Review Article

FAITH IN POLITICS

New Trends in the Study of Religion and Politics

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Joel Fetzer and Christopher Soper. *Muslims and the State in Britain, France, and Germany*. New York: Cambridge University Press, 2005, 208pp.

Jonathan Fox and Shmuel Sandler. *Bringing Religion into International Relations*. New York: Palgrave MacMillan, 2004, 212pp.

Anthony Gill. *Rendering unto Caesar: The Catholic Church and the State in Latin America*. Chicago: University of Chicago Press, 1998, 269pp.

Stathis Kalyvas. *The Rise of Christian Democracy in Europe*. Ithaca, N.Y.: Cornell University Press, 1996, 300pp.

Pippa Norris and Ronald Inglehart. *Sacred and Secular: Religion and Politics Worldwide*. New York: Cambridge University Press, 2004, 329pp.

Scott Thomas. *The Global Resurgence of Religion and the Transformation of International Relations*. New York: Palgrave MacMillan, 2005, 300pp.

Carolyn Warner. *Confessions of an Interest Group: The Catholic Church and Political Parties in Europe*. Princeton: Princeton University Press, 2000, 249pp.

RELIGION has long been peripheral to the concerns of most political scientists. Perceived as limited in theoretical reach and methodological sophistication, studies of religion in politics have typically been shunted to the margins of the profession. But of late religion has begun to force its way into the mainstream of the discipline, a trend fostered by two important developments. First, the increasing methodological sophistication of specialists in this subfield has linked the study of religion to broader theoretical questions in political science. Second, real-world events have put religion front and center in current affairs, posing puzzles that demand explanations from our field if we are not to lapse into scholastic irrelevance. Consequently, a host of books, as well as two new series published by Cambridge University Press and

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Palgrave Macmillan, have been devoted to the study of religion in comparative and international politics.[1]

The renaissance in this subfield has led to important advances in our understanding of religion in politics, although notable lacunae remain. In comparative politics, the subfield's turn from purely descriptive work to structured comparison has yielded important insights suggesting the rationality of religious behavior, the role of contingency and choice in shaping politico-religious outcomes, and the weight of path dependence and institutional endowment in shaping values such as religious tolerance. But the subfield has still failed to reckon with the power of religion as an independent variable, the noninstrumental aspect of religious behavior, and the malleability of religious ideas, as well as their differential appeal, persuasiveness, and political salience over time. In international relations recognition of the importance of religious identities and values in international politics constitutes an advance over realist caricatures of this arena and promises to unlock important empirical puzzles posed by current events. However, few of the new studies go much beyond exhortation for a paradigm shift in IR. Far too many succumb to epistemological debates about the logic and validity of causal inquiry in human affairs. And most miss the opportunity to get on with the project of puzzle-driven research that might shed light (and middle-range theoretical insights) on questions of when and how religion matters in international affairs. Thus, while much of this work has started us on the way toward a richer understanding of the dynamics of religion and politics, this new literature also points to areas that call out for further exploration.

The Irrelevance of Religion

The long-standing neglect of religion in comparative politics and international relations derives from conditions specific to each subfield. Among comparativists, the tendency to ignore religion can be traced

[1] This essay takes inspiration from several excellent overviews of the field of religion and politics, including Anthony Gill, "Religion and Comparative Politics," *Annual Review of Political Science* 4 (2001); and Kenneth Wald, Adam Silverman, and Kevin Fridy, "Making Sense of Religion in Political Life," *Annual Review of Political Science* 8 (2005). Also useful were several excellent review essays on the role of ideas in politics, including Jeffrey T. Checkel, "The Constructivist Turn in International Relations Theory," *World Politics* 50 (January 1998); Sheri Berman, "Ideas, Norms, and Culture in Political Analysis," *Comparative Politics* 33 (January 2001); Mark Blyth, "Any More Bright Ideas? The Ideational Turn of Comparative Political Economy," *Comparative Politics* 29 (January 1997); Martha Finnemore and Kathryn Sikkink, "Taking Stock: The Constructivist Research Program in International Relations and Comparative Politics," *Annual Review of Political Science* 4 (2001). See also Kenneth Wald and Clyde Wilcox, "Getting Religion: Has Political Science Rediscovered the Faith Factor?" *American Political Science Review* 100 (November 2006).

to the subfield's theoretical inspiration drawn from the work of Weber, Durkheim, and Marx. All three theorists believed that religion was a premodern relic, destined to fade with the advance of industrialization, urbanization, bureaucratization, and rationalization (Norris and Inglehart, 3). This conviction, later named "secularization theory," became one of the most uncontested schools of thought in academe (Gill, 3). Under its influence religion was perceived as anachronistic, if not epiphenomenal. Most comparativists steered clear of its study.

By the late 1970s, however, empirical reality began to challenge the axiom that modernization would inevitably spell the decline and political insignificance of religion. The rise of the Islamic revolution in Iran, the persistence (and political salience) of religious devotion in the United States, the growing importance of liberation theology in Latin America—all suggested that religion remained a consequential force in contemporary politics, even in relatively developed countries. This reality sparked a surge of studies on everything from the politics of evangelical Christianity in the Americas and the dynamics of Islamic fundamentalism in the Middle East to the role of the Catholic church in Poland's break from communism.[2]

For the most part, however, these studies had limited impact on the discipline as a whole. For while they were often brilliantly analytical, they were also, by and large, descriptive case studies—not aimed at generating or testing hypotheses, not linked to larger theoretical debates in political science, and not cumulative in any theoretical sense. Even studies that were explicitly multicountry in conception, while terrifically informative, were rarely organized around structured comparison and often ended up as exercises in comparative statics rather than theory building.[3] Given the limited scope of these books, political

[2] There is a vast literature on the subject of religion in the public sphere. On religious resurgence, see, for example, Martin Marty and R. Scott Appleby's multivolume series on fundamentalism (Chicago: University of Chicago Press, 1991, 1992, 1993, 1995). On religion in public life, see Jose Casanova, *Public Religions in the Modern World* (Chicago: University of Chicago Press, 1994); and for more IR-oriented themes, see fnn. 32 and 33 of this article. There are also countless case studies of religious lobbies and their role in democratic politics, the rise of religiously inspired social movements, the development of religious discourse in public life, among other themes, many written by historians, sociologists, and theologians. For guidance on this very large literature, see the Web site of the APSA section on Religion and Politics for excellent syllabi and book reviews.

[3] A recent example of one such excellent book is Steven Monsma and Christopher Soper, *The Challenge of Pluralism: Church and State in Five Democracies* (Lanham, Md.: Rowman and Littlefield, 1997). See also T. S. Madeley and Zsolt Enyedi, *Church and State in Contemporary Europe: The Chimera of Neutrality* (London: Frank Cass, 2003). One book that acknowledges the need to go beyond comparative statics is Ted Gerard Jelen and Clyde Wilcox, eds., *Religion and Politics in Comparative Perspective: The One, the Few and the Many* (New York: Cambridge University Press, 2002); nevertheless, the bulk of the book consists of case studies offered as "data to help develop theories and refine concepts" in future work.

scientists without direct interest in the specific country or case under study could ignore the work with impunity.

In the subfield of IR a different set of factors spelled the disregard of religion. Historically, the establishment of the modern international order was associated with the formal ejection of religion from international affairs. Many of the constitutive principles of the international order, including the principles of state sovereignty and noninterference in the domestic affairs of other states, were first codified by the Treaty of Westphalia. This compact aimed to end the wars of religion in Europe by enshrining the principle of noninterference in the religious preferences of other states. The historical experience of Westphalia indelibly associated the removal of religion with the establishment of international order and planted an enduring suspicion of injecting religion into international affairs.[4]

Beyond this quasi-normative impetus (that religion *ought* to be excised from international affairs if one valued international peace), the disregard of religion by IR scholars was also spurred by the intellectual conviction, predominant during much of the cold war era, that a realist model best captured the dynamics of international politics. This model assumed that states were the primary building blocks of the international system and that state pursuit of interests (defined as the quest for power and wealth) constituted the main driver of international affairs. Ideas were largely secondary forces in this process—instrumental (but not causative) in the states' quest for power and wealth. Religion as a subset of ideas was similarly relegated to the sidelines. Liberal scholars eventually challenged some of the assumptions of the realist model, especially its statecentrism. They made space for ideas in international politics in the form of laws, institutions, and regimes that limited anarchy and fostered cooperation in the international system. But like the realists, they largely ignored the role of religion in international affairs. Nor did the subfield's "constructivist turn" in the 1990s do much to resurrect the subject of religion. Constructivist scholars problematized the notion of state interest and injected identity and ideas into the political construction of state objectives. But rarely was the study of religious identity or religious ideas central to their intellectual agenda.[5]

[4] Pavlos Hatzopolous and Fabio Petito, *Religion in International Relations: The Return from Exile* (New York: Palgrave MacMillan, 2003), 6, 12; Scott Thomas, "Taking Religious and Cultural Pluralism Seriously," in Hatzopolous and Petito, 24; John Carlson and Erik Owens, "Reconsidering Westphalia's Legacy for Religion and International Politics," in John Carlson and Erik Owens, eds., *The Sacred and the Sovereign* (Washington, D.C.: Georgetown University Press, 2003), 14. For an excellent historical account of the concept of sovereignty, see Daniel Philpott, "Sovereignty: An Introduction and Brief History," *Journal of International Affairs* 48 (Winter 1995).

