



JOHN J. MEARSHEIMER

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*John J. Mearsheimer is one of the towering figures in the International Relations scholarship. His magisterial book “Tragedy of Great Power Politics” reshaped theoretical landscape of the discipline by introducing Offensive Realism. Starting from the 1990s John J. Mearsheimer emerged as the most vocal and consistent champion of the Realist tradition in IR. He even received the unofficial title Mr Realism.*

*But even before that, in the 1980s, he had made an important contribution to the International Security Studies with his original research on conventional deterrence. He also published a biographical work on Basil Henry Liddell Hart, a major British military historian and theoretician of the early 20<sup>th</sup> century, as well as a book on the role of lies in international and domestic politics.*

*Throughout the last several decades John J. Mearsheimer has been actively involved in debates on the U.S. foreign policy in which he has not shied away from expressing views running against conventional wisdom. For example, in 1990 he was a strong supporter of the Operation “Desert Storm”, but in the 2000s he became a major critic of the American intervention in Iraq. During the 1980s, he emphasized that NATO deterrence of the Soviet Union was credible, but*

*after the end of the Cold War opposed the Alliance enlargement.*

*Publications by John J. Mearsheimer more than once caused heated debates. In this regard, the book on the role of the Israel lobby in the U.S. foreign policy that he co-authored with Stephen Walt produced resonated widely with American intellectual circles. In 2014, Foreign Affairs published another famous article by Professor Mearsheimer, which shifted most of the blame for the Ukrainian crisis onto the Western states.*

*John J. Mearsheimer graduated from United States Military Academy at West Point. He earned his doctorate at Cornell University. Starting from 1982 he has been teaching at the University of Chicago, where he holds the position R. Wendell Harrison Distinguished Service Professor. The latest book by Professor Mearsheimer called “The Great Delusion: Liberal Dreams and International Realities” came out in 2018.*

*In 2019, Igor Istomin, chief editor of the Mezhdunarodnye protsessy (International Trends) discussed with John J. Mearsheimer his research career, developments in IR theory and possible evolution pathways of great power politics. The interview was first published in Russian in the 2<sup>nd</sup> Issue of the journal in 2019. For the benefit of a broader audience, we republish it in English in full<sup>1</sup>.*

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<sup>1</sup> The journal expresses gratitude to Boris Nekrasov for his assistance in preparing interview for publication. The article includes a photograph of John J. Mearsheimer from his personal website (<https://www.mearsheimer.com/photos/>).

**I.I.:** *May I ask you, first and foremost, about how you decided to pursue an academic career?*

**J.M.:** Well, one would have never thought, if you knew me when I was young, that I would end up being a professor at the University of Chicago. I was not seriously interested in academics or international relations (IR) for most of my high school and college life. And in fact, I was mainly interested in sports. But when I was a junior in college – I was at West Point at the time – I took a mandatory course dealing with IR and I just loved it and loved the idea that there are IR theories that try to explain how the world works. And I decided right then and there that I was going to take more courses dealing with IR and related subjects in my senior year at West Point – which I did. I also decided that year that I was going to get a PhD in Political Science and focus on IR.

My first job after I graduated from West Point was in the Air Force and I was stationed in Los Angeles. I started going part time and then full time to the University of Southern California, where I got a master's degree in IR in 1974. And then in 1975 I left the Air Force and went to Cornell to get a PhD in Political Science. Once I was in that PhD program I discovered that I was actually quite good at coming up with theories and making arguments about how the world works; and of course, I loved what I was doing. It is very important to have great enthusiasm about the scholarly enterprise if you want to become a big time academic, which eventually became my goal. In short, I was really enthusiastic about the idea of becoming a scholar and I was good at theory, and those two factors together, I think, played a key role in helping me to become the person that I am today.

**I.I.:** *You already said that you had been in the military – as far as I understand you were first in the Army and then in the Air Force. And it was during the Vietnam War, when American society was badly divided. How did this historical period affect your scholarly perspective? Did it lead to your decision to go into IR?*

**J.M.:** I think that the Vietnam War affected my scholarly perspective in profound ways. It was not responsible for me becoming an IR

scholar. But it definitely influenced how I think about the world. I was in the American military from 1965 to 1975. I was an enlisted man in the army from 1965 to 1966 and then I went to the West Point from 1966 to 1970. After graduating, I was in the Air Force from 1970 to 1975. My time in the military was coterminous with the Vietnam War. The first American combat units landed at the Da Nang on March 8, 1965. I went into the American Army as an enlisted man on June 22, 1965. Saigon fell in the spring of 1975 and I left the Air Force shortly thereafter in the summer of 1975. So, my experience in the military and my life as a young man, or even as an older boy, was profoundly influenced by the Vietnam war, which, as you correctly point out tore the United States apart. The late 1960 and early 1970s were an incredibly contentious time in the United States. What I learned during those years that has had a marked influence on my thinking about IR, is that great powers like the United States and the Soviet Union should stay out of wars in places that we used to refer to as the Third World or the developing world.

