase in the level of popular religiosity. And while an earlier generation of historical work (for example, Delumeau 1977, Thomas 1971, Dickens 1964) endorsed the view that a moribund medieval Christianity was revitalized by the Reformation—a view cited approvingly by advocates of the market approach (Stark & Iannaccone 1994, p. 214)—recent work on the late Middle Ages actually points to an upsurge in religious fervor in the decades preceding the Reformation (see, for example, Olin 1992, Oakley 1979, Duffy 1992). Thus, one could argue that religious vitality preceded pluralism during the Reformation, rather than the other way around.

Nor should we assume that increases in vitality during the Reformation era were produced entirely or even primarily by improvements in the quality or availability of the religious product, for there were many nonreligious incentives to participation and many nonreligious sanctions for nonparticipation. The churches were still the major providers of education and welfare during this period, and church membership was often a de facto or even de jure precondition for access to public office and public respectability. Conversely, failure to perform one's religious duties was often a public offense. These nonreligious incentives were strengthened during periods of confessional strife and warfare, when displaying irreligion—or displaying the wrong religion—could be seen as a sign of unreliability or even treason. Thus, religious participation might increase as a result of an intensified religio-political competition that bears little resemblance to market competition.

Like the large-N studies, the historical and comparative evidence fails to support the claim that there is a generally positive association between religious pluralism and religious participation. As with the evidence from the large-N studies, the historical and comparative evidence suggests only that there might be certain times and places, such as the nineteenth-century United States or, perhaps, post-War Japan (Iannaccone et al 1997, p. 357–58) where decreased religious regulation or increased religious pluralism was concomitant with increased religious participation. This evidence cannot be made to support the universalistic ambitions of market models of religion. For a theory with such ambitions, Stark et al (1995, p. 436) correctly note, "it is urgent that we test it in diverse times and places." Indeed, they say, "data for ninth-century China would be even better than data for nineteenth-century England." We have not found a study of ninth-century China, but the historical studies we have found clearly show that the theory does not travel well across historical time or cultural space.

We have not exhaustively reviewed all extant historical evidence, and perhaps there are additional settings—either in other parts of the Christian world (such as Latin America) or in parts of the world dominated by Islam or other non-Christian religions—in which increased religious pluralism seems to have produced increased religious vitality. Even in that event, however, the old wisdom that religious pluralism is negatively associated with religious vitality will be validly replaced only by the conclusion that "sometimes it is, sometimes it isn't," not by the conclusion that the relationship is generally positive. There are too many contexts in which the relationship appears to be nonpositive to justify a claim to the contrary.

CONCLUSION

We have done two things in this essay. First, we have weighed the evidence on religious pluralism contained in the relevant large-N studies. Second, we have used comparative and historical evidence drawn mainly from Western Europe and North America to assess the empirical and conceptual scope of the claim that religious diversity increases religious pluralism via market competition. Our conclusion is simple. The claim that religious pluralism and religious participation are generally and positively associated with one another—the core empirical hypothesis of the market approach to the study of religion—is not supported, and attempts to discredit countervailing evidence on methodological grounds must be rejected. A positive relationship between religious pluralism and religious participation can be found only in a limited number of contexts, while the concepts themselves translate poorly to nonmodern settings.

What does this conclusion imply about future research on religious pluralism, church-state relations, and religious competition?

First, the supply-side approach should be distinguished from a broader agenda that has been called a new paradigm in the sociology of religion. Although supply-side and rational choice approaches to religion are often equated with the new paradigm, it is important to note that the article that coined the term (Warner 1993) did not identify the new paradigm with an economic or rational-choice approach to religion, nor did it claim a positive association between pluralism and participation. Although Warner observed that organized religion in the United States has not experienced the kind of decline experienced in many European countries, and although he also observed that religious institutions in the United States operate in the context of an open religious market, he did not claim that there was any necessary connection between these two states of affairs, much less that open markets or pluralism are a sine qua non of religious vitality. Rather, he used the US case to argue that an older wisdom, developed on the basis of European experience and insisting that religious monopoly was a sine qua non of religious vitality, did not apply to the United States.

Our reading of the evidence suggests that Warner was right to question this older wisdom. The relationship between pluralism and vitality is not uniformly positive, but neither is it uniformly negative. One quarter of the published quantitative results on this relationship are null, and there are times and places where increased religious pluralism is accompanied by increased religious vitality. The original new paradigm analysis thus accurately identified a major weakness in the received wisdom, but it did not imply the stronger, unsupportable claim that pluralism and participation are positively related in any general way. Future scholarship should refrain from equating the analysis and agenda actually articulated in Warner (1993) with the religious economies or rational-choice approach to religion.

