



Taking Stock of Neoclassical Realism¹

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Neoclassical Realism, the State, and Foreign Policy. Edited by Steven E. Lobell, Norrin M. Ripsman, and Jeffrey W. Taliaferro. Cambridge: Cambridge University Press, 2009. 310 pp., \$31.99 paperback (ISBN-13: 978-0-521-73192-8).

Within contemporary realism literature, neoclassical realism, which seeks to combine both structural and domestic variables to explain state behaviors, has been where the action is (Sterling-Folker 1997; Rose 1998; Introduction, this volume). Admitting structural realism's notion that structure is an important factor for shaping state behavior, neoclassical realism adds a new assumption: structural impact has to be relayed to state behavior via domestic politics, especially state structure and leadership/elite's perception.² Other than this core assumption, however, most neoclassical realists do not agree on what exactly shape states policies, and they have ventured far and wide in the past decade, generating an extensive literature.

After more than a decade of robust growth, we definitely welcome a stock-taking of neoclassical realism. This volume explicitly seeks to do such a job, besides presenting some ongoing studies by some of the leading neoclassical realists.

In their introduction, the volume's editors review the history of neoclassical realism. They refine the core of neoclassical realism by pitting it against classical realism, neorealism, and *innenpolitik*. They also outline neoclassical realism's conception of the state and international structure. Lastly, they demand that contributors look at the three stages of a state's (security) behavior (which they put as three key questions): strategic assessment (the volume focuses on threat assessment), strategy formulation, and implementation of strategy (which covers resources extraction and mobilization, and actual implementation). Most chapters in the volume (Brawley, Dueck, Lobell, Ripsman, Schweller, and Taliaferro) can be neatly classified according to their focus on the three stages. Fordham provides a methodological critique of neoclassical realism's (implicit) assumption that one can neatly separate domestic politics from structure. The concluding chapter sets out some direction for future research. The odd man out may be Sterling-Folker's contribution, which is more akin to the second image reversed approach (Gourevitch 1978), with a constructivism twist.

This volume is important, and overall, very strong. It should be widely read and cited. Below, I shall advance three major criticisms. In light of the vast neoclassical realism literature, I do not limit my criticism to the present volume.

The most glaring omission of this volume (and the larger literature of neoclassical realism) has been international cooperation. The editors of the present volume set this tone early: the volume is to focus on how states assess and cope with threat and opportunity for expansions (1, 33-2). The volume thus has a strong "competition bias" throughout, in Glaser's (1994–1995) words. When this

¹I thank Rajesh Basrur and Taylor Fravel for their critical comments on an earlier draft. I also thank Randy Schweller for clarification. I apologize for not citing more works, due to space limitation.

²The assumption that structural factors impact state behavior also differentiates neoclassical realism from *innenpolitik* in either the liberalism or Marxism tradition. See "Introduction," this volume.

is the case, neoclassical realism is in danger of falling into the offensive realism camp rather than becoming a theory of foreign policy that is consistent with both offensive and defensive realism (Taliaferro 2000–2001).

The competition bias—which is an obvious selection bias, also entails other problems. The editors in their concluding chapter argue that neoclassical realism is fitting for understanding two out of the four worlds (280–287). The four worlds are: (1) clear information on both threat and policy responses; (2) clear information on threats but unclear information on policy responses; (3) unclear information on both threat and policy responses; and (4) unclear information on threats but clear information on policy responses. While it may be true that neoclassical realism has little to offer for World 3, the conclusion that neoclassical realism only fits two world is unjustified, biased, and is due to their competition bias. If the competitive bias is eliminated, the scenario in which a state chooses to forge close cooperation with another state when their relationship is ambiguous fits squarely into their World 4. When this is the case, World 4 actually constitutes an important territory for neoclassical realism to claim.

The competition bias also brings an obvious normative problem: many neoclassical realists seem to believe that a cohesive and autonomous “foreign policy executive” (hereafter, FPE) and elite (and public?) is always a blessing. Yet, the exact opposite may be true. Heading into the invasion of Iraq of 2003, America’s FPE and the larger elite class were perhaps too cohesive and too autonomous. The result has been an unnecessary and disastrous over-expansion.

