

Interview – Omar McDoom

Written by E-International Relations

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Omar Shahabudin McDoom is a comparative political scientist and Associate Professor in the Department of Government at the London School of Economics and Political Science. His research interests lie in peace and security. He specializes in the study of conflicts and violence framed along ethnic and religious boundaries and in strategies that promote coexistence and cooperation between social groups in plural societies. He has field expertise in Sub-Saharan Africa — primarily Rwanda, Burundi, the Democratic Republic of the Congo, and Uganda — and in South-East Asia, notably the Philippines.

Dr McDoom's work has been published in *International Security*, the *Journal of Peace Research*, and the *Journal of Conflict Resolution*. He has held research fellowships at Harvard and Oxford universities. His professional experience includes work as a Policy Officer for the World Bank, as a Legal Officer for the Government of Guyana, and on electoral missions for the OSCE and UN. He holds law degrees from King's College London and the Université de Paris I, Panthéon-Sorbonne, a Master's degree in International Development Studies from George Washington University, and a PhD in Development Studies from the London School of Economics and Political Science. He is also an attorney (non-practicing) admitted in New York.

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

For me, one of the more interesting debates that I am engaged in, and I think underlies a number of IR debates, is the debate between these twin forces of rationality on the one hand, and emotions on the other. Now, these are often seen as opposing forces, and the normative bias in political science and, to some extent, IR as well has been towards rationalist explanations of intra- and interstate behaviour. The frontier for me in this debate is the recognition that these are not mutually exclusive forces that drive human actions and human decisions but, in fact, they work together. This is not a radical idea. If you are a social psychologist that's actually assumed or at least has been shown for decades at this point. I want and have been trying to bring this idea into political science. I've looked at this specifically in the context of identity and how ethnic identity shapes outcomes, everything from nationalist mobilization, civil wars, intergroup violence, political party formation, alliance formation, but it applies pretty broadly to almost every area in which individuals make judgments or decisions. For instance, a piece I'm working on at the moment, drawing on this idea of what I call the integrative approach to emotions and reason, shows that they work together. Political actors don't make these decisions using logic and rationality, etc., exclusively. At the same time, they're not in the grip of emotions that they cannot control. If you're a member of an ethnic group, for instance, you may feel pride or have strong feelings of loyalty but this does not mean you can't input interests or rationality into the equation.

My most recent piece on this looks at this in the context of political alignments and explores when co-ethnics are more likely to form parties with other co-ethnics or with non co-ethnics. I argue that individuals make what they think are rational decisions, but these decisions are shaped by emotions through a number of mechanisms such as evaluation, evaluative judgments, perceptions of risk, model evaluations, estimates of probability. Imagine if the emotions were those in intergroup conflict, for example, those of fear, anxiety, resentments. These are all negative emotions, and I believe that they lead members to overestimate risks or to underestimate the probability of cooperative strategies. It is through these mechanisms about evaluation, about probability assessments, about risk assessments, that reason and emotion work together. I see a recognition of that relationship as the new frontier,

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trying to integrate emotion and reasoning into thinking in political science.

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

There are two broad areas, one is ideological, almost philosophical, and the other is more methodological and possibly even epistemological. What has happened over the last decade or so for me, ideologically, is as someone was born and grew up in the UK – and in the post-Cold War, Western, global North, international liberal order – is that I've come to question ideologically the merits of liberalism as the basis for national systems and our post war international order. This concern I have for liberalism predates the rise of populism and Brexit and Trump. Even though they have brought this question sharply into focus, the Economist magazine, for instance, did this whole big piece devoted to the defence of liberalism not so long ago because it was so worried that its ideological basis was coming under attack. I've asked myself, what are the things that troubled me about liberalism from a kind of political theory or even philosophical perspective?

There were three things I thought about. One is the focus on the individual as the unit of analysis. In micro foundational research, we're talking about how individuals make decisions and I've come to question whether we've given too much importance to evaluating the power of the individual at the expense of communities. You see this in various debates, over multiculturalism for example, community collective rights versus individual rights. I think that liberalism suffers from an excessive methodological individual bias.

