

# Critical Dialogue

**How Political Parties Mobilize Religion: Lessons from Mexico and Turkey.** By Luis Felipe Mantilla. Philadelphia: Temple University Press, 2021. 274p. \$110.50 cloth, \$34.95 paper.  
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Religious parties representing many religious denominations are increasingly common around the world. However, studies of parties based in Christian (Anna Grzymala-Busse, *Nations under God*, 2015) and Islamic (Jillian Schwedler, *Faith in Moderation: Islamist Parties in Jordan and Yemen*, 2006; Charles Kurzman and Ijlal Naqvi, “Do Muslims Vote Islamic?” *Journal of Democracy*, 21 [2], 2010) traditions have rarely focused on building common insights regarding their origins, tactics, and successes. Luis Felipe Mantilla’s book attempts to bridge this scholarly gap by focusing on a key question: How and under what conditions do political parties mobilize religion? It answers this question by offering innovative measures of religious mobilization and religious parties, and a theoretical framework, and an empirical analysis that spans Catholic and (Sunni) Islamic religious traditions.

The author begins by defining religious mobilization as the totality of mobilization along three distinct dimensions: religious identity, religious doctrine, and links with religious associations (table 1.1, p. 27). He identifies the specific party choices and practices that indicate low (secondary) and high (primary) levels of mobilization on each dimension. He then combines party mobilization levels across these dimensions, yielding a theoretically rich range of overall religious mobilization. Using this measure to compare religious mobilization in Catholic and Sunni Islamic countries, he finds that parties in Catholic countries engage widely in low levels of religious mobilization, whereas those in Sunni countries mobilize less frequently but at much higher levels (pp. 31–33). Interestingly, electoral success is highest for parties that engage in low levels of religious mobilization in both Catholic- and Sunni-majority countries. In what will be considered the most controversial decision in this book, these mobilization levels are then used as alternative criteria to classify parties as religious or not (table 1.2, p. 28).

Using the most permissive definition, a party is classified as religious if it engages in religious mobilization on at least one dimension at a secondary level. Under the moderately permissive definition, a party is considered religious if it engages in secondary mobilization along all three dimensions or in primary mobilization along at least one dimension (pp. 26–29). Finally, using the strictest definition, a party is classified as religious only if it engages in primary levels of mobilization along all three dimensions. The author argues that using these alternative criteria to classify religious parties allows scholars to test how sensitive theories related to religious parties are to the definition used to identify those parties. Chapter 1 then lays out a theory explaining the conditions under which religious parties come into existence and succeed in elections.

Mantilla argues that two factors—the internal structure of religious communities and the state institutions governing elections and religion—influence religious mobilization levels. When clerics have little authority and are unable to coordinate their actions, lay activists are highly fragmented, and state regulatory and electoral environments are permissive, parties will engage in the highest level of religious mobilization. Conversely, when religious communities are led by clerics with concentrated religious authority, and state regulatory and electoral environments are less permissive, parties will engage in the lowest level of religious mobilization. This is because powerful clerics are less interested in risking control of their religious message and authority by collaborating with political parties operating with different goals, whereas lay activists are motivated to cooperate with parties to increase their own influence in an environment where they must compete with other legitimate religious authorities (pp. 37–40). State regulation of religion matters because it influences what forms religious mobilization can take, while electoral institutions influence how easy or hard it is to translate religious mobilization into political representation. Finally, Mantilla argues that although these factors yield more concrete predictions regarding the presence of religious parties, their predictability regarding electoral success is lower because other factors also mediate electoral success.

To test these arguments, the author analyzes the impact of religious structure and state regulation of religion and

electoral permissiveness on (1) the existence of religious parties in a country in a given year and (2) the total number of legislative seats won by all religious parties in a year in a country, using data from 22 Catholic- and 18 Sunni-majority countries from 1990 to 2012. These tests are done using three alternative sets of religious parties based on their religious mobilization level. Mantilla finds that results vary substantially depending on how one defines religious parties, thus confirming his larger point that the definition of religious parties is itself a crucial choice that influences empirical support for any subsequent theory relating to those parties.

Given the limitations of operationalizing complex concepts such as the internal structure of religious communities and the relative influence of clerics versus lay activists for quantitative analysis, Mantilla conducts a comparative historical study of Mexico (a Catholic-majority country) and Turkey (a Sunni-majority country) to examine the validity of his theory. The next four chapters trace the trajectory of religious mobilization in these two countries over a century, identifying the values that the two explanatory factors—religious community structure and regulatory and electoral institutions—take on at different times and their influence on parties' incentives to select a certain level of religious mobilization. These chapters provide a convincing, in-depth, and persuasive test of the book's theoretical framework. However, they pay less attention to the earlier question raised in the book of whether using alternative definitions of religious parties changes the empirical support for the theory.