[5] See Finnemore and Sikkink (fn. 1); and Checkel (fn. 1). See also Daniel Philpott, "The Challenge of September 11 to Secularism in International Relations," *World Politics* 55 (October 2002), 10.

All this has changed of late. In comparative politics the stimulus for bringing religion into the mainstream of the discipline has been the increasing methodological sophistication of students of religion. More and more we see the emergence of scholars interested not solely in analytic description and comparative statics but also in theoretically ambitious hypothesis generation. These scholars embrace puzzle-driven research. Their work is explicitly comparative in design. They bring to the enterprise a host of sophisticated tools, both quantitative and qualitative, and are well versed in the competing comparative approaches, from rational choice to historical institutionalism. Most importantly, many of these scholars explicitly link their findings to larger questions in the field, focusing on the relationship between agency and structure, ideas and institutions, contingency and path dependence. Consequently, even comparativists with little interest in the particulars of the evangelical movement in Latin America or the institutional structure of Shiism in Iran have good reason to take notice of this work.

In IR the spur to bringing religion into the mainstream has been less methodological than empirical. Real-world events have forced reconsideration of the subject. The end of the cold war unleashed a surge of identity politics in the international arena, with some of it cast in religious terms. Several signal books spotted this development early on, including Samuel Huntington's *Clash of Civilizations,*[6] Benjamin Barber's *Jihad vs. McWorld,* [7]and Mark Juergensmeyer's *The New Cold War? Religious Nationalism Confronts the Secular State.*[8] But what really swelled the American audience for inquiry into the role of religion in international relations were the events of September 11 and the rise of what was perceived as a religiously driven, transnational terrorist movement. "La Revanche de Dieu" had reached American shores in violent and threatening fashion and IR specialists felt pressed to explain its implications for international order and security.

New Directions in Comparative Politics: The "Religious Economy School"

The move away from analytically informed but essentially descriptive case studies of religion in politics and toward explicitly comparative, puzzle-driven work began to gather steam in the last decade or so. The

[6] Huntington, *Clash of Civilizations* (New York: Touchstone, 1997).

[7] Barber, *Jihad vs. McWorld* (New York: Times Books, 1995).

[8] Juergensmeyer, *The New Cold War? Religious Nationalism Confronts the Secular State* (Berkeley: University of California Press, 1993).

first major trend in this development is represented by a clutch of books dubbed "the religious economy school" by one of its authors.[9] The school is united by a commitment to applying microeconomic analysis and the logic of rational choice to the study of religion. For the most part these authors focus on explaining the dynamics of religious behavior and specifically the behavior of religious institutions (such as the Catholic church). All embrace an economic model of church behavior. All enlist the metaphor of church as economic firm, arguing that the church must reckon with organizational imperatives for survival and market share just as any other firm would and that such rational reckoning predicts church behavior as well if not better than any of its ideological commitments. Many make compelling cases for the importance of agency, choice, and contingency (as opposed to structural determinism) in shaping religious outcomes. In all, the religious economy school suggests some important insights about the dynamics of religious behavior, although not without cost.

One of the earliest examples of the religious economy school is Stathis Kalyvas's *The Rise of Christian Democracy in Europe.* Focusing on the evolution of confessional parties in Europe, Kalyvas explores two puzzles. First, what explains the variation in the emergence of successful confessional parties in Catholic Europe in the late nineteenth and early twentieth centuries? Specifically, why do significant Catholic parties emerge in countries such as Germany, Italy, and Belgium but fail to emerge in (equally, if not more Catholic) France, Spain, and Ireland? Second, why do confessional parties in Europe progressively secularize over the course of the twentieth century even as they retain their religious identification? Both questions have enormous significance for those trying to anticipate the evolution of religious parties in many different regions of the world today.

To explore these puzzles Kalyvas embraces a combination of comparative historical analysis and rational choice theory. He argues that the differential development of confessional parties across Europe was a consequence of church and lay leaders responding to two conditions: (1) liberal attack,[10] and (2) the political resources available to parry that liberal attack. Where liberal attack was absent (for example, in Ireland and Spain) no mobilization of confessional parties was motivated, no matter how Catholic the populace. Where liberal attack was present, the church proved willing to support confessional parties, but only as

[9] Gill (fn.1).

[10] In nineteenth-century Europe, many liberals campaigned to secularize public institutions like education and marriage that had previously been the exclusive domain of the church.

a last resort when no alternative political strategies were available. The church had a strong distaste for political parties and for popular mobilization generally.[11] Wherever possible, it opted for elite-based bargains to defend its interests. Consequently, in France, where there was a powerful monarchist party committed to defending church privilege and seemingly capable of defeating the liberals, the church strongly opposed the organization of a confessional party and allied with the monarchists instead. By contrast, in Germany and Belgium, where no elite-based strategy seemed credible, the church reluctantly sanctioned the mobilization of confessional parties to parry the liberal threat.

In short, confessional parties were never the ideological preference of church leaders but rather reflected a strategic calculation on the part of an institution ever attuned to its own survival.

A similar concern with institutional survival explains Kalyvas's second puzzle. He argues that confessional parties, constrained by democratic imperatives, embraced secularization out of a strategic calculation of their organizational interest.[12] The strength and survival of these parties depended on forging alliances with other political forces and capturing an ever larger portion of the electorate (many of whom were not Catholic and/or not devout). In an effort to appeal to both, the confessional parties chose to dilute their religious message and stress their independence from the church. Strategic calculation persuaded them to opt for partial secularization, as well as prevented them from dropping their religious identification altogether, since this identity guaranteed them a reliable voter base.

Kalyvas makes a compelling case for the priority of strategic calculation of organizational interest over ideology in accounting for church behavior in the matter of confessional parties. Church ideology was constant across country cases. Nowhere was the church enthusiastic about the political mobilization of the laity. But church support for confessional parties varied across country cases in direct relation to the church's sense of organizational threat and the means it had to parry that threat. Similarly, Kalyvas makes a strong case for the priority of choice and agency over structural determinism in shaping political out-

[11] The creation of Catholic parties threatened to end the church's monopoly on representation of the Catholic community; it challenged the church's hierarchical control over Catholics; and, most importantly, it signaled endorsement of democratic principles, a position distinctly at odds with the church's official doctrine at that time (p. 48).

[12] Strategic calculation of organizational interest also explains the confessional parties' gradual "conversion" to enthusiasm for democratic principles and practices. Lay leaders in the parties came to realize that much of their political power and legitimacy derived from electoral success (not church support) and that the best way to reinforce their position (as well as ensure autonomy from church control) lay in embracing parliamentary ways rather than cleaving to church directives.

comes. The successful emergence of confessional parties did not correlate with structural variables such as the demographic weight of Catholics in society or the depth of the historical church-state split. Rather, it was strategic choice exercised by Catholic leaders that spelled the difference. Finally, Kalyvas argues persuasively for the importance of contingent and unintended consequences in political affairs. This is especially evident with regard to the progressive secularization of confessional parties in Europe. The church never intended to create semisecular parties. Once it set confessional parties in motion, however, these organizations followed their own logic, strategically calculating their survival under the constraints of the democratic system, and this led to consequences never anticipated by church leaders. In short, Kalyvas makes a powerful argument for the importance of strategic calculation, agency, contingency, and interaction effects in public affairs in general and in religious politics specifically. But the elegance of the argument comes at the cost of some blinkering, as will be shown below.

A second book in the religious economy school takes up the story of confessional parties in Europe where Kalyvas leaves off. In *Confessions of an Interest Group: The Catholic Church and Political Parties in Europe*, Carolyn Warner asks: why did the church choose to ally with Christian Democratic parties in some countries in the postwar era but not in others? Warner thus takes the story beyond the dynamics governing the original emergence of confessional parties and focuses instead on the variable willingness of the church to support these parties later on. She is especially intrigued by the finding that the church routinely allied with parties that were ideologically suboptimal. Equally surprising is the fact that across country cases, the ideological correctness of the confessional party did not correlate with level of church support. How can these surprising findings be explained?

Like Kalyvas, Warner explains the puzzles by enlisting the metaphor of church as firm. She develops an economic model of church behavior, arguing that the church, in forging political alliances, rationally calculated its organizational interests. It pragmatically sacrificed ideological purity when the costs of purity outweighed the benefits. In Germany, for example, the church allied with the biconfessional (Catholic and Protestant) Christian Democrat Party rather than with the purely Catholic Zentrum Party, because the CD seemed likely to win more votes (even capture a majority).[13] Access to rule was the summum bonum for the church, an objective that outweighed ideological purity. In

[13] The Zentrum, tainted by its association with the rise of Nazism, commanded a smaller popular base.