The second is the foundational liberal idea of competition over cooperation. We see this premise that competition is valuable in markets very clearly but also in politics, like the whole premise of competitive politics, elections that are contested, and you don't have to look too far to see the risks of competition in markets, the externalities that come with market forces, that's nothing new. But also, increasingly, our views on competitive democracy and how that can have adverse consequences, particularly in ethnic politics. If you work on the Global South, you see this very clearly in a number of contexts, where combative politics have been introduced prematurely and the outcome has been bad. I've seen this in my work on Rwanda and the liberal democratic model is not suitable for countries like it.

The final thing is freedom. Freedom and its value are foundational for someone like me who grew up in the West. However, I again feel that we've pushed too far, it's almost liberal extremism for me and we are seeing the limits of liberalism as we did with previous ideologies. There does not seem to be sufficient responsibility with the idea of giving individuals freedom, or a sufficient recognition of the trade-offs too much freedom can bring to order, both social and political. This is not an argument in favour of an authoritarian peace or anything. It's just an argument about understanding the limits of freedom, the dark side of freedom. Those are the kind of ideological shifts in my thinking.

The second area is methodological and possibly epistemological. Here, I think about my graduate school training in the early noughties when large-N cross national quantitative research in political science was the rage and very positivist. Over time, the new standard or expectation for scholars these days is clean causal inference and experimental and quasi-experiments are seen as the gold standard for making causal claims. This is a particular view of causation, which I think has limits. I've seen the limits of what I was trained in as a graduate student, this large-N quantitative work which looks at average treatment effects, largely correlational, all based on observational data. However, I've also come to see the limits of the kind of experimental and quasi-experimental work as well. One of the big areas of testing the limits is, of course, with external validity: Do results hold in other contexts? Imagine someone who is an Africanist, you see this very often with area studies, who will look at experiment results and say, oh, look what they found happened in Malawi. Look what we found that happened in Kenya. Okay, I can explain why that happened in Kenya and why that happened Malawi, but will that happen somewhere else? We very much doubt it. And they will be able to tell you, what are the macro contextual factors that explain that particularity or that graph, that the limits of external validity, the historical, the geographic the demographic factors, and it is largely this kind of a historical contextual approach to clean causal inference designs that I have taken an exception to. Even though I see the value of case studies, I see the limits with these too with selection bias issues. So, generally, I have become much more open minded about different methods or research designs, and their suitability for different types of

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problems as well as becoming more open-minded epistemologically and seeing the value of non-positivist approaches.

You have just released a book on the Rwandan genocide. What drew you to this topic in the first place? What drew your recent research focus for this book?

I was a law school student when the genocide happened, it almost passed me by, and so my interest came later, as a PhD student. There was this interesting book published by Peter Uvin called *Aiding Violence*. It was a study of donor relations in Rwanda before the genocide and how they may have unwittingly contributed to the genocide. I read that book and called Peter up. I remember sitting at my desk at the World Bank while he was a Professor at Tufts at the Fletcher School, and I try to think now how I would react if a student called me out of the blue. To his credit, he talked to me on the phone, to this complete stranger, for an hour and a half about his book. At the end of it, he said well why don't you do a PhD on this question about the origins of the genocide? So, I did. I applied to his school and I didn't get in. However, I got into the London School of Economics instead and Peter then decided to become an external supervisor for me and stayed on with me.

So that's how it began, this intellectual interest in the study of aid and conflict. It became much broader than that and became an interest in violence and the dark side of human nature. For me, the real reason was the nature of the violence. It was shocking because it was such an intimate, interpersonal violence with this remarkable cruelty, that was really shocking to me. But it was also a puzzle: how is that possible that so many people could do it? And that was more interesting to me than the question which a lot of people focused on, why the international community stood by and did nothing. That question is not so surprising to me, at least. To me, the puzzle is why so many ordinary Rwandans came to participate in the killing and that became the focus of the book.