I learned from the Vietnam War that you do not want to invade and end up occupying countries like Afghanistan or Iraq, which is why I was deeply opposed in 2002–2003 to the US invasion of Iraq. I thought it would end up looking much like our experience in Vietnam. And by the way I thought the same thing regarding Afghanistan. I understand that in the wake of 9/11 it was almost impossible for the United States not to invade Afghanistan. Nevertheless, I thought that once we had toppled the Taliban, we should have gotten out as quickly as possible. Otherwise, we would end up in the same situation that the Soviets ended up in when they went into Afghanistan in 1979. I might add that when the Soviets invaded Afghanistan, most people in the American national security community were shocked and thought that this was evidence that the Soviets were on the march and that they were going to shift the balance of power in their favor and against the United States. And at first it looked like the Soviets were going to win a quick and decisive victory in Afghanistan. My view at that time, however, was that the Soviets had made a

huge mistake and actually they had jumped into a giant quagmire, just like we had done in Vietnam. Of course, that proved to be the case and the Soviet military suffered a humiliating defeat in Afghanistan, just like the British had in the 19<sup>th</sup> century, and the Americans eventually will in Afghanistan. The bottom line is that great powers like the United States should stay out of places like Afghanistan, Iraq, and Vietnam. And actually, if two great powers are engaged in security competition, each should hope the other invades one of those smaller countries. All of this is a long way of saying that the Vietnam War taught me that it is very important for great powers to avoid invading and occupying countries in what we used to call the developing world or the third world.

**I.I.:** *Much of Cold War security studies was dominated by concerns about nuclear weapons. However, your first book was devoted to "Conventional Deterrence". How did you come around to writing about this topic?*

**J.M.:** Let me make two points the first of which deals with the background for my decision. It is important to understand that by 1975 when I started graduate school at Cornell University, there was not much new to say about nuclear deterrence. From 1945, when the first nuclear weapon was exploded, up until roughly 1965, an enormous amount of intellectual capital was spent in the United States, as well as other countries, studying nuclear deterrence and nuclear strategy. By 1975, there was just not much new to say about nuclear issues. So, anyone who started graduate school in 1975 was unlikely to work on nuclear deterrence. At the same time, there was hardly any literature on conventional deterrence. Indeed, I believe that my dissertation on "Conventional Deterrence", which eventually became my book with the same name, represents the first time that any scholar, policy-analyst, or policy-maker used the term conventional deterrence in an article or a book. I think I was the person who first introduced that term to the security literature.

This brings me to my second point concerning how I ended up writing "Conventional Deterrence"? It was really a case of pure dumb

luck. I took a course called "Strategy" in my second semester at Cornell with Professor Richard Rosecrance. This was during the spring semester of 1976. And in the first class he said: "Here is a list of possible topics that you can write papers on". One of the topics he listed was conventional deterrence. Anyway, when the class was over, I had a meeting scheduled with Professor Rosecrance. I went to his office, and he said to me as soon as I walked in: "John, what are you going to write your paper on for this course?". I hadn't given it any thought. The first topic that popped into my mind for reasons I cannot explain was conventional deterrence. So, I said to him right there without thinking about it that I was going to write my paper for the course on conventional deterrence. I believe he gave me an A- grade for the paper and I think I got an A- for the course. Anyway, that summer (this was the summer of 1976, when the course was finished) I ran into Professor Rosecrance on the Cornell campus and he said to me: "What have you done with the paper you wrote for me on conventional deterrence?" I said that I haven't done anything with it. He replied, why not? I said: "Well, you gave me an A- on it, so obviously it was not that good a paper". He said: "On the contrary, it was a brilliant paper." He said: "I only gave you an A- because I didn't want you to get a big head!". I was actually shocked, but anyway once he said the paper was terrific I began to work hard on that subject and I then decided to make it my dissertation topic. It was actually a brilliant suggestion on Professor Rosecrance's part, because, as I said to you earlier, nobody had written anything on the subject; and as you know, Igor, it is very important to choose topics that people have not written much about or anything at all. In sum, I was just plain lucky to end up writing about conventional deterrence, a subject that had virtually no literature, and one where I could use my ability to create theories and make interesting arguments. It was really the ideal way to start one's career.