Italy the church remained allied with the Christian Democratic Party (despite the CD's association with scandal and its preference for coalitions with despicable socialists and liberals) because the church lacked a credible exit option. No other Christian-oriented party existed, and withdrawing support from the CD might have created an opening for Communist Party rule (the summum malum) in Italy. In addition, removal of the CD from office would have cost the church the significant side payments it reaped from the party's rule: office jobs in the administration for church members, government contracts for church banks, and so on.

In France, by contrast, rational calculation of organizational costs and benefits encouraged the church to withdraw support from the leading confessional party, which was weak and unlikely to win a major share of power. Thus, when the party disappointed the church ideologically, there were lower opportunity costs associated with the exit option, which the church exercised by withdrawing its support. In each case the church weighed the party's relative purity, policy capability, and reliability in making its political alliances. Such calculation, rather than ideological purity alone, governed church behavior.

Warner makes clear the utility of an economic model for explaining church behavior. But her work also emphasizes the utility of supplementing a purely economic model with attention to factors such as historical legacy, institutional structure, and leadership in order to explain variation in the church's political position. Warner argues, for example, that one cannot explain the variation in the political ambition and boldness of the different national churches without acknowledging the role of historical precedent. In France the historical association of the church with the Vichy regime discredited it in popular eyes and forced the church to rein in its political ambitions in the postwar period. By contrast, in Italy (and to a lesser extent in Germany) the church was less implicated in Fascist (and Nazi) rule, and the relative legitimacy of the institution in the postwar period permitted it to entertain bolder political aspirations. Similarly, institutional structure also contributed to the church's political ambition and boldness. In Italy the church was highly unified under a single hierarchical command, led by the pope. This spelled political decisiveness. In France, by contrast, the national church was highly decentralized, even fragmented. This spelled political impotence.

In short, Warner makes a compelling case for explaining church behavior by folding in contextually specific factors that are generally ignored by microeconomic analysis. Admittedly, the added explanatory power conferred by incorporating these variables comes at the cost of parsimony and generalizability. But such a trade-off is in the nature of things. The fact that Warner's account is more nuanced and historically contingent does not negate the fact that her work is still puzzle driven, theoretically insightful, and generalizable. Still, as with the other works in the religious economy school, the reliance on a microeconomic approach to explain religious behavior leaves certain gaps in our understanding, as will be explored below.

A third foundational book in this group is represented by Anthony Gill's *Rendering unto Caesar: The Catholic Church and the State in Latin America.* The central question motivating this work is how to explain the variation in the church's stance toward dictatorial rule across cases in Latin America in the post-1960 era. More specifically, how do we explain the fact that the national episcopacies in countries like Chile and Brazil actively denounced authoritarian rule while those in Argentina, Uruguay, Honduras, and Bolivia did not?

To explain this phenomenon, Gill rejects rival hypotheses that focus on leadership, ideology, or purely structural variables. With regard to leadership, Gill argues that excessive focus on individuals leads to a loss of generalizability and ignores "the systematic and institutional forces that condition the decisions that individuals make" (p. 41). With regard to ideology, Gill rejects the hypothesis that ideological reform associated with Vatican II explains the newfound enthusiasm for democracy in the church; he makes his case on the grounds that exposure to Vatican II was uniform across Latin American churches whereas their opposition to authoritarianism was variable. With regard to structural factors, Gill rejects the hypothesis that variation in the political stance of the church across countries was driven by variations in the level of economic distress and political repression experienced each. In fact, Gill finds no significant correlation between levels of poverty and repression and church stance. For example, Bolivia and Guatemala suffered high levels of poverty and repression, yet the national church stood by the authoritarian regime. Chile and Brazil suffered lower levels of poverty and repression, yet the national church in those countries proved anti-authoritarian (p. 107).

To solve the puzzle, Gill proposes a hypothesis that takes an institutionalist approach, likens the national church to a firm, applies a rational actor model of behavior, and emphasizes the importance of what he call "religious competition." He argues that the key variable accounting for the variation in the church's political stance is the degree to which the Catholic church faced religious competition, in particular, competition posed by evangelical Protestantism. Historically, the Catholic church had enjoyed a near monopoly over religious life in Latin America and had also long been allied with authoritarian regimes on the continent. These two conditions meant the church could afford to neglect the concerns of its laity (poverty, repression) because alliance with the regimes in power provided the church with sustaining material benefits and because the church's monopoly on religion meant the laity lacked an exit option with which to punish it. But in the twentieth century the influx of an energetic Pentecostal movement began to challenge the church's monopoly on religion in some Latin American countries. The Pentecostal movement made an explicit pitch to the poor and in many cases proved highly successful in drawing significant numbers of former Catholics to their ranks.

Faced with this challenge, Gill argues, the national church acted much like a firm: it sought to protect its organizational survival and rationally calculated what changes were necessary to protect its "market position." Consequently, where the church faced a threat from evangelical Protestantism, it chose to make a credible commitment to the poor and, among other things, challenged the brutality of reigning authoritarian regimes. Where the church faced no such threat, it refrained from denouncing authoritarianism. Thus a country's relative endowment with religious competition is the best predictor of the national church's stance on authoritarianism.

To test this hypothesis, Gill relies on an admirably wide variety of methodological approaches, both quantitative and qualitative. He employs cross-national multivariate analysis, some (albeit limited) interviewing, a brief historical review of twelve country cases, and close historical analysis of two country cases, Chile and Argentina. In short, Gill's work embodies the emerging methodological orthodoxy in comparative politics (to embrace methodological diversity) and energetically triangulates in the hope that one technique can "correct for the weaknesses of another" (p. 11).

The utility of this analysis is multiple. First, Gill makes a strong case for a supply-side (as opposed to demand-side) approach to explaining the evolution in religious behavior. This is a hot debate in the sociology of religion (see Norris/Inglehart below), and Gill's contribution to the discussion is important. Second, Gill makes a compelling case for the utility of the firm metaphor in accounting for the behavior of institutions. Like Kalyvas and Warner, he argues that the church is little different from other institutions and that rational reckoning with organizational imperatives (for example, concern for survival and market share) predicts its behavior as well as, if not better than, ideological commitment. Finally, Gill provides an excellent example of how comparativists might best implement the strategy of methodological triangulation to solve political puzzles. It doesn't hurt that Gill's writing is also elegant and crisp, a model of clarity that alone should mandate broad readership among comparativists.

The religious economy school, as represented by these three foundational books, offers important new insights into the dynamics of religious behavior, especially the behavior of religious institutions. All three make a compelling case for the priority of organizational concerns over ideology in explaining the choices of the church. All three show the utility of economic metaphors and the logic of rational choice for explaining church behavior. And all three clarify the importance of choice and agency (rather than structural determinism) in shaping religious outcomes. The reliance on sophisticated, puzzle-driven, structured comparison to explore their proposed hypotheses makes their work especially persuasive.

What are the shortcomings of this approach? Three concerns come to mind.

The first concern touches on the generalizability of the core insight of the religious economy school: that a firm metaphor aptly describes the logic of religious institutions and that rational calculation of organizational interest trumps ideological prescript in determining the behavior of religious institutions. While the religious economy school asserts the utility of this analysis for religious institutions *generally*, it is interesting to note that all three foundational books for this school base their analysis on accounts of *one and the same* religious institution, namely, the Catholic church. Is this mere coincidence? One wonders whether the model would apply equally well to other religious institutions. Clearly certain qualities set the Catholic church apart and these might lead to its exceptional prioritization of organizational concerns and firmlike behavior over fidelity to ideological prescripts. For example, the Catholic church is renowned for its centralization, bureaucratization, and hierarchical organization. The church is thus the "ur-institution" of religious institutions and this might make its leaders more attentive to institutional concerns than is typical. In addition, the Catholic church was the dominant religion (or at least in a very strong position) in each of the historical cases studied. This may have made it exceptionally invested in sustaining the status quo (even at the expense of religious ideals). Finally, Catholicism is a proselytizing religion. This makes it especially concerned about market share.

One wonders whether a less centralized, less bureaucratized, less dominant, more upstart, and/or nonproselytizing religious institution would have the same incentives to be firmlike. Might upstarts like Pentacostals in Latin America be more attentive to religious prescripts in an effort to win adherents with their religious uprightness? Might institutions in religious traditions like Islam that are less centrally organized, less hierarchical, and more internally competitive tend to be more accountable for their ideological fidelity to their followers? Might institutions in religious traditions like Judaism that are not committed to proselytizing tend to be less focused on capturing market share? The reliance on one specific religious institution as evidence for a general theory of religious institutions invites skepticism and requires further investigation to be persuasive.