The book makes three important contributions. First, it highlights the importance of analyzing the internal dynamics of religious organizations and communities in understanding their effect on political actors, including political parties. Previous research has explained varying levels of the political mobilization of religion by religious parties and organizations as stemming from differences in the modernization levels of societies (the secularization thesis; see David Smith, *Religion and Political Development*, 1970) or in the beliefs and traditions of different religions (the civilizational thesis, see Bernard Lewis, *What Went Wrong?* 2002). Scholars have increasingly emphasized how the extent of state regulation of religion (see Jonathan Fox, *Political Secularism, Religion and the State*, 2015) and the degree of competition between religious organizations (the religious marketplace thesis; see Roger Finke and Rodney Stark, "Religious Choice and Competition" *American Sociological Review*, 63 [5], 2013) affect the incentives of religious and political actors to mobilize religion in the political sphere. The author adds to this scholarship by highlighting how the relative influence of clerics and lay preachers and their fragmentation affect the opportunities other political actors have to leverage religion for political purposes.

Second, this book adds to the ongoing debate about how to appropriately define religious parties. Most scholars currently use the ideological principles that religious and nonreligious parties articulate in their founding documents or election manifestos to classify them programmatically (e.g., Kurzman and Naqvi, "Do Muslims Vote Islamic?"). They then study the conditions that motivate religious (or other) parties to subsequently change their platforms and tactics. This is the approach taken by, for example, the literature on moderation theory, which breaks down subsequent change into ideational and strategic change (for example, see Schwedler *Faith in Moderation*, 2006; Carrie Wickham, *The Muslim Brotherhood: Evolution of an Islamist Movement*, 2013; and Kurzman and Naqvi, "Do Muslims Vote Islamic?"). Party choices to change platforms or form alliances with religious or other associations can result from either ideational and strategic changes or both in parties. Researchers in this camp, including myself, would have liked to see the author present an analysis that used his finely grained measure of religious mobilization as the outcome to be explained. Instead, he uses the time-varying choices of doctrinal and policy positions and alliances with associations to generate a time-varying identification of a party as a religious party. In other words, the author argues that if a party walks the religious walk in terms of mobilizing religion at a certain time, it is a religious party at that moment in time, no matter its founding ideology.

This definition of a de facto religious party raises larger questions about whether changes in policy platforms or alliance tactics accurately reflect a fundamental shift in the nature of a party. The PML-N party in Pakistan personifies this conundrum. It presents itself as a conservative, not religious, party. Yet its actions include supporting a law that would have effectively implemented sharia law in 1998, passing laws protecting women in mixed-gender workplaces in 2010, and criminalizing domestic violence in 2016 (Christophe Jafflot, *Pakistan at the Crossroads*, 2016). The first policy is strongly supported by other religious parties and organizations in Pakistan; the latter two are strongly opposed by them. The question is, do these policies reflect deep changes in the PML-N's nature as a religious or nonreligious party, or do they reflect time-varying tactics that the party adopted because of changing political imperatives? The author classifies this party as a religious party using the moderate level of religious mobilization to define religious parties (p. 28).

Third, and finally, by focusing on changes in the frequency and intensity with which doctrine is emphasized, the frequency of religious policy proposals, and the alliances with religious organizations, Mantilla successfully outlines a strategy that allows scholars to transcend religious traditions in the study of religious mobilization. Given the global ascendance of religious parties, this is a valuable contribution to the toolkit of scholars.

### Response to Vineeta Yadav's Review of *How Political Parties Mobilize Religion: Lessons from Mexico and Turkey*

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— Luis Felipe Mantilla 

Vineeta Yadav provides an excellent commentary on many of the core arguments in my book. I am particularly grateful for her discussion of how my approach to the conceptualization of religious parties differs from that of much of the extant scholarship. She is correct in noting that, rather than ascribe an essential religious (or nonreligious) identity to parties and then observing how their behavior changes over time, I propose that parties can be considered religious if and when they “walk the religious walk,” as she neatly puts it, by mobilizing religion in electoral contests.