What were your key findings from this book and how do these relate to previous scholarship?

This book is based on my PhD thesis. Unusually for an academic, I went back to the research eight years later and decided to publish it. I have to thank some fellow political scientists who said the work I did was kind of unique, and the data collected could probably never be collected again as I spoke to so many of the actual killers. You couldn't do that today in Rwanda, given the restrictions on access to foreign researchers. What the book does is answer two fundamental questions which are (1) how and why did it happen and (2) how and why did so many Rwandans, but not all, come to participate in it? The argument of the book is quite complex. There's a long causal pathway diagram that explains how the causal trajectory to the genocide begins with the baseline and explains the macro political phenomena that led to the genocide. But I'll focus on a couple of things.

First, one of the contributions is that we have this view of the genocide amongst political scientists that it occurred where there was this extremist elite that captured the state and then implemented the genocide using the machinery of the state. However, one of the things that surprised me was the agency from below that I discovered in my own work. It's not that it was entirely bottom up, but there was popular pressure from below. Indeed, what I concluded was that it was, in fact, this meeting of an elite level agenda with a local agenda that explains the outcome.

The second big finding was around the question of differential selection, why some killed but not others. Often, we've approached this from one of two perspectives, that it's either something unusual and dispositional about the killers or this other perspective that there's nothing unusual, they're just ordinary people in extraordinary circumstances. However, I came up with this third finding, not just dispositional, not just situational, but also relational. It wasn't just who you were. It wasn't just where you lived or the circumstances you faced. It was also who you knew that mattered. For the relational explanation, I show the social networks of the killers and I contrasted them with social networks of the non-killers, and I find this very clear difference. The social networks of the killers were somewhat counterintuitive as they were actually larger structurally, simply bigger than the social network of the non-participants. You often think about social capital as being a positive force but here actually you see the dark side of social capital. Knowing more people meant that you were more likely to be drawn into the violence. The mechanisms at work that I found were mostly about the forces, social influence forces, co-optation, behavioural regulation, monitoring, information diffusion. These are some of the mechanisms that explain why networks mattered for this relational explanation. It was not only

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dispositional, not only situational, but also relational forces, and a very complex interaction of all three types of factors that explained why some came to kill and others didn't.

The third big contribution is around trying to explain why Rwanda's violence took on extraordinary characteristics. Rwanda's violence was, even for a genocide, exceptionally quick and with an exceptionally large-scale mobilization. The geographic ambit was very wide. Why did we see this? I show that there are very unusual characteristics about Rwanda as a country, geographic and demographic. For instance, one of them is to do with the population density. As many people will know, Rwanda is one of the most densely populated countries in Africa and one of the most densely populated countries in the world. However, the reason that population density mattered is that in places where people live in such close proximity to each other, they live in very dense social networks, which mean that social forces are amplified. The forces of coercion, cooptation, and conformity are more powerful in a densely populated society, especially when that density is in rural rather than urban areas. Importantly, I don't expect to see the same outcome if it were urban. There's something very peculiar about rural sociology that is distinct because of the lack of anonymity, because of the multiplicity of the relations. It's very different to the kind of urban context where you can live in an apartment building and not know your neighbours for years and years.

You touched on this question of external validity. What implications do you think these findings may have for genocide studies more broadly?