A second concern touches on the central assumption of the religious economy school: whether the logic of rational choice is appropriate for describing the logic of religious behavior. The concern is not motivated by the belief that human behavior ought to be dichotomized into self-interested versus selfless categories, with religiously motivated behavior rendered immune to rational calculation because it falls into the selfless category. As Gill persuasively argues, even selfless behavior driven by religious motivation (for example, helping the poor, spreading the faith) must be carried out in a world of scarce resources. The faithful "must make tough decisions on how best to achieve those goals" and so must become cost-benefit calculators in their use of scarce resources for selfless ends. The logic of rational choice is therefore useful in helping us understand selfless behavior.[14]

Questioning the utility of rational choice for understanding religious behavior stems not from a selfless versus self-interested dichotomy in human behavior but rather from an instrumental versus expressive dichotomy. A rational choice model is clearly effective in illuminating behavior that is instrumental (that is, goal oriented). But not all religious behavior is instrumental. A good deal of it is purely expressive. For example, it is impossible to understand the bombings of the Twin Towers on September 11 as a solely instrumental act, aimed only at achieving a specific, tangible goal (such as bringing down the U.S. government or ending the U.S.-Saudi alliance). To understand the motivation behind this behavior fully, one must recognize its performative quality, its utility in terms of expressing rage, asserting identity, and validating a sense

[14] See Gill's intervention in the debate regarding his book posted on www.providence.edu/las/discussion.htm (last accessed August 2, 2007).

of empowerment.[15] To the extent that religious behavior is expressive (and clearly not all of it is), then the religious economy approach will not be terribly helpful in explaining it.

A third and final concern raised by the religious economy school concerns a lacuna in the study of religion and politics that it almost by definition cannot address. The approach does not shed light on the question of when and how religious conviction plays a causal role in politics. The primary goal of the religious economy school seems to be to normalize religious institutions, to show that churches are just like any other organization or firm, their behavior ruled by institutional calculations that trump ideological prescripts. In short, the analysis discounts the role of religious ideas and convictions in accounting for the religious behavior it studies. While this approach may be appropriate to solving certain specific empirical puzzles (for example, why did the church ally with some confessional parties in postwar Europe and not with others), it does not imply that religious ideas and convictions never play a role in politics. (I doubt that the authors of the religious economy school would argue the latter point.) But work in this genre does not shed light on this central question. Without diminishing the contributions of religious economy school, this lacuna suggests that much work remains to be done.

Public Opinion Surveys and Large-N Studies

The religious economy school is only the first example of this new trend toward theoretically ambitious and methodologically sophisticated studies of religion and politics in the subfield of comparative politics. A second approach embraces the use of advanced quantitative methods and public opinion data to gain leverage on this subject.[16] Pippa Norris and Ronald Inglehart embark on such a project with their recent book, *Sacred and Secular: Religion and Politics Worldwide*. Their books kicks

[15] For a brilliant account of the expressive side of religiously inspired violence, see Mark Juergensmeyer, *Terror in the Mind of God* (Berkeley: University of California Press, 2003). The importance of expressive as opposed to instrumental behavior is not limited to the realm of religion in politics. This is a well-explored phenomenon in the field of ethnic politics. See, among others, Donald Horowitz, *Ethnic Groups in Conflict* (Berkeley: University of California Press, 1985), especially the references to the work by Henri Tajfel therein.

[16] This work builds on a long tradition of work on culture and politics starting with Gabriel Almond and Sidney Verba and taken up by Ronald Inglehart and the host of Eurobarometer and Afrobarometer studies that followed. See Almond and Verba, *The Civic Culture* (Boston: Little Brown, 1965); and Inglehart, *Modernization and Postmodernization* (Princeton: Princeton University Press, 1997), among his many other works. Note that Mark Tessler has used these methods to good effect to study the dynamic of religion and politics in the Muslim world. See, among others, Tessler, "Islam and Democracy in the Middle East," *Comparative Politics* 34 (April 2002).

off a new series on social theory, religion, and politics published by Cambridge University Press whose self-described ambition is to "offer theoretically grounded, comparative, empirical studies that raise 'big' questions about a timely subject (religion) that has long engaged the best minds in social science."[17]

The central question driving *Sacred and Secular* is this: how do we explain the diverse levels of religiosity found around the world? Empirical reality has delivered a serious blow to secularization theory, the belief that modernization would yield secularization and that, over time, religion would fade in importance, if not disappear altogether.[18] Secularization theory has been challenged not only by the recent resurgence of religion in many less developed countries but, more importantly, by the persistence and salience of religiosity even in advanced industrialized countries such as the United States. Given this extensive variation in the level of religiosity found around the world, is there a parsimonious explanation that can account for it?

To tackle this question, Norris and Inglehart first survey the available stock of hypotheses on religiosity and sort it into two categories: "demand-side theories" and "supply-side theories." Demand-side theories explain the incidence of religiosity by focusing on the human *need* for religion. Classical demand theorists like Weber argued that religion served a key cognitive function for human beings: it provided a sense of order, certainty, and safety in a prescientific world that seemed highly unpredictable and governed by unfathomable forces. Others like Durkheim argued that religion served a more functional purpose: it answered various social needs such as sustaining social solidarity and providing core social services such as schooling, health care, and welfare safety nets. For both theorists, modernization spelled the erosion of religion.[19] For Weber, the progress of science and rationality meant the demystification and mastery of the natural world; this, in turn would undermine superstition, metaphysical beliefs, religious conviction, and

[17] Quote taken from the frontispiece of *Sacred and Secular*. The Cambridge series is edited by David Leege and Kenneth Wald.

[18] The current consensus among the sociologists of religion seems to be that modernity is not antithetical to religious piety and that modernization does not spell secularization. See, for example, Shlomo Eisenstadt, "Multiple Modernities," *Daedalus* 129, no. 1 (2000); and Peter Katzenstein, introductory chapter, in Timothy Byrnes and Peter Katzenstein, eds., *Religion in an Expanding Europe* (New York: Cambridge University Press, 2006). For an especially spirited refutation of the secularization thesis, see Rodney Stark, "Secularization, R.I.P.," *Sociology of Religion* 60, no. 3 (1999). Norris and Inglehart revive the secularization thesis by demonstrating that religiosity is negatively correlated with level of economic development.

[19] Modernization is defined here to mean the twin processes of material development (industrialization and urbanization) and intellectual development (the advance of science and rationality in intellectual inquiry).

practice (pp. 7–8). For Durkheim, the growth of the modern state, with its provision of schools and welfare safety nets, meant that religious institutions would be "stripped of their core social purpose"; this in turn would spell their wasting away (p. 9).

The problem with this approach is that religious devotion has not uniformly declined with the spread of rationality and the growth of the modern state. The persistence of religious devotion in advanced industrialized countries, especially in countries such as the U.S., Italy, and Ireland, poses a significant dilemma for classic demand-side theories.

Norris and Inglehart then turn to supply-side theories.[20] This approach focuses not on the human demand for religion but rather on its *supply*. According to supply-side theorists, the vitality of religious engagement is a function of religious pluralism, competition, and freedom. Vigorous competition among religious denominations forces religious leaders and organizations to be attentive to their congregants if they are to retain market share. Competition and freedom in the religious marketplace spell religious energy and creativity and ultimately parishioners' engagement. By contrast, where a single religious denomination prevails (and, worse still, where that religion is the beneficiary of state subsidies and official establishment), religious organizations and leaders become complacent and moribund; the lack of competition "stultif[ies] ecclesastical life in the same way that state-owned industries, corporate monopolies, and business cartels are believed to generate inefficiencies, structural rigidities, and lack of innovation in the economic market" (p. 12). Religious engagement declines.

Thus, supply-siders argue, the competitiveness of the religious market shapes the vitality of religion. Furthermore, it is key to solving one of the enduring puzzles in the field: the simultaneous robustness of religion in the United States and its relative enervation in much of Western Europe. In the U.S., supply-siders point out, religious pluralism and disestablishment prevail; this spells the vitality of religion. In much of Western Europe religious establishment and more limited religious pluralism is the rule; this spells the debilitation of religion.

But while this analysis seems plausible when limited to the cases of U.S. and Western Europe, it proves less persuasive when the scope of the empirical lens is widened geographically. A metareview of more than twenty-four empirical studies fails to find a positive relationship between religious pluralism and religious engagement in more than a few cases.[21] In postcommunist Europe religious pluralism and religious

[20] This approach is also referred to as "the religious market theory."
[21] The study, cited by Norris and Inglehart on p. 13, was carried out by Mark Chaves and Philip Gorski.

freedom are *negatively* correlated with the level of religious engagement (p. 24). Even in Western Europe anomalous cases can be found in Ireland and Italy, where religious devotion remains (paradoxically) the strongest in the region despite the near monopoly of the Catholic church on religious life. Additional anomalies exist in Poland, Colombia, Venezuela, Brazil, El Salvador, and many Muslim-majority countries where religious engagement is very strong despite the monochromatic nature of the religious landscape. Clearly, the supply of religious competition alone cannot account for the variation in religiosity around the world.