My comparative historical research on religion and parties has convinced me that treating “religious” as a fixed and essential attribute of political parties is problematic, not least because the process by which this label gets assigned is often opaque and inconsistent. For example, Kurzman and Naqvi, whose seminal work on Islamic parties arguably sets the gold standard for datasets on the topic, say that they include “all those parties that seek to increase the role of Islam in political life” (“Do Muslims Vote Islamic?” *Journal of Democracy*, 21 [2], 2010, 51). Yet there are many parties that could fall in this rather vague category that are not considered Islamic parties, such as Turkey's MHP or Pakistan's PML-N, probably because they compete locally with other, more explicitly religious organizations.

An exclusive reliance on founding ideology to establish a party's status as religious, as proposed by Yadav, reflects the distinctive trajectory of scholarship on Islamic politics and does not always travel well to other traditions. As my book notes, studies of Catholic politics typically emphasize links to religious organizations, rather than ideology, as the sign of a religious party. Appeals to religious identity—which is conceptually different from having a doctrinally infused ideology—can also mark a party as religious, particularly in more denominationally pluralistic societies. In Muslim-majority contexts, these distinctions may be less critical because parties that mobilize religion along one dimension tend to do so along the other two as well—although, as noted earlier, there are some important exceptions. But they are fundamental if we want to examine religious parties across traditions.

Moreover, why should decades-old documents be determinant of essential religiousness? And what about parties that were founded on nonreligious principles but have become increasingly reliant on religion? These are conceptual issues, but they have practical consequences. My students and colleagues here in the United States often

ask me whether the Republican Party is a religious party. If the question is one about the foundational or essential nature of the party, the conversation rapidly devolves into partisan posturing. In contrast, we can usefully discuss the extent to which the party has mobilized religion through appeals to identity, doctrinal references, and partnerships with religious organizations in civil society.

Finally, I agree with Yadav that in-depth case studies provide a particularly appropriate means for testing my arguments about how and why political parties mobilize religion. I disagree, however, with the notion that the goal of these case studies should be to determine “whether changes in policy platforms or alliance tactics accurately reflect a fundamental shift in the nature of a party.” I suspect that even the most rigorous experimental methods would have trouble deciphering the fundamental beliefs and commitments of politicians. But well-researched case studies can certainly tell us how and under what conditions political parties mobilize religion to compete in elections.

**Religious Parties and the Politics of Civil Liberties.** By Vineeta Yadav. New York: Oxford University Press, 2021. 384p. \$74.00 cloth.

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Does the electoral success of religious parties pose a threat to civil liberties? This is an important and timely question. Focusing on Muslim-majority countries, Vineeta Yadav's *Religious Parties and the Politics of Civil Liberties* proposes a thought-provoking answer: the effect of having religious parties in government depends on the ability of religious organizations (ROs) to operate as effective religious lobbies. Only when these ROs have sufficient resources to put pressure on governments are religious parties consistently willing and able to implement illiberal policy agendas. Yadav further proposes that the strength of ROs is itself a consequence of two prior causal factors: the presence of a sizable and concentrated Islamist bloc in the national legislature and a substantial inflation crisis.

This is an original and engaging argument, which the book presents in an effective and accessible manner. In the process, it weaves together insights from a remarkable range of research areas—from Islamist ideology and civil rights to interest group strategy and the political economy of inflation. The book is even more impressive in its empirical approach, which relies on an array of quantitative and qualitative methods, including sophisticated cross-national regression models, original surveys of political elites, and in-depth case studies. This is particularly notable given the dearth of methodologically sophisticated cross-national research on the causes and consequences of religious party success.

Laudably, the book takes seriously the organizational complexity of religious communities. Many large-N studies implicitly treat national religious communities as unitary actors by studying them only at the aggregate level. Disaggregating religious communities into component organizations that face different incentives and are often in competition with each other, as this book does, can reveal a much richer and more dynamic picture of religious politics in the Muslim world.

This approach is rare because rigorously measuring organizational strength cross-nationally is a daunting task. Here the book makes a series of potentially significant contributions. It defines ROs in Muslim-majority countries as non-governmental, national-level groups, led by prominent figures, and founded to renew Islamic beliefs and practices. It then identifies ROs that fit these criteria in all 49 Muslim-majority countries and measures the extent of their socio-economic institutionalization for every year between 1970 and 2016. It also identifies Islamist parties and measures both their seat concentration in national legislatures and their presence in governments on an annual basis during the same 46-year period. As Yadav notes, only Charles Kurzman and his coauthors (Charles Kurzman and Ijlal Naqvi, “Do Muslims Vote Islamic?” *Journal of Democracy*, 21 [2], 2010; Charles Kurzman and Didem Türkoğlu, “After the Arab Spring: Do Muslims Vote Islamic Now?” *Journal of Democracy* 26 [4], 2015) have previously produced a comprehensive list of Islamist parties, and no one to my knowledge has attempted to measure the cross-national distribution and strength of the kinds of ROs described here.