This is a great question, and I have a couple of ideas here. First, is one about the power of ideas and ideology more generally. A long-standing debate is the role of structural material factors versus the role of ideational and ideological factors, and the debate has largely moved in favour of structural and material factors in the explanation of conflict. But in genocide studies the debate has moved the other way in the last few years towards ideational explanations, explaining the role of extremist ideas in shaping genocide because there is something distinctive about genocide; it's different from just ethnic conflict because, with genocide, the objective is to end the group. It's an eliminationist solution. Often, the argument in genocide studies has been that this is because of ideas, that there are these exclusionary ideological ideas that shape the decisions of ruling elites and lead them to make these calculations, that this elimination is the solution to the problem. While I agree somewhat with that, I'm pushing the pendulum back a little towards materialist factors. As I point out in the case of Rwanda: yes, there was an exclusionary, extremist racist ideology in Rwanda in 1994, but it wasn't the only ideology in the public sphere in circulation. There were other non-extreme, more moderate ideologies that also coexisted. This begs the question, as a good social scientist, what explains why some ideologies come to prevail over others? Why, in the case of Rwanda, did extremist ideas prevail over more moderate ideas? This is an important question in Rwanda, because if you were to just go back before the genocide and ask that question, what is the dominant ideology, it would not have been extremist exclusionary ideologies. So, we have to explain the competition between ideas and why extremists or moderate ideologies prevail over the other.

Where I come out on this question in the book is to say that, ultimately, what mattered in this context was that certain material factors explained why extremists came to win out over the moderates. In this case the extremists simply had bigger guns. When it came to the power struggle after the president's assassination, they controlled more of the coercive apparatus of the state, the presidential guard, the reconnaissance Battalion. This balance of power is important when we think about genocides and other contexts. When we think about the role of ideas and material factors we should be looking at the competition between them. It means that when we think there is a risk of a genocide or we see the risk factors, hate speech, the rhetoric, etc, all these things that we think of as indicators of a potential ethnic violence, we should then think about bolstering moderates and, therefore, the moderate ideas.

However, there are these tipping point moments, and there clearly was this tipping point moment in Rwanda that was missed, right after the President was assassinated. The UN saw Rwanda in two factions and decided not to take sides. It took them too long to work out that one side was bent on a genocide. So, ultimately, I conclude from a theoretical point of view that ideas do matter. However, while ideas are a necessary condition, they are certainly not sufficient. They work in conjunction with material factors because the material factors explain why certain ideas come to prevail over others. However, you certainly do still need those exclusionary ideas and, if you didn't have them, you could not get a genocide.

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Do these findings relate to radicalization?

Indeed, we often think of radicalization in the context of terrorism studies, but we can also think of it in terms of ethnic violence and ethnic conflicts and civil wars. We often see the polarization of communities with the radicalization of societies. The conventional wisdom is that polarisation predates the violence, that we get the radicalization first. Again, I draw on my interest in social psychology here to show that radicalization can also be a consequence of violence. Where behaviour comes first and attitudes follow. In practical terms, you might hate first and kill later, but you could also kill first and then hate later. The act of killing, the act of violence itself is transformative. You then need as a human being to justify your action and your behaviour, and your attitude shift becomes a form of cognitive dissonance reduction. You need to justify what you did to yourself and you begin to adopt these negative, extreme views of the outgroup. You begin to think, yes, they deserved to die because they did this. They are a threat. They have done this. So you develop all of these attitudinal poles, views or beliefs as a consequence of the active killing. This was one big thing that came out of my work, thinking of radicalization not just as an antecedent, but also as, I guess, something that comes with and that follows violence.

What were the profiles like amongst the perpetrators in the Rwandan genocide?

In my work I challenged this conventional wisdom about the orderliness of perpetrators. This is where I guess I challenge the social psychologists and all of these ideas where ordinary people can do these terrible things if they are just faced with certain circumstances. In fact, I found considerable diversity among the perpetrator body, and I had the advantage of being able to interview and profile a very large number of perpetrators. They were ordinary in one sense, in the very simple associate demographic sense. However, in terms of their attitudes or their dispositions towards the violence, they were very different; such as in their commitment to the violence.

I should flag that I am looking and observing this after the fact, so their commitments to the violence is difficult to observe beforehand, but I do try to get at this by triangulating interview testimony, what people said about these perpetrators and how they behaved before the genocide even happened. It was very clear that there was a variation or heterogeneity in commitments or dispositions towards ethnicities and the ethnic minority. Now, that shouldn't really be so shocking, really, as you think about this in our own society, why would it be so surprising that we would see the variation in societies in people's attitudes towards ethnic others? Ultimately, I just challenge the view that perpetrators are ordinary, this just over-predicts violence. Hence, we should not assume that everybody would do it just because we find that they are mostly pretty ordinary in some ways.

