Interview - Charles King

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E-INTERNATIONAL RELATIONS, OCT 18 2015

Charles King is Professor of International Affairs and Government at Georgetown University, where he also serves as chair of the Department of Government. He previously served as chair of the faculty of Georgetown's Edmund A. Walsh School of Foreign Service, the country's premier school of global affairs. King's research has focused on nationalism, ethnic politics, transitions from authoritarianism, urban history, and the relationship between history and the social sciences. He is the author or editor of seven books, including*Midnight at the Pera Palace: The Birth of Modern Istanbul* (W. W. Norton, 2014); *Odessa: Genius and Death in a City of Dreams* (W. W. Norton, 2011), which received the National Jewish Book Award; and *The Ghost of Freedom: A History of the Caucasus* (Oxford University Press, 2008), which was named "History Book of the Year" by the *Moscow Times*.

King's research has been supported by the Social Science Research Council, the International Research and Exchanges Board, and the Kennan Institute for Advanced Russian Studies. In 2012-13 he was a Fellow of the Woodrow Wilson International Center for Scholars. He has also held visiting appointments at the University of Michigan and Bosphorus University in Istanbul, where he was a Fulbright Scholar. He is a life member of the Council on Foreign Relations.

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

I follow pretty closely the literatures on civil war/substate violence, ethnicity/race/nationalism, Russia/Eastern Europe, and interpretive methods, as well as various literatures in history and related social sciences. Across all of them, I think, what I find exciting is the trend toward more micro-level research questions—not "small questions" as such, but rather pushing down the kind of research we do in order to meet politics where it actually happens: in the lives of individual women and men. If you look at the work, for example, of Stathis Kalyvas, Kanchan Chandra, Rogers Brubaker, Tim Pachirat, Lee Ann Fujii, Elisabeth Wood, and Kate Brown (whose last book, *Plutopia*, won the top American history and Russian history prizes in the same year), this is precisely the direction important and innovative scholars are pushing us. It is also one of the principal orientations of the interpretive tradition: to try to see the world from the perspective of those who are actually performing the actions we are trying to understand.

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

I originally studied philosophy and history, not political science, and I think I have long been influenced by ways of understanding the world that privilege contingency and context. There are a few pieces of writing that I encountered in graduate school that I find myself going back to again and again, and regularly point out to my own students as prime examples of the human brain in action: Robert Darnton's essay "The Great Cat Massacre," which is a superb model for how to set up a research question using interpretive methods; Clifford Geertz's "Thick Description," which is unrivaled as a piece of writing on methods and ethics; Bill Buford's *Among the Thugs*, a journalistic account of football violence that helped shape the way I think about who violent people actually are; anything by James Scott, whom far too few graduate students encounter today; the work of Elinor Ostrom, whom one should read as a moralist as well as a theorist; the work of Rogers Brubaker, who is such an elegant thinker and writer. I have also had the very good fortune to have delved into vastly different literatures at different stages of my career: "transitology," nationalism, and ethnic studies when I was a graduate student; security studies and political violence as a

postdoctoral researcher; archaeology, ancient history, and early modern history as an assistant and associate professor; and more recently urban history, anthropology, interpretive methods, and Holocaust studies. I've always thought the world was far too complex to be relegated to a single disciplinary lens. But I suppose my starting point in understanding the world is more or less an ethnographic one: the idea that things we perceive as pathologies, oddities, or deviance from some global norm are in fact human adaptations to a set of social and cultural constraints. I'm always amazed at how much of contemporary American political science—such as the literatures on democratization, foreign policy, and political violence, for example—seem to miss this point.

In your recent article "The Decline of International Studies" in Foreign Affairs, you have talked about the fact that in the US the studies of international affairs have recently "come under growing attack." What are the main reasons behind it, and what are the implications of it?