**I.I.:** *Your most famous work, "The Tragedy of Great Power Politics", develops the theory of offensive realism, which is a more general theory of*

*IR than the theory in "Conventional Deterrence", which has a more limited focus. How did it come to pass?*

**J.M.:** This is another very interesting question about the trajectory of my career. When I began graduate school, I was not very knowledgeable about what is sometimes called grand IR theory. This would include the standard liberal, realist and Marxist theories of the day. I paid them some attention in graduate school, but not a lot, mainly because I got deeply interested in conventional deterrence, which was a middle-range theory. It was not grand theory, although I was certainly dealing with an important theoretical issue. I focused on that middle range theory for almost all the time I was a graduate student at Cornell, which was from 1975 to 1980. And then from 1980 to 1982, when I was a post-doctoral fellow at Harvard's Center for International Affairs, which was then run by Sam Huntington, I focused on transforming my dissertation on "Conventional Deterrence" into a book. In the fall of 1982, I started teaching at the University of Chicago, which is a very theoretical place. It is an institution that places great emphasis on big theories. So, when I began teaching in the 1982–1983 academic year, one of the first courses I taught was a seminar on IR theory. And as hard as it might be to believe, that was the first time – in the winter quarter of 1983 – that I read Waltz's seminal book – "Theory of International Politics". That book came out in 1979, but here it is in 1983 when I am first reading it. Why? Because before 1983 I was mainly thinking about conventional deterrence. But now I am at the University of Chicago thinking about grand theory in a serious way for the first time. That was the beginning of a journey that led to "The Tragedy of Great Power Politics".

**I.I.:** *When and why did you start to question Waltz's version of structural realism?*

**J.M.:** The truth is that when I first started teaching Waltz's "Theory" book, I found it hard to figure out exactly what his argument was. That was true for probably the first three times I taught the book. So, in the beginning, I was just trying to understand his theory, not

criticize it. At the time, many scholars thought that Waltz's version of realism was very offensive in nature. But I began to understand slowly and steadily that his theory was actually quite defensive in nature. It did not say that the structure of the system encourages states to pursue aggressive policies abroad. Indeed, it said the opposite. But something even more important happened sometime in the mid-1980s. My good friend Jack Snyder, who was teaching at Columbia, was writing his book: "Myths of Empire". Jack asked me to read his theory chapter, which is pure, unadulterated defensive realism. It's a much clearer version of defensive realism than Waltz's. I read Jack's theory chapter carefully to give him comments and I was very dissatisfied with his argument, because I thought it did not capture the fact that great powers are essentially aggressive actors in a realist world. That led me to start thinking more and more about Waltz's theory and to recognize that he too was ultimately a defensive realist. I then decided that I would write a book on realism that laid out the case for what would later be called Offensive Realism. So, in a very important way, "Tragedy" was a response not simply to Ken Waltz but to Jack Snyder as well. The first piece I published that reflected my realist perspective was "Back to the Future", which I wrote in 1990 as the Cold War was ending and we were entering a new world. And then, of course, "The Tragedy of Great Power Politics" was later published in 2001. I might add that some of my key ideas about offensive realism were laid out in "The False Promise of International Institutions", which was published in the mid-1990s.

**I.I.:** *You actually began establishing your credentials as "Mr. Realism" just as the Cold War was ending, which was a hard time to make the case for realism and when other theoretical perspectives were gaining ground. How is it that you remained committed to realism? Why weren't you tempted to become an institutionalist or a constructivist, or follow some other theoretical approach than realism?*

**J.M.:** There is no question that from roughly 1989 up until I would say about 2017 it was hard to be a realist, especially in the United

States, or more generally in the West. Francis Fukuyama's liberal perspective, which was reflected in his famous article, "The End of History", dominated Western thinking after the Cold War. So, when I made the argument – starting in 2001 in "Tragedy" – that China could not rise peacefully, most people thought that my claim was not serious. Some people would laugh at me and say that I had become a dinosaur – that I didn't understand that the world has undergone a fundamental transformation. I disagreed, of course. I believe that we live in a realist world and that realism does an excellent job of explaining how international politics works. So, I maintained a strong realist perspective throughout the period from 1989 up to 2017 and I think that was the period when some people came to refer to me as "Mr. Realism". Others, I might add, liked to refer to me as "the Prince of Darkness" because of my realist views.

