Interview – Paul Staniland

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E-INTERNATIONAL RELATIONS, AUG 6 2024

Paul Staniland is a Professor of Political Science at the University of Chicago and a nonresident scholar in the South Asia Program at the Carnegie Endowment for International Peace. He is also the Associate Chair of the Political Science Department, and previously served two terms as faculty director of the Committee on International Relations MA program. Staniland's research focuses on political violence and international security, with a regional emphasis on South and Southeast Asia. His first book, *Networks of Rebellion: Explaining Insurgent Cohesion and Collapse*, was published by Cornell University Press in 2014. His second book, *Ordering Violence: Explaining Armed Group-State Relations from Conflict to Cooperation*, was published by Cornell in 2021. Staniland received the 2022 Karl Deutsch Award for contributions to the study of International Relations and Peace Research from the International Studies Association. He is working on a new book about how third-party states have navigated, and been affected by, major power rivalry in post-1945 southern Asia.

Where do you see the most exciting research/debates happening in your field?

There are so many possible answers to this question. Sometimes it seems fashionable to be down on the discipline and cynical about research, and do I understand why. But there are so many important and interesting topics available for study these days; I think there is a lot of exciting work going on and that could be done. I'll pick a couple just as examples.

First, a lot of great new work is being done on electoral politics and violence, from patronage networks to armed political parties to how insurgents try to influence elections to state repression of electoral rivals. This work is pushing a lot of boundaries in very productive ways, across contexts and methods. It taps into big-picture scholarship on democratic backsliding (as well as those skeptical of that framing), political parties, state formation, and the quality of democracy, sitting nicely at the center of several pressing and important topics.

Second, we are seeing a wave of research on international politics in Asia that is connected to broader IR questions and debates. This seems to be driven by the combination of real-world interest in US-China competition and its implications with the growing range of methodologies that can be applied to these questions, ranging from archival research in IR to survey experiments to text-as-data analysis to elite interviews. This is a first-tier policy question that can be cut into with interesting and rewarding social science approaches.

Third, the field of civil-military relations seems to be returning to relevance, and seeing a renewal of political science interest. After a long period when militaries were seen as in political retreat, there has been a surge of coups and subtler but important military interventions in Africa, the Middle East, and parts of Asia. Moreover, other forms of authoritarian regimes, whether personalist or single-party, still need to deal with their militaries. We are seeing new methods applied to these questions, including surveys and historical datasets, as well as new opportunities for cross-case analysis.

How has the way you understand the world changed over time, and what (or who) prompted the most significant shifts in your thinking?

During graduate school, I became increasingly interested in thinking about processes and changes over time. I

remember my dissertation chair Roger Petersen reading an early chunk of my dissertation and saying "the word that keeps coming to mind here is 'trajectory.'" Influenced by that suggestion, I'm interested in trying to find both initial causes of variation and subsequent trajectories that sustain or shift those initial conditions, whether in the realm of insurgent organization, state-armed group interactions, civil-military relations, or, now, swing states' responses to major power competition. It's just how I've come to think about the world – starting points and movement, with an acceptance of lots of messy endogeneity as a fundamental part of political life, rather than a problem to be sidestepped or solved. This creates a lot of challenges for both theory and method, and I won't pretend to have solved them, but I think my work does at least set up interesting and useful frameworks for helping us think about a variety of questions.

Your work has focused on security and violence, particularly in South Asia. How has local conflict, such as the civil war in Myanmar, impacted regional stability?

Local conflicts have very often had major implications for regional stability. This has been most relevant to context in which "local" wars tap into interstate tensions – Kashmir's centrality to India-Pakistan conflict, most notably, as well as the Pakistani Taliban's cross-border operations from Afghanistan and the US intervention in Afghanistan's spillover into Pakistan. Transnational insurgencies and state sponsorship are an enduring source of regional instability; this continues to be the most likely source of another major India-Pakistan crisis.

In other cases, local conflicts have regional implications by generating or augmenting cross-border refugee flows and illicit markets. India's Northeast and Myanmar's border regions both have important non-state armed actors that have connections to the movement of goods, capital, and people across borders. These dynamics have an important political element – armed groups sometimes are governing these flows or trying to tap into them as economic and political resources. Governments are forced to deal with the implications of these flows as well, such as the Rohingya movements into Bangladesh in 2017 or Indian and Chinese concerns about managing their borders with Myanmar. Right now, outside of the Afghanistan-Pakistan border, this is where a lot of the regional implications can be found, not in the realm of purely state-to-state crises. That said, these things can shift and escalate unexpectedly, so the future is unpredictable.

Months before Prime Minister Modi's coalition lost seats in the Indian general election, your collaborative research at Carnegie showed that democratic opposition in the region was stronger than some had previously thought. Do these results support that perspective?

I think broadly they do, though I should be clear that I didn't expect such a substantial opposition over-performance relative to the opinion polls. It's been enormously difficult to build and sustain hegemonic national regimes in the region. India has seen the most success because its dominant parties – the Indian National Congress from 1947-1989 and the Bharatiya Janata Party since 2014 – have been quite deeply embedded at the mass level while also able to manage elite factionalism. Even they, however, have felt real pressure and backlash at various moments.