There exist several studies on international cooperation which neoclassical realism has failed to take notice (Solingen 1998; Schultz 2005; Fravel 2008).³ Neoclassical realists in the competition mode will do neoclassical realism some good by paying more attention to the domestic politics of international cooperation.

A second problem involves a lack of synthesis. Although all neoclassical realists submit to the assumption that domestic politics is a key for understanding state behavior, they do not share an integrative framework for analyzing the actual process through which states formulate and implement policies. More often than not, each author develops his/her own explanatory framework without attempting to build upon each other’s work, although there has been some apparent and substantial overlapping among different authors’ frameworks.

For instance, Fravel (2008) banks on regime insecurity to explain China’s compromise on territorial disputes, yet regime insecurity was also one of the factors within Schweller’s framework.⁴ Likewise, although a focus on interest groups seems to be an emerging common theme, many have failed to note that social cohesion within Schweller’s framework at least partly overlaps with the presence of interests groups. Finally, these authors have essentially ignored Solingen’s earlier framework that centers upon regime type and coalition type.

Due to a lack of critical and accumulative synthesis, the accumulation of knowledge by neoclassical realism so far has been limited. In the long run, this may become neoclassical realism’s Achilles’ heel: Neoclassical realism cannot

³Of course, Solingen or Schultz may not want their works to be classified as neoclassical realism works. Interestingly, both Ripsman and Sterling-Folker studied cooperation before. It must be emphasized that cooperation when facing a common threat (as in cases studied by Schultz and Fravel) is consistent with both offensive realism and non-offensive realism theories (Tang 2010: esp. chapters 1 and 4).

⁴Fravel also failed to note that elite cohesion might have also been a major factor behind China’s compromise on territorial disputes. This was especially true in the early years of People’s Republic of China because Mao enjoyed almost unquestioned authority over his subordinates. Schweller separates elite cohesion from social cohesion. I think it is more reasonable to take elite cohesion as part of social cohesion: social cohesion is to be understood at both the elite and the societal level. Schweller (2006, 54) does not help his case in defining elite cohesion as “the degree to which a central government’s political leadership is fragmented by persistent internal divisions.” Defined as such, elite cohesion seems to overlap with elite consensus. Elite cohesion, as I define it, is the degree to which members within the elite class of a society treats each other as elite. Meanwhile, societal cohesion is the degree to which citizens within a state treat each other as having equal claim to the state and citizenship.

continue to claim to be a progressive research program without critical synthesis at some point.

Methodological issues are a third problem. Two contributions in the present volume address these issues. Fordham points out that neoclassical realists tend to believe that structure and domestic politics are neatly separable thus additive. Yet, apparently, structure and domestic politics interact with each other and thus constitute a system. As a result, an interactive or systemic approach, rather than an additive or linear approach, should be the preferred approach. Brawley emphasizes path dependence as another key methodological issue when examining state behaviors within a long time frame. There are several other deeper methodological issues that neoclassical realists have so far failed to appreciate adequately.

First, existing works tend to focus on strategic failure (to balance) rather than strategic success. This is partly understandable: because success means all three stages must have gone well, explaining success is more demanding. Yet, explaining failures will not be easy either, precisely because failure at any one of the three stages will lead to strategic failure. Did a policy fail because it was bad, incoherent, or because it was good but badly implemented? Some neoclassical realists have not been very careful with this issue. For instance, Taliaferro (this volume) suggests that China's and Japan's contrasting responses to the coming of the West were largely due to the two states' capacities to mobilize resources. But this may not be so clear-cut. Was China's "self-strengthening" attempt unsuccessful because the Qing dynasty had no strategy or merely a bad strategy or because the Qing dynasty was unable to mobilize to implement the (good) strategy? Certainly, most historians have favored the thesis that Qing China had no strategy or an incoherent strategy (that is, piecemeal reform) at best, whereas Japan had a coherent strategy (that is, wholesale Westernization).⁵

Second, different factors may have different weight in the three phases of state behavior. For instance, competition of ideas features prominently in strategic assessment and strategy formulation whereas state capacity features more prominently in strategy implementation. The first two stages can be (and often have been) studied together, whereas the third stage can only be studied by assuming some given strategies. When this is the case, as in Dueck's contribution, it is hardly surprising that after a strategy was already in place, post-WWII American FPE often has had enormous freedom in shaping specific policies (that is, specific military interventions); domestic politics has rarely, if ever, compelled FPE to intervene militarily; domestic concerns have only shaped the exact form and conduct of interventions (for example, limited war, refraining from calling up reserves).

