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**Vipin Narang and Paul Staniland. "Democratic Accountability and Foreign Security Policy: Theory and Evidence from India." *Security Studies*, 27:3 (2018): 410-447. DOI: <https://doi.org/10.1080/09636412.2017.1416818>.**

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Review by **Rohan Mukherjee**, Yale-NUS College

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“**W**e need to rethink how democratic politics relate to foreign policy behavior” (444). This is how Vipin Narang and Paul Staniland describe the objective of their article, one that they achieve with theoretical sophistication and a deft grasp of the literature on the democratic difference in security studies.

The authors begin their article by exposing a surprisingly under-studied aspect of democratic politics in security studies, which is variation in the institutional environments of democracies. Students of comparative politics, international political economy, and foreign policy analysis have long been aware of the need to carefully disaggregate the category of democracy in order to measure the impact of domestic politics on various outcomes of interest. Students of international security have been less inclined to do so. Narang and Staniland’s contribution therefore takes a logical next step by unpacking variation in democratic politics and what this might mean for foreign security policy (the impact on foreign policy more generally has been the subject of extensive study in the subfield of foreign policy analysis).

The authors build a theory of democratic accountability. Their outcome of interest is twofold: “how much politicians invest in foreign policy and how constrained they are by domestic factors” (421). Although the authors do not state this explicitly, it appears from their analysis that how much politicians invest in foreign policy is a function of issue salience, or “how much the average voter cares.” Correspondingly, how constrained politicians are by domestic factors is a function of the clarity of political responsibility, or “how easily elected officials can be held responsible” (416).

Different levels of salience and clarity in combination generate four ideal-typical accountability environments, each characterized by a particular combination of political effort and domestic constraint. When an issue is

salient and responsibility for it clear, the environment is one of ‘High Responsiveness’: politicians will invest a great deal of effort in foreign policy and will be highly constrained by public opinion. Conversely, when an issue lacks salience and clarity of responsibility is low, the environment is one of ‘Sclerosis’: politicians will put in minimal effort and face few domestic constraints in doing so. When issue salience is high and clarity of responsibility low, the environment is one of ‘Protected Politicians’: politicians put in the effort but there are few mechanisms of reward or punishment originating from the public. Finally, when issue salience is low and clarity of responsibility high, the environment is one of ‘Bounded Flexibility’: only the highest national politicians put in the effort and the downside risk of failure is high due to a public that is able to punish when triggered.

Based on this theoretical foundation, the authors conduct an empirical plausibility probe drawing evidence from India’s foreign security policies in three areas: Pakistan, China, and defense procurement/development. They find that India’s Pakistan policy has played out in a domestic political environment of High Responsiveness; its China policy (barring the early 1960s) has operated in an environment of Bounded Flexibility; its counterinsurgency policy in Kashmir vis-à-vis Pakistan in the early 1990s bears the hallmark of Protected Politicians; and its defense management has been in a perennial state of Sclerosis.

The article is an important contribution to the security studies literature for three reasons. First, it is above all a theoretically sophisticated and thoughtful treatment of an important research question. The authors engage deeply and widely with the existing literature on democracy and foreign security policy. They offer a clear and intuitive theory by which one can distinguish variation across democracies, across issues, and over time when it comes to democratic security policymaking. Their overall theoretical treatment is a model of careful analysis and argumentation.

Second, the article incorporates the vitally important case of India—the world’s largest democracy—into the literature on democracy and foreign security policy. Despite a voluminous literature on audience costs, warfighting, and crisis bargaining, security studies scholars have rarely looked beyond the United States and Europe when investigating the causal mechanisms underpinning democracy’s impact on state behavior. As the authors note, this oversight creates a blind spot whereby the literature assumes that democracy creates an environment of High Responsiveness, when in fact—as the respective subfields of foreign policy and comparative politics have long shown—many other political environments are possible and indeed observable, even in the West.<sup>1</sup>

Finally, the article opens up many valuable areas of research. Future research can focus on the applicability of the theory to cases other than India; on elucidating the causal mechanisms underpinning the associations between salience and clarity on the one hand and accountability environments on the other; on the endogenous manipulation of salience and clarity (alluded to by the authors themselves); and on the sources of variation in salience and clarity as well. The type of middle-range theory offered in the article is thus ripe for extension both higher up and lower down the causal chain.

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<sup>1</sup> See Nicolas Blarel and Niels Van Willigen, “Coalitions and Foreign-Policy-Making: Insights from the Global South,” *European Political Science* 16:4 (2017): 502-514.

Keeping in mind the exploratory and theory-building nature of the article, there remain a few areas that require further refinement and clarification, perhaps in future efforts by the authors and others. I offer three such areas below that pertain to theory, methodology, and empirics respectively.

First, regarding the theory, the authors would do well to introduce a strategic-choice approach to their analysis.<sup>2</sup> This would involve a clear specification of who the actors are in the model, their preferences, and the strategic environment in which they operate. As it stands, the theory relies on the ambiguous category of “politicians” as the principal actors, yet at various points it introduces a national/sub-national distinction within this category, as well as other actors such as bureaucrats and military officials. Foreign policy in a democracy is the result of the actions and interactions of all these players, therefore a disaggregation of accountability incentives faced by different actors—in effect, unpacking the state—would produce richer theoretical insights into state behavior.