To account for variation, Norris and Inglehart propose an alternative explanation that they call "the existential security hypothesis." Religious enthusiasm, they argue, is driven by the human need for security, safety, and predictability. Where the individual (or community) feels itself subject to existential threat (for example, premature death from hunger, disaster, or inadequate access to the basic conditions of survival such as potable water, adequate health care, and political stability), he/she is more likely to experience the sort of significant stress that propels one into the arms of religion. Norris and Inglehart write:

> Individuals experiencing stress have a need for rigid, predictable rules. They need to be sure of what is going to happen because they are in danger—their margin for error is slender and they need maximum predictability. . . . [They need] the absolute and rigidly predictable rules that religious sanctions provide. (p. 19)[22]

By contrast, individuals enjoying more existential security "can tolerate more ambiguity. . . . An increasing sense of safety brings a diminishing need for absolute rules" (p. 19). In other words, the human demand for religion is not constant (as supply-siders would have it). Furthermore, the demand for religion varies in quite predictable fashion, rising and falling with the prevalent sense of existential security. This is why modernization *is*, in very broad fashion, positively associated with a rise in secularization. It is not so much that the spread of Weberian rationality or the growth of the modern state leads to the decline of religion. Rather, it is the fact that economic development delivers existential security—"the feeling that survival is secure enough to be taken for granted" (p. 4). This is what diminishes the pull of religion.

[22] They continue: "Virtually all of the world's major religious cultures provide reassurance that, . . . the universe follows a plan . . . if you follow the rules, everything will turn out well, in this world or the next. This belief reduces stress, enabling people to shut out anxiety and focus on coping with their immediate problems" (p. 19).

Lending credence to the existential security hypothesis is the fact that religiosity persists most strongly among vulnerable populations in poor countries where people routinely face threats to their survival. Conversely, religiosity has declined most dramatically among the prosperous populations living in affluent and secure postindustrial nations. The persuasiveness of this refined demand-side hypothesis is further enhanced by the fact that it can account for the puzzle of U.S. exceptionalism, that is, the exceptional robustness of religion in the U.S. as compared with its relative frailty in most other wealthy, postindustrial societies. Norris and Inglehart argue that religion remains far more robust in the U.S. than, say, in Western Europe, because existential security dilemmas remain much more prevalent in American society. The limited provision of welfare safety nets, the insecurity of employment, and the greater level of economic inequality in the U.S. (at least when compared with the welfare states of Western Europe) lead to the persistence of sizable populations that feel highly vulnerable to existential risk. This, they argue, is one of the leading factors fueling the relative vitality of religion in the U.S.[23]

To support their existential security hypothesis, Norris and Inglehart enlist a wealth of public-opinion data measuring religiosity in eighty countries, rich and poor, embracing all religious traditions. Then, drawing on macrolevel economic data for these countries, the authors test for correlation between composite human development indicators (their well-defended proxy for existential security) and religiosity. The authors supplement simple correlational analysis with multivariate analysis (OLS regression), sectoral comparisons (testing for correlation between religiosity and vulnerability among each society's more vulnerable segments: the poor, the elderly, women, and so on), and the analysis of time-series data on religiosity where it was available. In short, the authors enlist a host of sophisticated statistical methods, as well as an enviable store of cross-national data to test (and ultimately find strong correlational support) for their hypothesis.

The strengths of this book are many. Norris and Inglehart take on big questions of real-world importance. They suggest insightful, plausible, parsimonious hypotheses to account for them. They consider rival hypotheses and present them fairly and concisely. They test hypotheses in rigorous fashion, applying sophisticated statistical techniques to a

[23] Norris and Inglehart also identify other factors that contribute to U.S. religiosity, including high levels of immigration from Latin American and Asian countries where religious devotion remains strong. But their primary explanation focuses on the increased vulnerability many Americans feel with regard to their existential security as compared with their West European counterparts.

wealth of cross-national data. They acknowledge some of the intrinsic weaknesses of their approach (see below) and attempt to compensate by multiplying their tests. Overall, the text is beautifully written, a model of clarity, theoretically engaging and satisfying. On the strength of its introductory chapter alone, which sets up the historical debate over secularization with brilliant concision, the book is must reading for all students interested in the politics and sociology of religion.

Still, there are substantive questions that *Sacred and Secular* leaves unanswered, as well as certain methodological problems that are intrinsic to any large-N study reliant on public-opinion data. As the authors themselves acknowledge, correlational analysis, no matter how robust, cannot establish causality alone. "Proving" the existential security hypotheses through statistical correlation is problematic. The authors compensate for this by multiplying their tests, conducting sectoral analyses, and carrying out time-series analyses wherever possible. Still, to substantiate the causal mechanism they propose, this large-N study would be well supplemented by some in-depth, qualitative analysis of a few cases that lend themselves to more detailed process tracing. In addition, exclusive reliance on public opinion data to measure religiosity always raises questions about the data's reliability and validity. As the authors themselves acknowledge: "Minor changes in fieldwork practices, sampling procedures, or even question order in the survey will probably swamp the effect of long-term trends" (p. 35). Their argument would be made more persuasive if public-opinion data were supplemented with some other tangible measures of religiosity, though it is admittedly difficult to develop measures that would be cross-culturally valid.[24]

But more substantively, two questions central to Norris and Inglehart's subject remain unanswered. While *Sacred and Secular* provides a plausible account for the variation in religiosity found where existential security varies, it cannot account for the variation in religious engagement found where existential insecurity is much more constant, for example, in much of the less developed world. Why, for example, is religiosity high in much of the Middle East but not in much of East Asia? Is it plausible to argue that the lower level of religiosity found in Chinese society is due to the fact that the average Chinese citizen enjoys more existential security than the average Tunisian or Saudi? Do not politics and cultural tradition come into play? And, temporally speaking, why was religious engagement so much more strident in, say, Egypt in the 1980s and 1990s than it had been in the 1950s and 1960s? Has existen-

[24] For example, measures of attendance at houses of worship would undercount religiosity in religious traditions that do not emphasize collective prayer.

tial security for most Egyptians substantially declined over this time? Or have other political and cultural factors proved important?

Beyond the question of religious variation in the less developed world, what about religious outliers in the postindustrial countries? The existential security hypothesis can account for the case of U.S. exceptionalism, but does it explain why religiosity remains strong in Italy and Ireland (where presumably existential security is more in line with the West European norm)?

Clearly, one theory and one manuscript cannot explain everything. But these questions suggest fruitful avenues for future research. Methodologically this calls for studies that supplement large-N statistical work with qualitative case studies that are better able to trace the causal mechanisms underlying the proposed hypotheses. Substantively, this means studies that group units culturally, regionally, or temporally, to determine whether these contextual factors, rather than just the level of economic development/existential security, might be important in accounting for levels of religiosity. One further item on the agenda for future research: students of religiosity like Norris and Inglehart should be encouraged to take their question into more explicitly political terrain. While the incidence of religiosity is clearly an important phenomenon, the question seems of greater interest to sociologists than to political scientists.[25] However, were the political implications of this issue explored (for example, concerning the relation between the vitality of religion and the *political salience* of religion),[26] the work would find a larger audience in the political science community.

Comparative Historical Study of Religion and Politics

A third approach to exploring the dynamics of religion and politics is represented by Joel Fetzer and Christopher Soper's new book, *Muslims and the State in Britain, France, and Germany*. In this book, the second in the new Cambridge series, Fetzer and Soper rely on on structured comparison and comparative historical analysis to shed light on the puzzle of religious tolerance. Fetzer and Soper ask: how do we account for the variation in state accommodation of Muslim religious practice

[25] Supporting this is the fact that the vast majority of scholarly studies cited by Norris and Inglehart are published in sociological venues such as *The American Sociological Review*, *The British Journal of Sociology*, and *The American Journal of Sociology*.

[26] The relationship between the vitality of religion and its political salience is by no means simple. For example, in much of the Middle East, the incidence of religious belief in society has remained relatively constant over the past century but the political *salience* of religion has skyrocketed over the past thirty years.

in Western Europe? Specifically, why does the British state prove to be relatively accommodating toward its Muslim minority, funding Islamic schools and permitting girls to wear the hijab to class, while the French state proves relatively hostile to such accommodation, and the German state falls somewhere in between?

Fetzer and Soper consider four rival hypotheses to account for this variation. First, the *resource mobilization hypothesis* argues that state accommodation of Muslim religious practice is a function of the collective organizational power that Muslims can mobilize to defend their preferences. Factors such as the community's endowment with skilled leaders, its degree of communal unity, and its members' economic and educational stature all shape the community's capacity for collective action and exaction of state accommodation. Second, the *political opportunity structure hypothesis* argues that state accommodation of Muslim religious practice is a function of regime characteristics such as the nature of the electoral process, the structure of government (federal or unitary), and the relationship between the different branches of government (checks and balances between the judiciary, legislature, and executive). These structural factors define the channels of political access and shape the impact groups can have in shaping policy. Third, the *preexisting ideology hypothesis* argues that the state's historical commitment to liberalism, republicanism, pragmatism, and the like plays a central role in shaping the likelihood that it will accommodate its Muslim community. Fourth, the *church-state legacy hypothesis* argues that the history and institutional structures of church-state relations in a country play the central role in shaping the degree to which the state accommodates Muslim practice. This legacy provides varying institutional and ideological resources that Muslims may engage and defines the parameters of debate, ambition, and strategies for political action. Emphasizing the importance of the church-state legacy is Fetzer and Soper's original contribution to this debate.