Third, because factors interact with each other to constitute a system, the same factor may operate in different, sometimes opposite, directions in different situations. Schweller (2006: 47) noted that regime vulnerability mostly affects a state's ability to extract resources for balancing. This does not sound correct: a vulnerable regime may cripple the state in all three stages of balancing. Yet, an equally strong case can also be made for the opposite, as the diversionary war thesis has done (for example, Argentina before the Falkland/Maldives war). Similarly, Ripsman (this volume) maintains that relative state autonomy is more important than regime type. This too seems to be simplistic. Clearly, the presence of open and legal opposition parties makes concessions in democracies more difficult than in autocracies (Schultz 2005), and China's numerous concessions in territorial disputes were made more likely under its autocratic political system (Fravel 2008).⁶

⁵Of course, it may also be true that the Qing dynasty would have been hard-pressed to extract and mobilize the necessary resources to implement a strategy *even if* it had a coherent and sound strategy.

⁶Fravel failed to note this variable, perhaps because this variable has been constant for his project. Regime type is a variable within Schweller's (2006) framework, but it does not feature significantly in his discussion.

Overall, in what direction and how powerful a variable operates depend on other factors. As such, an additive approach that stacks different explanatory variables together is not really tenable: only a systemic approach will do (Jervis 1997).

Fourth, so far, many neoclassical realism writings contain only “confirming” cases, with the exception of Fravel (2008) perhaps. This cannot be very satisfactory in terms of methodology. At the minimum, this makes testing and refining neoclassical realism theories difficult. Neoclassical realists should pay more attention to methodological issues in qualitative studies.

Fifth, although neoclassical realists unanimously emphasize the role of policy-making executives, the role of leaders has been mostly missing from the discussion. Yet, there is no doubt that individual decision-maker traits, especially their personality and worldview, have all impacted their decisions. After all, it is leaders that construct threat, debate and decide strategies, and order mobilizations. In this sense, Sterling-Folker’s contribution, which does not fit into the volume easily, points to a more fruitful direction: we need to understand elite identities and how their identities shape their perceptions to understand state behavior.

Finally, there is a deeper methodological problem, rooted in neoclassical realism’s intellectual affinity with structural realism. Structural realism holds that structure dictates state goals/interests (security and/or power) whereas structure and domestic politics together dictate state strategies (Powell 1994; see also Waltz 1979, 91–92). Yet, the assumption that states seek power and/or security is only useful for *theorizing about international politics at the structural level*, but of very limited value for understanding actual state behavior. Indeed, maintaining that states seek power/security robs explanatory power for state behaviors that are possessed by state interests, an understanding explicated by Machiavelli’s “the ends justify means.” For understanding actual state behavior, we have to go down to a state’s specific interests (Tang 2010: chapter 1). This will bring identity and other things into a realism framework: a state’s specific interests are not given but constructed by elites through a discourse. Dueck’s contribution (this volume) totally ignores this issue.

This brings out another issue. Should neoclassical realism more forcefully differentiate itself from other schools (for example, liberalism and constructivism) or move toward some kind of synthesis? Sterling-Folker has moved into the synthesizing direction, and so has Schweller (he now talks about ideology and legitimation). But others seem to be firmly operating within the more traditional realism approach, taking state identity, ideologies, and preferences over goals (interests) as given. In light of the preceding discussion, it seems the stand of sticking-with-(material) realism is less and less fruitful. Neoclassical realists may have to dip into the murky water of individuals and identities.

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