Part of the issue is purely budgetary. Many of the spending constraints imposed after 2008—the so-called sequestration policy—still affect many areas of U.S. federal appropriations, including support for scholarship through the Department of Education, the Department of State, the Department of Defense, and the National Science Foundation. But scholarly research has also become an increasingly politicized issue. There are Republican members of Congress—and we should be clear: this is, in fact, a matter of party politics—who have brought to social science research the same kind of "culture war" grandstanding that they applied to the arts and humanities in the 1980s and 1990s. It is crass, cynical, and purely political posturing. The amounts of money that Congress is cutting are minuscule compared to the overall US budget.

You have also explained that the US government agencies are increasingly preferring policy relevance research, particularly the research on matters of national security (and by extension, international security, such as terrorism), but at the same time are scaling back their funding on regional and language studies. How does this policy fit with the need to have a deep understanding of the histories and cultures of different regions of the world in order to formulate policies of engagement with them?

Policy relevance is, unfortunately, a deeply disingenuous and misguided way of thinking about scholarly impact. For example, let's say I propose two research topics: one is on China's growing strategic rebalance in East Asia based on a systematic set of interviews with Washington-based experts; another is on gender bias in prenatal care in an outlying region of China, based on a six-month ethnographic fieldwork project in the region. Most selection committees—at least those offering US federal funding—will see the first project as definitely policy-relevant and the second as minimally or only potentially so. But if what we're trying to do is (1) understand China and (2) do real scholarship based on empirical evidence, the second project clearly ought to win out. When I'm on selection committees, I always ask myself not "Is this policy relevant?" but "Is this work based on actual empirical research that has the potential to tell us something real?"

To make a slightly different point, do you believe that the exigencies of marketable skills are another factor responsible for weakening the studies of international affairs as the debate about liberal arts and humanities education versus STEM (Science, Technology, Engineering and Mathematics) education in the US goes on? If so, could the US afford to weaken its intellectual capital vis-à-vis its global engagement?

I think there are no more marketable skills than knowing real things about real places. None of this is incompatible with quantitative ways of knowing. The ability to run a smart survey, or to build a reliable large-n dataset, or to run a field experiment based on a deductive formal model—all of that ought to be easily compatible with deep, local, and context-specific knowledge. There is certainly a great deal of talk in the United States about the need for better STEM training and research, but I don't see this as necessarily inimical to good social science education. In fact, I think the real dangers to excellence in the next generation of social scientists have more to do with trends within political science than with shifts in funding priorities or overall changes in university-level agenda-setting.

Apart from politics, in your article you have also said that academia itself is partly responsible for this downfall. Why do think that scholars are moving away from the philosophical approach to understand

and explain the larger dilemmas associated with politics and are increasingly focusing on hypotheses formulations on less important relationship between variables?

If there is one problem I'd identify, it's the baleful effect of King, Keohane, and Verba's *Designing Social Inquiry*. That book—even though it has received a great deal of criticism in recent years—set the agenda for graduate-level training in the social sciences (at least in the U.S.) for a generation. The principal problem, in my view, is that it dismissed an entire century's worth of debate about what knowledge in the social sciences actually entails and replaced that diverse, sophisticated conversation with a scientized pastiche: a vocabulary that helped students perform a certain version of science that privileged deductive hypothesis-testing over other ways of knowing. It also pushed students of qualitative social science toward thinking primarily about the form of an argument, using a vocabulary drawn from regression analysis, than about its substance or importance. Go to any dissertation defense in a major U.S. university, or any job talk for that matter, and my sense is that you will find the discussion focusing mainly on structure rather than on the originality of sources, the interpretation of evidence, or the importance of the question. At least in the American academy, younger scholars are not really trained in the collection of evidence: to do deep political ethnography, uncover a previously lost archive, or otherwise to do the research-of-record on a political topic. It is a great loss for social science that we are now more concerned with a clever argument or a "neat" or "cool" research design than with preserving and collecting the variety of human solutions to the problem of political order.