To test these four hypotheses, Fetzer and Soper compare three country cases of state accommodation: Britain, France, and Germany. The three vary in terms of their accommodation of Muslim religious practice (the dependent variable), with Britain being most accommodating, France the least, and Germany falling somewhere in between. The three also vary in terms of their endowment with the four rival independent variables. Testing for correlation between these variables, the authors find support primarily for the church-state legacy hypothesis and, to a lesser degree, the political opportunity hypothesis.

They reject the resource mobilization hypothesis, however, because they find that Britain and France do not vary much in the collective political power of their respective Muslim communities. If anything, France, with its larger Muslim demographic and generous extension of citizenship to Muslim immigrants, should have a more powerful Muslim community than Britain, and certainly Germany. But France proves much less accommodating of its Muslim community than either Britain or Germany.

Similarly, the authors reject the preexisting ideology hypothesis not because they believe ideas are unimportant but rather because preexisting state ideologies are heterogeneous and malleable. In Germany, for example, a blood-based (jus sanguinis) notion of nationhood and citizenship prevailed for years, excluding immigrants (including Muslim immigrants) from full political membership in the country. But by 1999, appeals to other ideals in the German political tradition (equality, the rejection of racism) led to reform of the citizenship code to allow for greater inclusion of immigrants. In short, preexisting ideology did not determine the level of Muslim empowerment (and ultimately, accommodation) in decisive, uniform fashion.

More compelling for the two authors are the remaining two hypotheses. The authors admit that political opportunity structure is important in determining the level of Muslim accommodation across countries. For example, the fact that Britain had a decentralized educational system, with decisions about curriculum and school dress made at the level of the local school board (rather than centrally, in London) means that local concentrations of Muslims (for example, in Bradford) can mobilize and win policy victories that would be impossible to achieve at the national level.[27] By contrast, the more centralized nature of government in France makes it more difficult for the Muslim community to win policy concessions because this requires winning over political institutions at the national level, a challenging prospect for any minority.

Besides acknowledging the importance of the political opportunity structure, the authors also insist that a country's prior church-state legacy is key to explaining variations in religious accommodation. The fact that Britain never disestablished religion and always tolerated (even financed) religious education in the schools opened the door to Muslim demands for religious accommodation under the principle of "equal treatment." By contrast, the fact that France long endorsed a principle of strict separation between religion and state and long pronounced its commitment to "laicité" altered the terms of debate and limited the ability of Muslims to make a case for public recognition of (and support

[27] This argument is persuasively made by Erik Bleich, "From International Ideas to Domestic Politics: Educational Multiculturalism in England and France," *Comparative Politics* 31 (October 1998).

for) their religious practices. Germany, with its constitutional separation of religion and state but its recognition (and financing) of major religious institutions as "public corporations," constitutes a middle ground, with this church-state legacy opening the door to some accommodation of Muslim religious practice, given proper organization of the Muslim community.

In truth, the authors find that their evidence decisively eliminates only one hypothesis, the resource mobilization hypothesis, as explanatory of variation in religious accommodation. The remaining three all enjoy some explanatory power and the authors recommend their concatenation.

The strength of this book lies in its insightful grasp of three country cases and the role that historical legacy plays in shaping religious accommodation. The variation in the historic bargains struck between church and state is clearly central to determining the differential accommodation of religious minorities in modern democracies. This will be eye-opening for many Americans who tend to equate democracy with the strict separation of church and state and who will be surprised to learn that a wide spectrum of religious establishment is consistent with the practice of democracy.[28] It is also important as evidence of the power of path dependence in shaping political outcomes.

Still, there are some shortcomings. In terms of *generating* hypotheses regarding the dynamics of religious accommodation, the historical review of three country cases conducted by Soper and Fetzer is very productive. However, in terms of *testing* their hypotheses, this work falls short. By relying on three cases to test four hypotheses the authors run up against the classic problem of many works of comparative historical analysis, namely, too many variables, too few cases. This might be overcome with the addition of a another chapter briefly surveying a number of other developed democracies to see, at least in general terms, whether the hypotheses hold. Alternatively, the authors might devise quantitative measures of some of these variables to facilitate large-N comparison. Interestingly, the authors do showcase quantitative skills in a chapter appended at the book's end that analyzes European public opinion data on matters of religious accommodation. But this chapter seems almost tacked on, tangential to the book's main purpose (to explore the four hypotheses proposed to explain variation in religious accommodation). To some degree, the addition of this chapter seems like an attempt to meet the current methodological prescription in our field to multiply one's methods and embrace both quantitative and qualitative

[28] See Monsma and Soper (fn. 3).

methods in a single research project. But while the authors show facility with both kinds of methods, simply showcasing them side by side in one book does not make for triangulation.

One final observation: although the topicality of Sopher and Fetzer's book guarantees it a broad readership, the authors might have enhanced the impact of their research on the political science community by explicitly reflecting on its broader theoretical implications. This work is important not only for what it says about the dynamics governing the accommodation of religious minorities in modern democracies. It is also important for its implications regarding more general debates in the field concerning the consequence of path dependence and the weight of institutional legacies in shaping contemporary political outcomes. Why not elaborate these theoretical implications further?

Bringing Religion into International Politics

Like comparative politics, the subfield of international relations has also seen a spike in interest in the role of religion, although the impetus behind this development, as well as its outcome, is very different, if not reversed. The mainstreaming of religion in IR has been driven by empirical events, not by methodological advancement. Episodes like September 11 demonstrated the power of transnational religious forces but they took most IR theorists by surprise and could not be accounted for by classic IR theories. The disconnect between theory and experience mandated, in the eyes of many scholars, theoretical catch-up.

The outcome has been a host of books designed to "bring religion back into international relations." These books bemoan the "exile" of religion from IR,[29] insist that "religion matters," and call for a "paradigm shift" that acknowledges the centrality of religion in international affairs. In contrast to the research of comparativists, much of this work is characterized by majestic ambition, announcing the inauguration of grand theory but largely eschewing middle-level theorizing or empirically driven puzzle solving. This spells a problem for the accumulation of knowledge, but of a different sort from that discussed above.

Typical among this genre is Jonathan Fox's and Shmuel Sandler's book *Bringing Religion into International Relations*. This is one of several new books put out by the Palgrave Macmillan series on Culture and Religion in International Relations.[30] In their introduction, Fox and Sandler argue that "international relations cannot be understood

[29] Hence the title of the book edited by Hatzopoulos and Petito (fn. 3).
[30] The series is edited by Yosef Lapid and Friedrich Kratochwil.

without taking religion into account" (p. 7) and that the field is "in need of a new paradigm that would include religion as a major explanation for international relations" (p. 1). The authors point to a number of phenomena that, they argue, the classic realist paradigm either ignores or cannot account for. These include transnational religious conflicts, transnational religious forces such as religious terrorism and religious fundamentalism, and the power of religious norms in defining foreign policy. Realism falls short because it assumes that all states are driven by the same motivations and goals (the desire to accumulate power and wealth) and ignores the fact that states are often driven by normative considerations, including religious ones, when making foreign policy. Realism also fall short because it characterizes the international system as fully chaotic and self-help in nature, failing to acknowledge the role that norms and international institutions, including religious ones, play in reducing this chaos. To correct for these lacunae, IR scholars must bring religion back in.

The foremost problem with *Bringing Religion into International Relations* is that it focuses on making an argument that few commonsensical people would dispute, namely, that ideas and identity (with religious ideas and religious identity as a subset) play a role in international affairs. In fact a very large literature explores the role played by religion and religious institutions in the international arena. This literature studies such questions as the role of religion as a source of violent conflict and war,[31] the role of religion and religious institutions in conflict resolution,[32] and the role of religion as a source of international norms.[33]

The problem is not that the question of religion has been *overlooked* in international affairs[34] so much as that it has been *undertheorized*. A good deal of this literature is the work of historians, sociologists, and theologians. And while these studies are analytically rich and insightful, they are also largely idiographic rather than nomothetic in ambition.

[31] James Wellman, Jr., ed., *Belief and Bloodshed: Religion and Violence across Time and Tradition* (Lanham, Md: Rowman and Littlefield, 2007); Mark Juergensmeyer, *Terror in the Mind of God* (Berkeley: University of Californial Press, 2003) ; Jessica Stern, *Terror in the Name of God* (New York: Harper Collins, 2003); Oliver McTernan, *Violence in God's Name* (Maryknoll, N.Y.: Orbis, 2003).