If I could make one additional point that relates to your question, but I am sure we will talk about it later. As the post-Cold War period wore on and we moved into the early 2000s, I began to realize that realism does not do a good job of explaining the behavior of the single great power in unipolarity, which is how the world was structured at the time. As almost everyone agrees, the period from 1989 to 2016 was the unipolar moment, and the one great power was the United States. In that setting, it was impossible to talk about great power politics because there was only one great power. You need bipolarity as we had during the Cold War or multi-polarity, as we now have, for great power politics to be relevant. After all, you need two or more great powers to interact with each other to have great power politics. So, what I came to understand by the early 2000s – that I did not understand in the early 1990s – is that realism did not tell us much about how the United States acted in the unipolar moment. Specifically, it did not act according to the dictates of realism, because it did not have to worry about the balance of power, as it was the only great power on the planet. So, I wrote "The Great Delusion" as a way of explaining American foreign policy in unipolarity. My argument in that book, as you well know, is that

the United States behaved according to the dictates of liberalism, not realism, during the unipolar moment. The United States took a holiday from realism – because it had no great power competitors. So, my argument is that realism does an excellent job of explaining US-Soviet behavior during the Cold War because it was bipolar world. There was great power politics and realism explains in large part how the superpowers interacted with each other. With regard to the world that we are now moving into, which is a multi-polar world with Russia, China and the US as the three great powers, we are again in a situation where realist logic provides a powerful explanation for how those three states will interact with each other. But with regard to the unipolar moment, I do not think that realism tells us a lot about how the United States acted during that period, because great power politics was off the table.

**I.I.:** *Let me ask you a related question. What about other states in unipolarity? The United States might not have been behaving in accordance with realism, but was that true of the other states in the system?*

**J.M.:** This is a great question. Of course, most of the other countries in the system were acting according to realist dictates, especially the two key major states, China and Russia. It was the United States, the sole great power in the system that was not behaving according to Realist dictates. But let's unpack this a bit by talking about US-Russia relations in the context of NATO expansion, EU expansion, and the "color revolutions," which were all aimed at Eastern Europe. I believe that what the United States and its European allies were doing with these three measures was pursuing a liberal policy – I call it liberal hegemony. They were spreading NATO eastward, they were spreading the EU eastward, and they were promoting the "color revolutions" – all for the purpose of turning Eastern Europe into a sea of liberal democracies that were hooked on capitalism and integrated into all the key institutions that the West had created during the Cold War. I do not believe that NATO expansion was aimed at containing Russia, which would reflect a realist policy. Some people now argue

that NATO expansion was all about containing Russia, but there is little evidence that was the case. So, what I'm saying is that the West was operating according to a liberal playbook.

The Russians, on the other hand, were acting according to realist principles. The Russians saw NATO expansion, EU expansion, and the "color revolutions" as a direct threat to Russian security — as they should have. In essence, the two sides were playing according to different playbooks, which is one of the principal reasons that a major crisis erupted over Ukraine in February 2014. The Russians were operating according to realist dictates, while the Americans and their European allies were operating according to liberal dictates. The end result was that the Americans and their European allies were completely surprised by the crisis that broke out on February 22, 2014. If they had paid attention to basic realpolitik, they would not have been surprised by Russia's reaction to NATO expansion.

**I.I.:** *I would like to follow up this discussion by asking you to say more about which countries in the system are motivated by the offensive realist logic. Your book is called "The Tragedy of Great Power Politics" — so it deals primarily with great powers. But to what extent does your version of realism apply to the polices of small and medium-sized states? After all, you just described China and Russia as major states and said that Russia acted according to the realist playbook during the unipolar moment. Furthermore, your co-author and fellow realist Stephen Walt has developed his own realist theory to explain the behavior of small and medium-sized states in the Middle East. So, what is the relevance of your theory for states that are not great powers?*