Where do you see your future research going?

I have been working on the converse question; what causes groups to come together, questions of ethnic cooperation and ethnic coexistence. Largely in political science, we focus on this kind of institutional explanation. We design systems, institutional designs, electoral systems, federal unit, systems to kind of deal with ethnic difference and to allow ethnic coexistence. However, I've been more interested in the microsocial explanations of how individuals and communities get along. For instance, I've looked at this in the context of Mindanao, in the Philippines, which has this deep fracture along ethnic and religious lines, primarily between the Muslim locals and the settler community, largely Christian. Now, this is not a very well-known case, but one nonetheless that I was very interested in, and I've been looking at these four big theories of cooperation and testing them:

- (1) Around elite persuasion: your leaders tell you we should get along. What if they tell you we should not get along?
- (2) Contact theory; that people spend more time together, so do they get along better?
- (3) Inequality reduction: if we reduce disparities between groups, are they more likely to get along? This has been the central focus of this project, on inequality reduction and its impact not just in the political and economic sphere, but the impact in the social sphere. I have a really interesting finding that it depends on whether you belong to a high status or low status group. High status groups tend not to select inequality reduction because it changes the social and political order against them, basically redistribution against their interest. Obviously, low status groups tend to

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like it because it means redistribution in their favour. So, I show that inequality reduction actually has these kind of perverse, both integrative and distancing effects, depending on whether you are a high status or low status individual.

(4) Superordinate goals: if you can find superordinate goals for groups to want to work towards, then will that cooperation actually overcome some of the differences that they experience or believe in?

That is one big project and another new area of study is in kleptocracy. My own home country is Guyana in South America, in the Caribbean, and recently Guyana has discovered oil and no one is optimistic about the long term prospects for the country, given the very weak political institutions. The country obviously faces real questions over some elements of the resource curse and how it affects the country. I'm also very concerned about corruption and leakage. I think kleptocracy is a very interesting area of corruption but when I look at the social science literature on this, there's a very strong focus on the agency of the kleptocrats in the Global South. There is much less attention on agency in the North. Thankfully that has changed somewhat in the last decade or so, because we started looking at the role of the bribe givers, not just the bribe takers. We see things like the OECD convention on Anti Bribery.

For my research, we started looking at professional intermediaries – lawyers, accountants, and bankers and what they doing. The spotlight hasn't been cast on them. I'm thinking about who helped these kleptocrats, these corrupt politicians and officials to move their ill-gotten gains from the Global South resource into the Global North? Do they do this wittingly or unwittingly? I should be very careful here because this is where lawsuits often happen. I'm not suggesting that this is a widespread phenomenon and that it is happening everywhere. Indeed, some are doing this more complicity than others, while some are doing this wilfully blindly and some are doing this just unaware that they are being instrumentalized in this way. Importantly, I guess if you have clients from Guyana and you suddenly have a politician, then you should be aware: How does this politician who has a salary of 50,000 pounds a year suddenly buy a 2,000,000 pound property in Hyde Park?

What is the most important advice you think you could give young scholars of political science or international relations?

I wish I could listen to someone giving this kind of advice when I was a graduate student. I suppose what I've learned, and I would tell younger graduate students and the newly minted PhD students, is don't be captured by fads or trends in the profession. It's easier said than done, obviously, because the incentives are so strong to publish in journals. You do not need to take on exclusively the political methods of the moment, believe in the importance of the problem before the method and believe in the importance of your own intellectual agenda. Then somebody looking at the trajectory of your research can see that it has been guided by an intellectual agenda rather than the opportunity to publish. Then they can see that you are a scholar who is motivated genuinely by the problem, they can see the arc of your research, and you can see that continuity in your thinking over time. Fundamentally, recognize that there is space and give yourself the time to have the confidence in your in your research designs, even if they may not be what everybody else is doing at that particular moment.