[32] R. Scott Appleby, *The Ambivalence of the Sacred: Religion, Violence, and Reconciliation* (New York: Rowman and Littlefield, 2000); Douglas Johnston, *Faith-Based Diplomacy: Trumping Realpolitik* (New York: Oxford University Press, 2003); Douglas Johnston and Cynthia Sampson, eds., *Religion: The Missing Dimension of Statecraft* (New York: Oxford University Press, 1994).

[33] John Witte, Jr., and Johan D. Van der Vyver, *Religious Human Rights in Global Perspective* (The Hague: Martinus Nijhoff, 1996); James Turner Johnson, *Just War Tradition and the Restraint of War* (Princeton: Princeton University Press, 1981).

[34] Jonathan Fox, "Religion as an Overlooked Element of International Relations," *International Studies Review* 3 (Autumn 2001).

They offer excellent case studies of transnational religious institutions brokering peace or religious fundamentalist movements embracing violence. But they rarely undertake the kind of structured comparison that a political scientist would embrace—a comparison that can yield generalizable hypotheses about when religious difference is likely to spell transnational conflict or about which conditions foster the transnational contagion of religious terror.

If structured comparison is warranted, Fox and Sandler do not provide it. Their forays into empiricism are largely limited to a series of descriptive accounts of religious norms influencing foreign policy decisions, religious conflicts gone international, and transnational religious phenomena (fundamentalist movements; religiously inspired human rights movements). Their objectives are twofold: to prove that "religion matters" in IR and to argue that a major paradigm shift is consequently imperative in the study of international affairs.

But is a paradigm shift truly warranted by recognition of religion's importance in international affairs? Fox and Sandler's argument rests on two observations. First, classic studies in IR subscribe to a materialistic determinism. Material interest—the quest for wealth and power—drives international affairs. Nonmaterialistic motivations such as ideas, culture, and religion are discounted as epiphenomenal and denied an independent causal role. Second, classic studies in IR perceive the international system as fully anarchic and do not acknowledge the role that norms and institutions, especially religious ones, may play in reducing this anarchy.

Both of these observations are subject to question. First, not all mainstream studies in IR discount the role of ideas in international politics. As Michael Desch[35] points out, the field of international security has long given prominence to the role of cultural variables in shaping the foreign policy choices of states.[36] And while classical realists in security studies would resist the argument that cultural variables can *trump* material interest and structural imperatives in determining foreign policy, they do not argue that cultural variables are irrelevant or that material interest and structural imperatives explain everything. Rather, as Desch writes, "states have a hierarchy of interests: security at the top, but then economic welfare, ideological and humanitarian concerns in descending order."[37] Ideologically driven foreign policy is consistent with realism so long as this policy does not undermine a state's vital security or economic

[35] Desch, "Culture Clash: Assessing the Importance of Ideas in Security Studies," *International Security* 23 (Summer 1998).

[36] See, for example, Peter Katzenstein, ed., *The Culture of National Security: Norms and Identity in World Politics* (New York: Columbia University Press, 1996).

[37] Desch (fn. 35), 160.

interests.[38] In short, for realists, cultural variables (like religion) may exert causal power under conditions that are "structurally indeterminate," that is, where structural and material concerns are "permissive."[39] Desch cites Michael Barnett's work on alliances in the Middle East as an example. In that region, identity politics factor into a state's choice of ally "given the absence of an immediate threat."[40]

In this way, a realist model can integrate a causal role for ideational forces like religion in international politics. To make the case for a paradigm shift, Fox and Sandler must go beyond arguing that ideas (and specifically religion) matter. Rather, they must argue that ideas (and religion) systematically trump material interest in the making of international politics. This is a radical stance they seem unlikely to take.[41]

As for the second observation, that classical approaches to IR ignore the role of norms and institutions in reducing the anarchy of the international system, this seems odd given that the entire neoliberal critique of realism has focused on the role that norms, regimes, and institutions have played in reducing anarchy and fostering cooperation in the international arena. Admittedly, liberals pay little attention to the role of religious norms and institutions on the international scene. But does this neglect merit a "paradigm shift"? Are religious norms and institutions in some way distinct from nonreligious ones? If Fox and Sandler believe so, they must make an explicit argument clarifying the sui generis nature of religious norms and institutions. Without it, why would a paradigm shift be necessary?

The bottom line is that the authors need to engage this literature—realist studies that integrate ideas into international politics, liberal studies that focus on norms and institutions, and even constructivist studies that emphasize the role of identity politics in the international arena —before they dismiss all of IR as a realist caricature. But better yet, why not abandon the endless prosecution of "paradigm wars,"[42] move beyond commonsense observations that *religion matters* in international politics, and explore instead the truly challenging theoretical question:

[38] This is a paraphrase of Desch (fn. 35), 160.

[39] A similar argument is made by Craig Parsons, who shows that ideas can have independent causal impact in international affairs in contexts that are structurally indeterminate. See Parsons, "Showing Ideas as Causes," *International Organization* 56 (Winter 2002).

[40] Barnett, "Identity and Alliances in the Middle East," in Katzenstein (fn. 36), cited in Desch (fn. 35),163. For some other interesting discussion of the interplay of ideas and interest in shaping international politics, see Daniel Philpott, "The Religious Roots of Modern International Relations," *World Politics* 52 (January 2000); and Daniel Nexon, "Religion, European Identity, and Political Contention in Historical Perspective," in Byrnes and Katzenstein (fn. 18).

[41] By their own admission: "(R)eligion is not the main driving force behind international relations" (p. 7).

[42] See Finnemore and Sikkink for more on the paradigm wars of IR and the contrast between this subfield's dynamics with that of comparative politics (fn. 1, 404).

when does religion matter and *how*? Under what conditions, for example, does religion trump realpolitik in the making of foreign policy? When do religious movements prove contagious transnationally? Why does some religious conflict spell war and not others? Tackling these sorts of questions will go far toward advancing the accumulation of knowledge in IR. But to answer these questions requires midlevel theorizing, grounded in real-world empirical puzzles. The call for empirical spadework of this sort has long been sounded,[43] and some IR scholars not focused on religious variables have suggested ways to pursue this.[44] Now it is time to take up the challenge.

Perhaps the most provocative argument in defense of a paradigm shift in IR is presented by Scott Thomas in his new book, *The Global Resurgence of Religion and the Transformation of International Relations*. Like Fox and Sandler, Thomas rejects much of classic IR theory for its materialist assumptions and its tendency to view ideational factors as epiphenomenal and lacking in independent causal power. But in contrast to his colleagues, Thomas insists that religion has a sui generis quality to it and ought not to be "misspecified as a variable" or "reduced to one of a variety of ideas or ideologies or belief systems that have an impact on international life"(pp. 69, 76). Consequently, he argues that the IR paradigm cannot be remedied simply by acknowledging the causal capacity of ideas and by "bringing religion back in as part of a wider effort to bring ideas, values, and ideational factors back into the study of IR" (p. 69).

What is so unique about religion? Religion, Thomas argues, is not just "a body of beliefs," it is a "community of believers" (p. 24). This has several implications. First, religion is a marker of social identity, and as a result battles over religion are not simply battles over theological ideas (how many angels can dance on the head of a pin?) but rather are battles over the integrity of one's community (pp. 23–24). Second, religion plays a constitutive role in defining one's identity and is in some ways prior to interest (and any rational calculation thereof). "Interest depends on some conception of an individual's collective identity" (p. 91),[45]

[43] See Judith Goldstein and Robert Keohane, *Ideas and Foreign Policy: Beliefs, Institutions, and Political Change* (Ithaca, N.Y.: Cornell University Press, 1993), 6. Surprisingly, this same imperative was sounded by one of the editors of the Palgrave Macmillan series more than ten years ago. See Yosef Lapid, "Culture's Ship: Returns and Departures in International Relations Theory," in Yosef Lapid and Friedrich Kratochwil, eds., *The Return of Culture and Identity in IR* (Boulder, Colo.: Lynne Rienner, 1996), 9.

[44] See Parsons (fn. 39); see also Desch (fn. 35).

[45] The classic example of identity defining interest (though not specifically cited by Thomas) is the case of working-class citizens in Europe whose identity as members of a national group (German, French) defined their sense of interest and led them to fight alongside their conationals rather than unite as proletarians and resist the call to war.

and consequently, "religious ideas don't simply express interest, they constitute them" (p. 12). Third, religion is not simply a "moral statement to which rational autonomous individuals give their intellectual consent." It is part of a "cognitive script . . . [that] people internalize . . . not out of conscious choice . . . [and] in ways that can override rational choice or utility-maximizing behavior" (p. 95).[46]

What do these qualities mean for religion as a force in IR? Religion is not subject to the same sort of rational assessment as other ideas in the international arena (say, the importance of environmental protection, the need for world peace) and thus cannot be adopted or discarded on the basis of its instrumental utility alone. In addition, because religion is identified with the integrity of one's community, it is harder to compromise than are other ideational forces in the international system. (Can one split the difference over the integrity of one's community in the same way one can split the difference over competing visions of economic cooperation?) Finally, because understanding the role of religion in international politics requires grasping its *meaning* for believers, Thomas argues that a positivist approach to the study of religion in politics is precluded. In a "conscious world of human beings with intention and meaning," it may be inappropriate to assume that events are "governed by general laws, patterns, and regularities like the natural world/physical world." In short, *understanding* precludes *explanation.* Recognizing the role of meaning and conscious intent in human affairs precludes the goal of discovering lawlike regularities with predictive power that can be discovered and tested. At best, students of religion in IR should aim for interpretive narratives not predictive science.