It is therefore unfortunate that the current edition of the book is missing appendices A.1–3, which list the observed Islamist parties and sources used to generate the scores for parties and organizations, and that there is no table providing summary descriptions of the key variables. One must glean what one can from tantalizing figures and lines of text. Based on those, the data seem to contain some surprising findings. For example, Figure 4.3 (p. 109), which describes the distribution of the Islamist seat concentration variable, suggests that virtually all country-years in the 49-country sample include Islamist parties and that they almost always secure a sizable share of legislative seats. However, because about half the countries listed in the sample have either no Islamist party or only niche organizations that typically fail to capture any seats, the scores for these countries are missing, which presents a rather distorted view of the distribution. Moreover, even in countries where Islamist parties have achieved some success, such as Jordan or Yemen, this success has typically been limited and sporadic, suggesting that the lower end of the seat concentration distribution should be more densely populated than the figure indicates. For the same reasons, the statement that “religious political parties joined and participated in about 34% of ruling coalitions in the sample” (p. 145) seems inconsistent with prior research on Islamist parties.

The book proposes that ROs have fixed institutional and policy interests that they will persistently pursue through partnerships with Islamist parties (pp. 39–55). The benefits of successful partnerships are persuasively presented, but what about the risks? After all, political parties are notoriously unreliable. Indeed, Yadav explicitly assumes that they are very likely to renege on their promises to religious partners for the sake of political expediency (p. 91), something RO leaders almost certainly realize as well. Moreover, political backstabbing, corruption scandals, and secularist reactions all threaten serious harm to ROs that become too entangled with political parties. The fate of Turkey’s Gülen movement, rebranded by its erstwhile party allies as the Fethullahist Terrorist Organization and brutally persecuted, looms large in this context. So does the dramatic suppression of the Muslim Brotherhood in Egypt after its party’s government was toppled in 2013. These are (or were) two of the most institutionalized and powerful ROs in the Muslim world. It seems counterintuitive that smaller, more vulnerable ROs would be as consistently eager to take on these risks for an unreliable payoff, as Yadav suggests.

Perhaps the risks of partisan entanglement appear manageable in the book because it argues that virtually all Islamist parties and organizations share a fundamental commitment to highly orthodox interpretations of sharia and sincerely seek its implementation through state policy (pp. 11–14). If so, then ROs can be confident that their partisan allies at least intend to implement their shared agenda. However, this is a contentious proposition that flattens out important differences among religious parties and organizations in the Muslim world. Although the book describes many instances in which different organizations and parties have advocated illiberal policies, it seems unnecessary to present them as indistinguishable in terms of their position on civil liberties.

This assumption of Islamist homogeneity has troublesome empirical consequences. When presenting the survey results of Turkey and Pakistan, the book consistently combines politicians from various parties under the labels “religious parties” and “non-religious parties” (pp. 185–88, 220–24, 247–49, 276–79), obscuring the scores of specific parties. In Turkey, half of the “religious parties” sample comes from a niche party (Saadet) that has never secured seats in the national legislature and that no one, to my knowledge, has considered anything but a hardline Islamist party. The other half comes from the AKP, a party that has governed Turkey for almost two decades and that plays the role of fallen icon in this book due to its once bright, but now much tarnished, image as a beacon of Muslim democracy. The relevant comparison would seem to be between the AKP and other major parties. Despite their shared origin, the AKP and Saadet may be apples and oranges, and aggregating them seems to stack the deck against those who argue that the former has moderated its stance on liberal rights.

Beyond the surveys, the detail and analytical rigor of the case studies are impressive. The chapters on Turkey and

Pakistan exhibit a laser-like focus on the key independent and dependent variables and the causal processes that link them. The discussions of ROs' shifting investments across sectors and the erosion of civil liberties over time are detailed and informative. The price of this focus is somewhat limited engagement with alternative arguments and causal pathways. For example, in the case of Turkey, I found no mention of the state's embrace of the Turkish-Islamic Synthesis ideology in the 1980s, a factor that Turkey specialists like Hakan Yavuz and Banu Eligür have considered critical to the subsequent development of political Islam in that country.