This approach is important because even if bureaucrats or military officials are unelected, they are not unaccountable. Thus even when clarity of political responsibility may be low, foreign security policy may be constrained by clarity of administrative responsibility, i.e. most voters are likely to know that the foreign ministry is in charge of foreign affairs, or that the military is in charge of external security. Of course voters cannot credibly punish bureaucrats, but politicians can, as a way of deflecting responsibility. This incentive might then act as at least the lower bound of domestic constraint on the state, shaping foreign policy to varying degrees across issues and over time.

Similarly, a clearer theoretical exposition of who counts as “the public” would be useful for the theory. Recent work has persuasively argued that often democratic leaders respond less to public opinion and more to the opinion of elites.<sup>3</sup> The evidence from public opinion surveys in India that the authors cite suggests that the vast majority of the public remains uninformed or uninterested in foreign policy. Therefore a model that explicitly accounts for bureaucrats, political appointees, military leaders, think-tankers, and scholars as the elite audience to which a democratic leader is in some sense accountable (or at least responsive) would produce new and likely different mechanisms by which issue salience and clarity of responsibility operate to shape the level of effort and constraint in policymaking.

Second, regarding methodology, future research must develop clear and explicit ways by which to measure the variables of the authors’ theory. It is difficult, for example, to measure issue salience simply by relying on attitudes toward a particular country as the authors do in the article. How people feel about Pakistan does not necessarily translate into Pakistan being a salient issue for the public. The use of war as an external shock that escalates salience is useful—and this is the approach the authors take in their analysis of High Responsiveness in India’s Pakistan policy—but this approach risks conflating seeing war as a salient event (keeping in mind that all of India’s wars with Pakistan were initiated by Pakistan) with seeing Pakistan as a salient foreign policy issue.

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<sup>2</sup> See David A. Lake and Robert Powell, *Strategic Choice and International Relations* (Princeton: Princeton University Press, 1999): 3-38.

<sup>3</sup> Elizabeth Saunders, “War and the Inner Circle: Democratic Elites and the Politics of Using Force,” *Security Studies* 24:3 (2015): 466-501.

This is not to say that Pakistan is not a salient issue, but that the authors need a more reliable and convincing measure of salience that can be applied across different contexts. Extensive literatures on issue salience in American politics, European politics, and political psychology (mostly missing from the article) can help here. The same applies to measuring “effort,” which in the article is deduced qualitatively or from assessments by other scholars of Indian foreign policy. A direct measure of policy effort—perhaps by looking systematically at the profile and background of bureaucrats and officials tasked with the Pakistan file in peacetime and at the profiles of military officers tasked with operations during wartime—would go a long way to clarifying this important outcome variable and its relation to the explanatory factors in the theory.

Finally, regarding the article’s empirics, in keeping with the authors’ cautionary note on the difficulty of making causal claims at this theory-building stage, we should be careful not to read too much into the evidence without thinking about potential alternative explanations. At times, for example, it appears that the complexity of an issue—specifically in the realm of defense procurement—is conflated with a lack of clarity in assigning responsibility (see 429). These are analytically and empirically distinct mechanisms by which constraints may operate (or not) on politicians, and should be treated accordingly.

Similarly, with regard to India’s Pakistan policy, while there is a correlation between the public salience of Pakistan (though its measurement is unclear in the article) and the level of effort and constraint in India’s Pakistan policy, it could just as well be that Indian leaders act in ways that reflect their own shared and consistent view of Pakistan as a longstanding rival and existential threat. Going by public statements and actions of the Pakistani and Chinese leaderships respectively, the intensity of Pakistan’s preferences with regard to India’s security and stability has been much greater than that of China. Should it then be surprising that Indian leaders put less effort into their China policy than their Pakistan policy? A balance-of-power approach (as cited by the authors) suggests that this empirical pattern is anomalous, but a balance-of-threat approach handily accounts for it.

Equally, the lack of attention to Pakistan during the period of coalition government led by the Janata Dal (JD) political party in the early 1990s—what the authors label an example of Protected Politicians—could well be explained by standard intra-coalition bargaining and veto-player approaches rather than the inability of voters to identify and punish individuals responsible for security policy. In other words, and in keeping with standard accounts, the JD government fumbled its Pakistan policy because it was disunited and occupied by internal factionalism, not because it was unclear to voters who was responsible for Pakistan policy. As noted above, most voters could probably consistently identify the prime minister and foreign ministry as being responsible for Pakistan policy. A clearer measure of clarity of responsibility applied to the cases would therefore help isolate the mechanism of interest to the authors compared to other mechanisms at play.

In the final analysis, this article is a thorough, well researched, and helpful contribution to the study of democratic politics and foreign security policy. Although it contains some methodological and empirical weaknesses, these are compensated for by the importance of its theoretical contribution. The criticisms raised above should be seen as avenues for future research, wherein the greatest contribution of the article lies as the first word in a long overdue conversation.

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