There is a growing pressure on young scholars of IR to learn and apply statistics. What are the implications of quantitative methodology in terms of freethinking and reasoning, because researchers simply have to rely on the nature of relationship between variables given by the machine (or mathematical function) even if the result contradicts the reality, and in the name of Popperian falsificationism, such research process continues?

I think the best quantitative researchers actually have a great deal in common with people who are interested in deep local knowledge. After all, the quality of your dataset is only as good as your coding. If the coding is based on an impressionistic and reductionist assignment of numerical values to complex and contested phenomena, how could the quantitative findings be of much use? In other words, students should certainly become skilled at quantitative analysis, but they should also develop a fine-tuned sense of where their data are actually coming from. And I think it's always perfectly reasonable for scholars, whatever their methodology, to conclude that there are some topics that are simply not researchable questions. For example, there is a large body of research focused on the issue of the causes of ethnic conflict. But the more I have thought about this field, the more I've come around to the idea that, as Rogers Brubaker has written, the real question is not "Why do ethnic groups fight?" (when an entire "ethnic group" is never involved in violence) but rather "Under what conditions does social violence get classed or marketed as 'ethnic'?" That's a very different question, and one that too few social scientists have taken up.

As a new academic researcher, one might get a sense that academic mainstream is about conventionalism – either you follow the convention and become a part of it or remain at the margins – and critics are not really entertained. In what ways can this be resolved, if true?

I suppose the mainstream is by definition conventional. But I have always thought that a certain quirkiness is the real key to both good social science and career success (as well as a great deal of professional luck). The sociology of the grad student lounge will always push one toward some imagined "mainstream" and away from truly daring work. That's unfortunate, and it's a trend that I think every graduate student should fight—first of all, by finding the right mentor. After that, I think one really has to care about something, to have a deep interest in a subject, or a place, or a culture, or a set of problems. I believe I can help graduate students figure out how to take something they care about and then do good social science on it. I find it much harder to help a methodologically teched-up student figure out what's worth caring about, if they don't already know for themselves.

Social science is not a domain disconnected from humans and societies. However, there are claims that social scientists seem to lose touch with the ordinary citizens in that they apparently tend to limit themselves to their communities of scholars and to their complex jargons, and their ideas are not being

explained to the broader audience. What are the ways you think we could overcome this communication-gap?

I think the answer is actually simpler than it might appear: Social scientists simply have to write and speak in multiple registers, that is, in ways that will make sense to multiple audiences. (The literary historian Stephen Greenblatt said it best: the basic rules are "Never say 'of course,'" and "If you refer to Descartes, explain who he is.") The guild-like nature of the academy can create real disincentives for this, but I think young researchers have to fight this trend. Established scholars can help by retooling some of the aspects of graduate education. For example, rather than having graduate students write one after another article-length paper, assignments could include things like writing a blog post, or a newspaper editorial, or a policy memorandum—perhaps even taking a published academic article and then rewriting its findings for a broader audience. (Blogs such as *The Monkey Cage* have done wonders for helping academics write for broader audiences and demonstrate the real relevance of social science.) We are also terribly bad at teaching people how to speak well, both in front of fellow academics as well as in front of undergraduate students or a general public audience. Imagine a graduate program that included mandatory coursework on "public writing" and "public speaking." I believe we'd produce better scholars—and a new cohort of individuals who would be able to convince the general public that higher education is still worth supporting.

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

The world is not your laboratory. It's a place inhabited by real people with real passions and tragedies, aspirations and enthusiasms. The great privilege of doing social science is that we get to observe humans at their absolute best and their tragic worst—building states, making war, forging peace, changing their minds about their government and its legitimacy. If you ever lose sight of what a gift it is to be able to do this—or if you somehow come to think of people as merely "subjects" of your superior, dispassionate investigations—you've stopped doing good science.

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This interview was conducted by Anil Sigdel. Anil is a Commissioning Editor at E-IR.