**J.M.:** I am embarrassed to say that I don't have a good answer to this question. This is a subject that I should think hard about and write about. There is no question that I focus mainly on great powers in my realist works. And I make it clear that the theory is principally about great power politics. At the same time, there is no question that there are many instances where major powers and minor powers — those are two categories that I distinguish from great powers — act according to realist

dictates. Some good examples during the unipolar moment would be China and Russia, which were major powers, and Iran and North Korea, which were minor powers. There is little doubt that realist logic almost always applies to major powers, but it does not always apply to minor powers. Now, the question is: when does it apply to minor powers? If we are talking about Belgium and Luxembourg, realism doesn't tell you much about their behavior, mainly because they are tiny countries surrounded by countries like France and Germany that are far more powerful. But if you look at an area like the Middle East, which is the region Steve Walt focuses on in his book, you see that realism applies there, even though no great powers are physically located there. The Middle East is mainly filled with minor powers acting according to realist logic. So, Steve nicely shows that a realist template is very useful for explaining the international politics of the Middle East. It would probably make sense if somewhere down the road I systematically explored how my realist theory applies to major and minor powers.

**I.I.:** *In many of your lectures and writings, you claim that theory is a crucial element — maybe the crucial element — of the academic enterprise. However, many scholars find it difficult to come up with new theories. What is needed to create a solid theory?*

**J.M.:** I have a number of points to make on this key issue. First, it is important to understand that some people are naturally good at coming up with theories, while others are not. It is like sports. Some people are good at soccer, but others are not. Some people are good at swimming, while others are not. Not all IR scholars are natural theorists. In effect, I would argue that only a handful of IR scholars who I have known over the years are really good at developing their own theories. Second, you can only have so many dedicated theorists in any particular field and this includes IR, simply because if everyone was primarily a theorist and committed to coming up with his or her own theory of international politics we would have so many theories that we wouldn't be able to keep track of all of them. In short, only a

finite number of people are good at theory and we only need a finite number of people who are seriously devoted to developing theory. Third, developing sound theories requires an enormous amount of work, even if you are really good at that enterprise. It is a long and difficult process and it invariably involves a lot of anguish and pain. It is much easier to test a hypothesis drawn from an existing theory. You find or construct a database and then you test your hypothesis with a few cases or some high-tech statistical methods. Or you can run an experiment. But these enterprises are not as difficult as coming up with a novel theory.

Let me illustrate my point with some words about how long it took me to develop Offensive Realism. As I said earlier, I first started thinking seriously about IR theory and Realism in particular during the winter of 1983. But it wasn't until 2001 that "Tragedy of Great Power Politics" was published. In other words, it took me about 18 years to come up with a full-blown version of Offensive Realism. It was a long and painful process. And what is paradoxical to me about this process is that the theory is actually quite simple, which is the main reason it has been paid much attention all around the world. One might think, therefore, that it was easy for me to quickly come up with my theory. But again, that was not the case.

Let me tell you another personal story that hopefully further illuminates my point. When I first started thinking about coming up with my own theory, I did not include uncertainty about other states' intentions among the theory's underlying assumptions. Of course, that assumption is of enormous importance for making my theory work. I remember having lunch with a graduate student sometime in the mid-1990s to talk about my theory. He said to me during our meal: "You know, it is clear from listening to you talk that uncertainty about other states' intentions is embedded in your theory, but you're not paying it much attention. I listened carefully to him and I remember when I was walking home after lunch and thinking about our conversation, saying to myself, "He is exactly right about the need to put my point about intentions up in bright lights". So, I went home and re-wrote the theory so

that uncertainty about intentions was one of its principal assumptions. In retrospect, it seems quite amazing that I took so long to recognize the critical importance of that assumption. There is a handful of other key arguments in "Tragedy" that also took me years to figure out.

Again, my basic point is that coming up with a theory on any subject is not easy to do, even if it seems like it should be easy, given that good theories are simple by definition.

**I.I.:** *In your article with Stephen Walt on "Leaving Theory Behind," and on many other occasions, you emphasize that IR scholars today are preoccupied with methods and hypotheses-testing at the expense of theorizing. What is the right balance between developing new and testing existing theories? Furthermore, given that you are a prominent voice in academia, do you see any evidence that your criticism is causing the pendulum to move back toward greater emphasis on theory?*