In Pakistan, the military remains politically very powerful but has consistently failed to build a stable political system despite its various manipulations. Sri Lanka has seen recurrent political-economic crises and electoral surprises, including after periods (like 2005-2015) that seemed to have ushered in a stably dominant political dispensation. Bangladesh at present under Sheikh Hasina is the closest to hegemony, but the personalized/family-ized nature of her rule means there is a lot of risk of instability. Nepal since 2008 has been the site of recurrent coalitional bargaining and feuds, electoral setbacks to ruling governments, and some periods of social movement mobilization. Myanmar's military regime is facing massive armed and unarmed resistance across huge swathes of the country. Outside of Myanmar, armed insurgency is well down across the region, so at a broad level state power is on the upswing, but government/regime dominance, much less political hegemony, is absent.

In your recent piece, The Myth of the Asian Swing State, you argue that local political contexts should play a larger role in defining Washington's approach towards Asia's non-aligned states. What does the U.S. stand to lose if it doesn't adopt this approach?

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My argument in that piece – which draws on my current book project – is that contemporary Asian states are much more autonomous from major power influence than they were during the Cold War. The bloodiest conflicts in Cold War Asia – Korea, Indonesia, Afghanistan, and Vietnam most notably – were over fundamental questions of who would rule domestically (and within which borders). Those kinds of intense civil wars or high-stakes contests over political control are far less frequent today. Moreover, the domestic cleavages within southern Asian states often do not line up with the terms of the US-China competition – in many third-party states, there aren't clear, consistent "pro-China" or "pro-America" ideological blocs.

As a result, the US needs to approach competition with China on a highly contextual, decentralized, case-by-case basis. In some countries, the Indo-Pacific Strategy to compete with China will be welcomed and should be emphasized; in many others, the last thing local actors or publics want to hear is that they are a battlefield for influence between the US and China. Their internal politics are incredibly varied, and so policymakers need to avoid top-down, one size fits all strategic approaches – what plays well in the Philippines may be counterproductive in Nepal.

In *Ordering Violence,* you suggest that the traditional, brokered peace deal is not the only way to resolve conflict. Does the pursuit of total resolution at times hinder more limited but realistic solutions in international conflict and civil wars? How else might peace be achieved?

Ordering Violence argues that there are a number of ways to achieve something like stability in internal conflicts, from traditional internationally-backed peace settlements to live-and-let-live deals, protracted ceasefires, or low-level, mutually managed conflicts. Rather than assuming that we should be pushing for centralized, state-building, armed group-demobilizing deals, it encourages a much more heterogeneous and contextual approach – sometimes a protracted ceasefire is a better solution than trying to ram through a problematic peace settlement, for instance.

In a piece with Basil Bastaki and Bryan Popoola coming out later this year in *International Security*, we delve much more deeply into what these options are and how policymakers should approach them, especially in cases when international peacekeeping and mediation are unavailable. Some conflicts are simply bad candidates for any form of stabilization and will likely persist, but in others, certain political configurations can exist – and sometimes be encouraged – that could lead to extended periods of state-armed group cooperation or even a formal peace deal. These are often protracted, incremental, and problematic in various ways, but can be superior to always trying to advance a robust peace settlement that would fail or advantage a repressive state.

Where is the biggest disconnect between those who study international relations and those who practice it?

Practitioners are dramatically busier than academics in terms of day-to-day churn; they have far less time to reflect, read, or write than academics. They also tend to have a firmer sense of the politically possible and, especially, of how bureaucracies work. Conversely, academics often can provide both analytical frameworks and specific substantive expertise that is interesting and useful to practitioners, in part because academics have a lot more time to sit and think and read without needing to constantly deal with the new crisis or demand of the day.

I think it's very valuable for the two categories of people to interact, and I've always enjoyed interfacing with policymakers and analysts. But it's equally important to be realistic about the likely results: very few policymakers are actually crafting grand strategic directions, while academics' value-added often is not very useful to specific policy questions urgently on the table.

These are ultimately quite different jobs. Recognizing that can make for a better understanding of relative strengths – academics sometimes have a tendency to denounce policymakers and demand radical changes that are wildly unlikely or overly simplistic, while policymakers can have a tendency to categorically dismiss academics as irrelevant or out of touch. A different framing is that each group has a different role and competence that sometimes can productively overlap.

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What is the most important advice you could give to young scholars of International Relations?

First, read a lot, broadly, across cases and topics.Having a strong base of substantive knowledge and an ability to connect ideas is essential, including (sometimes especially) for scholars who are very focused on methodology. These things work together, rather than in opposition to one another.

Second, sometimes take a break and intentionally decide to learn new things. As your career advances, you get busier with other responsibilities and it can become easy to keep working on the same basic things again and again. I've certainly had this experience myself. This was one reason why I decided to make my current book project not primarily about civil war (though it definitely overlaps) and to start studying cases like Nepal that I previously hadn't: I felt like I'd mostly run out of new things to say and didn't want to spend the next 30 years writing tweaks on the same basic topics. The risk of ended up stuck in an intellectual rut makes it important to try to out something new. You need to make time for that kind of refilling of your mental cup and be clear in your mind about when you've hit the flat of the curve in your current area of research. I'm now getting some new ideas about political violence research as a result of taking a (partial) break from it, so I will likely end up returning to some areas of prior work, but hopefully with something novel to say.