Second, future research might build on the literature reviewed here by investigating religious pluralism and its consequences in new ways. As we have emphasized throughout, there are some contexts in which pluralism and vitality are positively correlated, some in which they are negatively correlated, and some in which the correlation is null. One key task for future research will be to explain this variation by specifying the conditions under which one or another of these relationships obtains. Also, freed from the agenda of proving that either competition or plausibility is the primary mechanism by which pluralism might influence religious activity, future research on religious pluralism's consequences may yield interesting new results. Olson (1998b), for example, has investigated the possibility that pluralism affects religious participation by influencing individuals' social networks. Blau et al (1997) examine relationships between pluralism and growth for specific denominations, developing explanations for why pluralism might variously influence different religious groups. Smith et al (1998) have called attention to the cultural aspects of religious pluralism, emphasizing the connection between religious pluralism and the salience of religious identities. These are three examples of promising directions for future research on religious pluralism.

Another potential direction for research on pluralism is suggested by Finke & Stark's more recent proposal that pluralism increases religious participation only when pluralism crosses a rather low threshold. The "key distinction," they have written, "is between areas having no diversity and those having some degree of pluralism" (Finke et al 1996, p. 210, quoted in Finke & Stark 1998, p. 763). As Olson & Hadaway (1999, p. 504) point out, a hypothesis restricted to the difference between areas with some pluralism and areas with no pluralism will explain little of the world's cross-sectional and historical variation in religious vitality. In particular, this narrowed hypothesis will not help explain cross-sectional or historical variation in religious vitality in nineteenth- or twentieth-century United States, or in any other time or place where very few areas have so little religious pluralism that they fall below the posited theoretical threshold. Still, it seems plausible that religious participation is higher where individuals have some rather than no religious options, and this hypothesis might well indicate a properly narrowed scope for a market model of religious pluralism's effect on participation. Future research might productively be directed at establishing whether this more modest claim is true or false.

Third, there is more to learn about the connections between church-state institutional arrangements and religious activity in a society. As we have seen, the regulation hypothesis—that state intervention in the religious market leads to lower levels of religious pluralism and religious vitality—is at odds with much historical evidence. In the Reformation era, religious regulation, religious pluralism, and religious vitality all increased, a pattern inconsistent with supply-side arguments. In the modern era religious deregulation and religious pluralism have both increased throughout Europe and Canada, but religious participation has declined, the opposite of what the market model would predict. And even where deregulation is associated with increased pluralism (if not increased religious vitality), the causal order is not always clear. In at least some settings, institutional distancing between states and religion—deregulation—was actually a response to increased levels of pluralism, reversing the causal ordering assumed in market models (Beyer 1997). Dropping the market model assumption about causal order in favor of research aimed at discovering the proper causal order—or, more likely, discovering the balance of reciprocal causation between deregulation and pluralism—seems a fruitful agenda for future research.

Relatedly, our review of the historical and comparative evidence found both positive and negative associations between state regulation of religion and religious vitality. In some places, such as Russia and, perhaps, the United States, an increasingly open religious market seems to have produced higher levels of religious participation and vitality. But in much of Europe and Canada, the historical trajectory has been in the direction of an increasingly open religious market and *declining* participation. What explains this variation in the relationship between church-state changes and changes in religious participation? Future research might address this question.

Fourth, the absence of any general positive relationship between pluralism and participation does not bode well for the more general idea that market-like religious competition promotes religious vitality. One might, of course, try to protect claims about religious competition by arguing that pluralism is not an adequate measure of the competitive processes posited by market arguments, its common use to the contrary notwithstanding. This point has been made by proponents of market models, as when they write that "*competition* lies at the heart of our theory" and "religious *pluralism* is important *only* insofar as it increases choice and competition" (Stark & Iannaccone 1996, p. 266, emphasis in original). We agree that religious pluralism is not a good measure of religious competition, but it is important to recognize that, absent the pluralism literature, there is no other body of evidence on which to rest the claim that market-like religious competition generates higher overall levels of religious participation.

Beyond the pluralism literature, the much smaller literature on market share effects is the most likely place to look for support of a competition hypothesis. The argument here is that the amount of competition a religious group faces increases as that group becomes a smaller and smaller percentage of the population in a given area, and so finding that groups elicit more support and participation from their adherents when their market share is smaller shows that competition promotes religious participation. We argued above that a religious group's market share is not a valid measure of how much religious diversity it faces. For similar reasons it is not obvious why we should assume that a religious group constituting, say, 20% of the population faces more competition than a group constituting, say, 70% of the population. Among other considerations, the fact that minority religions are commonly associated with ethnic identities or language groups, and therefore are not competing with other religions for members, raises questions about the validity of a religious group's market share as a measure of how much competition that group faces for members. Moreover, whatever one thinks about this measurement issue, the empirical literature on market share effects provides only mixed support for the idea that religious groups with smaller market shares enjoy higher levels of participation (cf, for example, Stark & McCann 1993 and Perl & Olson 2000, on the one hand, with Phillips 1998, on the other hand).

