neglected and despised forms of cultural consumption and production with deep seriousness and respect—with striving to understand what members of these cultures themselves see as valuable and beautiful. And yet, however objectively apt or theoretically useful it may be to see jihadis as analogous to skinheads or hardcore punks (or, say, Saudi drifters or Egyptian *mahragan* artists), it is a point of view that will always be an alien and belittling imposition on the jihadi self-account. As the American fighter Omar Hammami asks (in a passage Hegghammer quotes on p. 20), "Who throws away their entire life for counter-culture?" And yet, if we do not accept at face value jihadis' own claims to authenticity, we presumably must accept that this is, in fact, what they are doing. And it is in precisely this paradoxical space that "jihadi culture" therefore necessarily exists.

Checking Presidential Power: Executive Decrees and the Legislative Process in New Democracies. By Valeria Palanza. Cambridge: Cambridge University Press, 2019. 262p. $105.00 cloth.
doi:10.1017/S1537592719003116

— Juan Pablo Micozzi, *Instituto Tecnológico Autónomo de México (ITAM)*
juan.micozzi@itam.mx

One of the most striking tools in the process of executive–legislative relations in presidential systems has been and continues to be the president's use of unilateral actions to enact policies. Because of their implications for policy making and even their effects on the quality of democratic performance—enabling some to become *delegative democracies* as described in Guillermo O'Donnell´s "Horizontal Accountability in New Democracies" (*Journal of Democracy* 9 (3), 1998)—presidential decrees became a salient topic in the discipline from 1990s onward, as synthesized by John Carey's and Matthew Shugart's seminal *Executive Decree Authority* in 1998. Even though the topic did not lose centrality in politics or political science, we had to wait about two decades for the publication of the next systematic book on the topic from a comparative perspective. Valeria Palanza's *Checking Presidential Power* offers an innovative and provocative outlook on the choice of decree authority or statutory measures to change the political status quo.

As the author explicitly points out, her theory moves away from several notions in the literature, such as pure presidential dominance in the policy-making process, the systematic weakness of legislatures, and the non-nested structure of the choice between laws or decrees. In this sense, two main micro-foundations of her argument stand out immediately: on the one hand, she understands legislators' behavior as motivated by external pressures (interest groups, lobbies, and even constituents), with influential groups choosing which alternative is going to be less costly to enact their preferred policies, and politicians opting for the output that will maximize their career prospects—an assumption that clashes with most of the empirical, party-based literature on legislative behavior in Latin America. On the other hand, actors' behavior varies as a function of the valuation of each policy, the constraining formal rules in place (*hurdle factors*), and the actors' commitment to ruling institutions. The interaction of these components is going to greatly affect the likelihood of a given movement away from the status quo being enacted via ordinary legislation or executive decrees. On the basis of the net cost-benefits balance, then, Palanza's game-theoretic approach offers formal paths to different subgame perfect equilibria. In this sense, interest groups will prefer, ceteris paribus, to endorse implementation via decrees, the cheapest and quickest path; however, they will choose other directions depending on how much legislators respect institutional hurdles, the relevance of each issue at stake, and how much rules constrain specific paths. As examples, Palanza argues that interest groups would always prefer constitutional decrees, but would endorse borderline-legal executive decisions in case legislators do not care much about rule enforcement. Similarly, regular legislative processes, although costlier, would be endorsed in cases of credible legislative defeats or judicial reversals.

One of the merits of the book is that it models both alternatives, law or decree, together, as choices deriving from the same process. In this sense, different parameters in interaction will guide the preferred path. It is noteworthy how the author conceptualizes and operationalizes her idea of *institutional commitment*, a central component of her theory. Following this notion, variation in legislators' performance does not strictly depend on whether they are soldiers of the executive, delegates of interest groups, or free thinkers. Rather, legislators are selfish maximizers who have career advancement in mind. The expected length of their tenure in office is, here, an endogenous determinant of their commitment to rules, because they would behave as short-term rent seekers if they did not expect to remain in place (progressive ambition), and the opposite if they had static goals. An interesting and testable implication of this assertion is that their behavior should be closer to that of predators in environments without the prospect of long legislative careers. Would term limits foster such behavior?