Thomas's arguments are provocative but not altogether convincing. First, religion may be different from some ideas in terms of its susceptibility to rational calculation, but it is not entirely sui generis as an ideational force in world politics. Ethnicity, nationalism, and other such drivers of identity politics are similar to religion in that they are markers of social identity, play a role in constituting interest, and confer meaning on human endeavor. But are the politics of ethnicity and nationalism entirely immune to rational calculation, instrumentality, and even compromise? A huge literature has emerged tracing the fluidity of ethnic identification, its redefinition according to situational context, calculation of instrumental utility, and availability of effective entrepreneurs. Why should religion be a less fluid force? There are countless historical examples of the reinterpretation of religious doctrine and the

[46] As the source of this insight, Thomas cites Martha Finnemore, *National Interests in International Society* (Ithaca, N.Y.: Cornell University Press, 1996).

redefinition of religious boundaries in line with changing political, economic, and sociological conditions. Why not subject this to systematic analysis?

And what of the claim that understanding precludes explanation, that recognizing the importance of meaning in human affairs precludes causal models and positivist ambitions in favor of more interpretative approaches? This is a weighty epistemological debate far beyond the competence of this reviewer. Suffice it to say that some like Yee[47] make compelling arguments along these lines. But many others, including leading constructivists, embrace a positivist model in their conduct of research.[48] As Checkel points out, constructivists need not "reject science or causal explanation. Their quarrel with mainstream theories is ontological not epistemological."[49] Recognizing the importance of meaning does not preclude regularities in human affairs. These may be subject to identification and testing. The resulting insights might even confer some predictive power, albeit more modest and probabilistic than would be the case for regularities in the physical world.

Overall, it seems a shame to postpone empirical investigations of real-world puzzles until these huge epistemological debates are resolved. Fox, Handler, and Thomas have indeed focused attention on areas that have been relatively undertheorized in IR scholarship until now. Why not borrow from the comparativist tool kit and embrace structured comparison to gain leverage on the real-world empirical puzzles that dominate the headlines and affirm the important role played by religion in contemporary international affairs?

LOOKING TO THE FUTURE

The spate of new books on religion and politics has prompted useful reengagement with the question of religion's role in comparative politics and international affairs. On the comparative front, increasing methodological sophistication has provided answers to a host of empirical puzzles ranging from the church's variable support for democracy to the differential levels of religiosity found around the world. It has led to important insights about the rationality of religious institutions, the role of choice and agency in religious affairs, and the power of path dependence in shaping religious tolerance. In international relations,

[47] Albert Yee, "The Causal Effect of Ideas on Policies," *International Organization* 50, no. 1 (1996).

[48] See Finnemore and Sikkink (fn. 1).

[49] Checkel (fn. 1).

current affairs have encouraged scholars to rededicate themselves to exploring the role of norms and identity (including religious ones) in the conduct of international politics, hammering the final nail in the coffin of approaches that are exclusively statecentric and materially driven. But despite these advances, some major challenges remain.

Two concerns stand out in the comparative subfield. First, the failure of the subfield to coalesce around a common question or overarching agenda constitutes a problem for the accumulation of knowledge. Studies of religion and politics in the subfield frequently talk past one another and rarely collaborate to build ambitious theoretical stands.[50] To solve this problem students of religion and politics ought to take their work to a higher level of abstraction, that is, elaborate further on the relevance of their findings to larger theoretical debates in the field. Kalyvas, Gill, and Warner achieve some success on this front, by expressly linking their results to such themes as the relationship between ideas and interests, the role of contingency in politics, and the role of agency versus structure in shaping politics. This is less developed in the Norris/Inglehart and Fetzer/Soper books, whose appeal lies more in the intrinsic interest of their motivating puzzles. By self-consciously elaborating the linkage between specific research results and larger theoretical debates, the contribution of this work to the field's accumulation of knowledge will be made more apparent, accessible, and consumable.

Second, it seems surprising that none of the comparative works cited here explicitly explore the question of religion's power as an independent variable in politics.[51] For most of these authors religious belief is either the dependent variable to be explained by socioeconomic conditions (Norris/Inglehart), or it is an incidental factor, largely overruled by rational calculation of an institution's needs (Kalyvas, Gill, and Warner). But what about religion's power as an independent variable to shape events, interests, and identities? And how might the study of religion answer the questions that grip so many students of ideas in politics. How do ideas change? And when do ideas resonate and become

[50] This disjointedness also constitutes a problem "from the point of view of academic marketing and disciplinary recognition" (a problem the subfield shares with other puzzle-driven segments of comparative politics) that undermines the goal of raising the subfield's profile and increasing its impact on the discipline as a whole. See Paul Pierson and Theda Skocpol, "Historical Institutionalism in Contemporary Political Science," in Ira Katznelson and Helen Milner, eds., *Political Science: State of the Discipline* (New York: W. W. Norton, 2002), 697.

[51] Several sociologists and historians, including Casanova (fn. 2) and Juergensmeyer (fn. 31), have written insightful accounts of how religion has shaped public life, but their work is primarily ideographic rather than nomothetic; it does not engage in the sort of structured comparison that a comparativist would employ to generate and test hypotheses. See also Gabriel Almond, R. Scott Appleby, and Emmanuel Sivan, *Strong Religion: The Rise of Fundamentalisms around the World* (Chicago: University of Chicago Press, 2003).

politically significant?[52] Explicit reflection in this area would tap into the lively debate spun off by the recent "ideational turn" in comparative politics.[53] Religion, as one subset of ideas, provides rich terrain for exploring these metaquestions.

These are not navel-gazing theoretical queries. Their significance for real-world political dilemmas is apparent. In the Middle East and the Muslim world, where Islamists are challenging the political, cultural, economic, and sexual status quo, the question of which interpretation of a religious tradition predominates, when and why, is of paramount political concern. The politics of ideational change and learning in that region is central to questions of democratic transition, gender equality, and intercivilizational peace.[54] Similarly, in South Asia, where religious identity has reshaped party politics, and in Latin America, where church doctrine has mobilized as well as demobilized crucial social movements, the changing interpretation of religious teachings has had serious political consequence. Exploring the real-world puzzle of change and stasis in religious ideology allows us to address these issues. To do so effectively we must acknowledge the Janus-faced nature of religion, steer a careful course between the Scylla of essentialism and the Charybdis of epiphenomenalism, and acknowledge religion's dynamic capacity to act simultaneously as both independent variable and dependent variable.[55] By taking on these theoretical (and policy-relevant) questions in a self-conscious way, there is no question that this literature would command attention, encourage scholarly dialogue, and contribute to the accumulation of knowledge.

In international relations, in contrast to comparative politics, the primary challenge lies not in scaling up the subfield's theoretical ambition but rather in scaling it down. IR scholars interested in exploring the impact of religion on international affairs need to focus more on developing empirically grounded middle-range theory than on pursuing paradigm wars. By embracing structured comparison and close empirical study, IR scholars will go far in advancing the accumulation

[52] For exposition of these outstanding questions, see Berman (fn.1).

[53] Berman (fn. 1); Blyth (fn. 1); Finnemore and Sikkink (fn. 1).

[54] See Carrie Rosefsky Wickham's innovative work on learning among Muslim activists (Paper presented at Princeton University, Department of Near Eastern Studies, March 2007). For more on how structural factors, both social and institutional, shape religious interpretations, see Jillian Schwedler, *Faith in Moderation: Islamist Parties in Jordan and Yemen* (New York: Cambridge University Press, 2006); and Quintan Wictorowicz, ed., *Islamic Activism: A Social Movement Theory Approach* (Bloomington: Indiana University Press, 2004).

[55] For more on the Janus- faced nature of religion, see Kristin Smith, "From Petrodollars to Islamic Dollars: The Strategic Construction of Islamic Banking in the Arab Gulf" (Ph.D. diss., Harvard University, 2006).

of useful knowledge that addresses real-world puzzles. These include the variable appeal of transnational religious movements, the variable power of religious ideals to trump material interest, and the variable tendency of religious traditions to inspire violence and/or cooperation in the international sphere.

In short, important questions remain, most notably those that focus on when and how religion matters in politics. With an increasing number of well-trained political scientists turning their attention to these issues, we should have faith that the next round of scholarship will deliver illuminating answers to queries on these themes.