Thus, given the absence of any support from the pluralism literature, the mixed support from the market share research, and the ambiguous relevance, anyway, of the market share research to claims about competition, it is difficult to discern any basis for the claim that a "large, rapidly growing, and remarkably diverse literature support[s] the thesis that competition strengthens religious organizations and increases the overall level of religious participation" (Finke & Stark 1998, p. 761). The general claim that religious competition generates religious vitality is no better supported by empirical evidence than is the narrower claim that religious pluralism generates religious vitality.

All that said, our fifth concluding point is that the empirical failure of the market model's core claim does not imply abandoning the agenda of investigating the consequences of religious competition. Advocates of market models of religion should be credited with bringing religious competition to the attention of the scholarly community, and we do not wish to discourage either additional work on this subject or the use of economic theory in analyses of religious competition. The weaknesses of extant research on religious competition are not endemic to the concept of religious competition or to the use of economic theory. Rather, the problems emerge from the inadequacies of the particular approach to the study of religious competition, and the particular application of economic theory, that have come to dominate the literature. New investigations of religious competition are likely to be valuable if they take more seriously the social and historical context in which competition occurs.

This is not the place to develop in full a more adequate conceptualization of religious competition to inform future research, but we can sketch some of its basic features. In settings in which religious congregations are voluntary associations dependent on donations from individuals, a more sociologically adequate framework for the study of religious competition would recognize, among other things, that there probably is more competition among congregations *within* denominations than there is *across* denominations; that religious markets are severely segmented along social class and ethnic lines; that religious groups and organizations compete for individuals' time and resources with secular as well as alternative religious voluntary associations and leisure activities; and that religious competition's consequences for religious identities is likely to be different than its consequences for religious participation.

As currently practiced, the empirical analysis of religious competition recognizes none of these realities. Instead, standard analyses using religious pluralism as a proxy for religious competition implicitly assume that all religious competition is between denominations, that all denominations within a geographically defined area compete equally with all other denominations in that area, that the only relevant kind of competition is competition among religious groups, and that the market mechanisms generating the percentage of a population identifying with any religious group will be the same market mechanisms generating the percentage of people who actively participate in religious groups.

Future research on religious competition in settings where religion is essentially voluntaristic should use a more conceptually adequate analytical framework. Improved conceptualization might preclude the use of some economic theories and models in the study of religious competition, but we see no reason that it should preclude the use, or lessen the value, of economic theories and models in general. Future research might follow Hull & Bold (1998) in exploring the possibility that economic theory other than that adopted by proponents of the supply-side model can be used to better understand religious phenomena.

Future research also should recognize that there are at least two qualitatively different types of religious competition. In settings where religious organizations depend on voluntary contributions from individuals, religious organizations will compete for individuals' time and resources, and they will compete both with each other and with mass entertainment, sports teams, political parties, and many other sorts of groups and activities. By contrast, in settings where religious membership is more like modern citizenship than like membership in a voluntary association, religion often becomes intertwined with political, social, and cultural conflict between states, classes, and national or ethnic groupings; religious allegiances become markers or signals of nonreligious allegiances; and religious competition means struggles over cultural, political, and territorial influence and power. As we argued above, the religious competition characterizing Reformation era Europe, and perhaps characterizing certain contemporary settings around the world, is of this second sort, and it is qualitatively different from the religious competition characterizing most of the contemporary West, which is of the first sort. One way for research to advance will be to discern when and where one rather than another type of religious competition occurs, and to develop explanations of the consequences of each type of religious competition.

Finally, let us state explicitly a conclusion that has been implied several times in the foregoing: The quest for a general law about the relationship between religious pluralism and religious participation should be abandoned. The evidence clearly shows that any such general law, to be accurate, would have to be formulated with so many exceptions and qualifications that its claim to generality or lawfullness would be empty. Rather than an either-or argument about whether religious pluralism is, in general, positively or negatively associated with religious participation, the most valuable future work on this subject is likely to include investigations into the social, cultural, and institutional arrangements that determine, in part, religious pluralism's consequences for religious vitality. This will be the route to a more adequate sociology of religion, one that moves toward a political economy of the religious sphere by placing religious markets in larger cultural and institutional contexts.

ACKNOWLEDGMENTS

Nancy Ammerman, Emily Barman, Penny Becker, Al Bergesen, Ronald Breiger, Lis Clemens, Karen Cook, Jay Demerath, John Evans, Claude Fischer, William Form, Ted Gerber, Helen Giesel, Michael Hout, Mary Ellen Konieczny, James Montgomery, Kate O'Neil, Chris Smith, Lynn Smith-Lovin, Sarah Soule, Steve Warner, Rhys Williams, and an anonymous reviewer offered helpful comments on earlier drafts of this essay. Special thanks to Helen Giesel for assistance in preparing the database of quantitative results.

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