Palanza's arguments are empirically tested in two case studies with varying levels of institutional commitment, Argentina and Brazil, along with a cross-sectional analysis of seven Latin American countries. All in all, her results tend to endorse her theoretical statements: decrees are the preferred choice when external agents praise policies highly, institutional commitment is low, and rules validate this choice. As an implication, this theory differs from traditional views synthesized in Gary Cox's and Scott Morgenstern's epilogue of *Legislative Politics in Latin America* (2002), which understands interbranch relations

as the product of anticipated reactions between combinations of dissimilar kinds of presidents and assemblies. Whereas the use and abuse of decrees would be a strategy to bypass checks and balances in the former tradition, Palanza understands them as the product of calculations by external actors under different levels of institutional commitment, legal constraints, and policy importance. This does not deny that political actors win and lose throughout the process, but their motivations do not directly take the other party into consideration in the model.

The book successfully challenges conventional ideas about presidential dominance and sterile legislatures, relying on formal models developed for the American case like that of Tim Groseclose and James Snyder ("Buying Supermajorities," *American Political Science Review* 90 (2), 1996). In this sense, a topic of further inquiry (acknowledged by the author in the book) is the widely assessed role of parties in Latin American cases. Both scholarly debates about the Brazilian case and most empirical literature in the region show that final decisions are usually determined by party lines, especially by the underlying government–opposition dimension. This fact is not at odds with Palanza's argument, but might deserve clarification about how higher-level actors might also prompt principal–agent links that affect interest groups' preferred strategies. Another link that might deserve clarification is that between legislators and voters. If, as stated, politicians just care about the extraction of resources to further their careers and do not give preferences strong leverage, what do they use resources for? Is the model based on the latent idea that patronage is the main connection between representatives and voters? If so, should there be a discount factor for the kinds of policies passed, and how will voters value those?

Of course, these inquiries do not detract from the value added by this challenging work, which reinstates the discussion about interbranch relations at the core of the discipline. With this contribution, Valeria Palanza provides an invaluable keystone for the analysis of policy making in Latin America, reminding all of us of something that tends to be omitted in studies of collective decisions: power groups also make a difference regarding observed legislative behavior.

The Politics of Islamic Law: Local Elites, Colonial Authority, and the Making of the Muslim State. By Iza R. Hussin. Chicago: University of Chicago Press, 2016. 352p. $115.00 cloth, $37.50 paper.
doi:10.1017/S1537592719003177

— Dörthe Engelcke, *Max Planck Institute for Comparative and International Private Law*
engelcke@mpipriv.de

Iza Hussin's book presents a nuanced, well-written, and novel approach to the study of Islamic law. Tracking the transformation of Islamic law from India in the 1770s to Malaya and Egypt in the 1870s, she explores how Islamic law "as a contingent and constructed political space" (p. 9) was made and unmade during the British colonial period in Malaya, India, and Egypt. She does so through the study of divisions of jurisdiction, treaties signed between British and local rulers, trials that helped define Islamic law, and portraits and texts.

Hussin justifies her selection with reference to the interdependence rather than the independence of her three case studies. This design allows her both to show how legal concepts that developed in India shaped the making and unmaking of Islamic law in Malaya and Egypt and to argue for the centrality of India in the reconfiguration of Islamic law. Hussin thereby shows that the development of Islamic law during the colonial period was not a one-directional process directed from the heartlands of the Arab world to the fringes. The increased exchange between India, Malaya, and Egypt was facilitated by the opening of the Suez Canal in 1869, new technologies, and the movement of British judges and colonial officials. Hussin is less concerned with establishing causality, and her work seems to suggest that we learn most when we take the interconnectedness of our cases as a given and focus on the effects of these relations and networks. Comparative studies on Islamic law are rare, and Hussin's work makes a compelling case for a comparative, interdisciplinary turn in the study of Islamic law that addresses wider questions prevalent in political science, history, and law.

Hussin analyzes the way in which Islamic law changed during the colonial period. She states that, through processes of negotiation between British colonial and local elites, Islamic law was integrated into the state system and limited to a narrow area: family law. Hussin characterizes this process as secularization. Secularization for her means not only the separation of state and religion but also that the state defines the place and scope of religion. This argument builds on the work of other scholars, most prominently Talal Asad (*Formations of the Secular: Christianity, Islam, Modernity*, 2003). The remaking of Islamic law had important implications for who was in charge of reform and how the reform of Islamic law would happen in the future. The acceptance of state control over the definition and scope of Islamic law is an outcome of that process. Hussin counters the view that legal reform in the colonial period can be considered a mere imposition by colonial authorities. Instead, she emphasizes the role of local elites, who gained new resources and opportunities in the process. Hussin thereby consciously restores the agency of local elites that is often omitted in the work of Talal Asad.

Hussin's innovative comparative approach is further substantiated by her novel use of a variety of sources, such as treaties and archival material in Malay, Arabic, and