Overall, the paired comparison of Turkey and Pakistan works well. It sheds valuable light on key causal processes and raises fruitful questions. Most usefully, the paired narratives paint a rich picture of the relationships between Islamist ROs and political parties that significantly enhances, and inevitably complicates, the interpretation of the key interaction terms in the models. For example, the careful discussion of the specific policies pursued and implemented by Turkey's Welfare Party before and after its period in government in 1996–97 shows that it faced significant constraints in implementing its policy agenda and was largely forced to make discreet and symbolic gestures, despite its sizable legislative delegation and the presence of strong ROs. In contrast, in Pakistan, where Islamist parties never obtained more than a tiny fraction of legislative seats and where ROs were less institutionalized than in Turkey, the Islamist policy agenda achieved much greater success than the model would seem to suggest, particularly in terms of bringing about restrictions in civil liberties that are unambiguously consistent with Islamist preferences. Another interesting contrast is that in Turkey the AKP seems to be in the driver's seat when it comes to constraining civil liberties, with ROs playing a minor role, whereas in post-2008 Pakistan it is the tiny Islamist parties that are big players and the ROs that have effectively pushed for restrictions on civil rights. The empirical richness of the case studies therefore illustrates that the key interactions may work in fundamentally different ways in different cases.

In conclusion, *Religious Parties and the Politics of Civil Liberties* is an engaging and thought-provoking book that deploys an impressive array of methodological tools to study an important topic from a novel perspective. As such, it is a welcome addition to the growing literature on religious political parties.

### Response to Luis Felipe Mantilla's Review of *Religious Parties and the Politics of Civil Liberties*

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— Vineeta Yadav

My thanks to Dr. Mantilla for his generous and insightful review of *Religious Parties and the Politics of Civil Liberties*.

Here I offer information on the availability of the book's appendices and some reflections on his comments. The online appendix to the book, which contains tables A1.–A.3, is publicly available through my website and Harvard Dataverse. These tables detail the coding protocols and sources used to operationalize all variables and their summary statistics. They also contain the list of all religious parties by country included in the analysis and the sources used to code their electoral and government performance.

Mantilla and I both agree that religious organizations (ROs) deserve much more detailed attention from scholars because they differ widely not just in their ideological beliefs but also in their organizational strengths, political engagement, and tactics. Where we differ significantly is in our definitions and analysis of religious parties. I define religious parties by the extent to which religious beliefs and positions influence their founding ideology. Changes in *subsequent* ideological and policy positions are then the object of study. Charles Kurzman and Ijlal Naqvi in their extensive study of religious party platform over time also find that Islamist parties start out with fairly similar positions based on orthodox interpretations of civil liberties in their founding moment but *change* these positions over time in response to political imperatives (“Do Muslims Vote Islamic?” *Journal of Democracy*, 21 [2], 2010). Thus, the book does not assume, as Mantilla states, that religious parties are forever “indistinguishable” in their ideological positions. Rather it studies why and how they become different over time with respect to the one well-defined issue of civil liberties in their agenda.

Given this definition of religious parties, variation in positions on civil liberties is the dependent variable being analyzed in all the empirical chapters, including the large-N and case study chapters. This is why the relevant comparison in Turkey is between the Saadet party and the AKP, not with secular parties that never had a religious agenda to begin with. Both religious parties were founded by leaders and activists belonging to the same banned religious Fazilet party who shared the same Millî Görüş outlook. The puzzle, given the definition of religious parties in my book, is why the AKP chose to moderate its religious positions for a long time and then began reverting to them recently, whereas the Saadet Party, which is now represented in parliament, did not soften its positions on civil liberties at any time. This approach differs fundamentally from Mantilla's approach to religious parties, which reclassifies other parties as religious if they adopted a sufficiently religious identity, ideology, and associations *at a given time*. His definition therefore is not limited by the platforms that parties adopted at their founding moments. Which of these approaches to religious mobilization better explains politics in countries where religion is mobilized is an important question with which the field needs to engage.

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The question Mantilla raises about the willingness of religious organizations to take risks by allying with religious parties is an important and intriguing one, because assumptions about the risk preferences of religious organizations, parties, and politicians are endemic in political science research. Yet, we know very little about them. Research based on citizens finds that individuals with more assets are more risk averse, and prospect theory, the most empirically supported decision-making model under

uncertainty, posits that individuals take more risks when they believe they are facing losses. This suggests that smaller religious organizations and those facing losses should be willing to take *higher* risks in their tactics, including allying with religious parties. However, whether we can apply these models to understand the risky choices of religious and political organizations and religious leaders and politicians is an open question worthy of attention from scholars.