**J.M.:** I think it is hard to pinpoint exactly what the right balance is. But you want to avoid a situation where IR scholars become committed to developing theories without any interest in testing them. You also want to avoid a situation where scholars dismiss theory as basically a waste of time and argue that testing hypotheses is what real social scientists should be doing. In the late 1980s and early 1990s, when rational choice or formal modeling was all the rage in Political Science, some IR scholars who worked in that realm had little interest in testing their models. And they were appropriately criticized for that approach. Again, you do not want a situation where scholars are developing theory for theory's sake. We want to develop theories and then test them against real world events to see how much explanatory power they have. But what happened in subsequent years – and this was clear by the first decades of the 2000s – is that we moved so far away from theory that we privileged empirical testing and experiments in ways that I don't think are healthy. This state of affairs is what caused Steve and me to write "Leaving Theory Behind". We tried to make the point that coming up with new theories and repairing or altering existing theories is terribly important –

as is testing those theories. We went to great lengths in writing that article to make it clear that we think that empirical methods and testing theories is an essential part of the social science enterprise. But at the same time, we must not lose sight of the importance of theory.

**I.I.:** *Coming back to the matter of IR theory, we have Waltz's theory of Defensive Realism and John Mearsheimer's theory of Offensive Realism. What are future prospects for developing new realist theories? Is there any space for innovative thinking about Realism?*

**J.M.:** This is a very hard question to answer, because if I thought there was a new realist theory that one could invent, I would either invent it myself or encourage an especially smart person to pursue that idea. I think that any theory — whether it is Waltz's, Morgenthau's or mine, to take three prominent realist theories — has problems. There is no such thing as perfect theory. Therefore, what will happen over time is that future scholars will find different ways of building on those realist theories and come up with new ones. To illustrate my point, I would note that in my case I stood on the shoulders of Waltz. In other words: I read Waltz, I was dissatisfied with his theory, and I set out to alter it in some fundamental ways and come up with a different realist theory. I would imagine that in the future scholars will read my work and Waltz's as well as the work of other realists like Jack Snyder, Steve Walt, Steve Van Evera, Charlie Glaser, and they will like certain aspects of those works and dislike others. They will then put together their own theory — drawing on those existing theories. But I am incapable of telling you what those new theories will look like. We just have to wait and see.

**I.I.:** *Let me ask a more specific question about the future of the Realist theory. Both your theory and Waltz's theory focus on the distribution of capabilities in the system. Many of the younger realist scholars, however, look at how changes in the balance of power affect the behavior of great powers. They pay special attention to the relationship between rising powers and declining powers and what that means for the prospects of*

*a major war. Do you think this new emphasis on developing dynamic theories of change is a promising direction for Realism to take?*

**J.M.:** There is a rich tradition in the realist literature of looking at dynamic change in material capabilities as a cause of war. Robert Gilpin became very famous making this argument. Indeed, his most famous book is called "War and Change". Ken Organski and Jacek Kugler, as well as Dale Copland, a former graduate student from the University of Chicago who now teaches at the University of Virginia, all wrote important books about how shifts in the balance of power lead to war. Here, too, I think that future scholars will build on this body of work and develop new theories about how dynamic change influences the likelihood of war. I believe that scholars like Waltz and me come at structure from a somewhat different perspective: we tend to focus more on what the existing distribution of power looks like rather than how it changes. Although there is room in both Waltz's theory and mine for change, that concept is not emphasized anywhere near as much in our theories as it is in Gilpin and Copland's theories.

**I.I.:** *How do you assess the IR field today, overall?*

**J.M.:** This is fascinating question. Let me answer it by comparing the IR field today with the field as it existed when I first started my PhD in 1975. I will make four basic points.

First, there are many more students studying for MA and PhD degrees in IR today than there were in 1975 when I began at Cornell; and this is especially true in security studies. The security field in the United States had been badly damaged by the Vietnam War. Thus, there were only a tiny number of security scholars in the United States in 1975, which was actually the beginning of the renaissance of that field. And there were not many security scholars outside of the United States. That situation has fundamentally changed. There are many first-rate security scholars in countries all over the world, including the developing world. It is really quite amazing how many more people who have been studying and teaching security today than we had in 1975.



The second big change is that globalization has created a situation where IR students all over the world are familiar with the same IR literature and effectively speak the same language. Everyone knows what the security dilemma is. I like to say that we security scholars live in a global village when it comes to how we talk and think about international politics. So, when I give a talk in Japan or Turkey or Russia, I find that the students and faculty in those countries fully understand all the concepts I employ and the arguments I make. And when the students ask questions from the audience, I almost always understand what they are talking about because again we are all reading the same works and speaking the same language.

Sometimes when I go to foreign countries to talk, my hosts will ask me what it is like taking questions from a Turkish or Japanese audience. I always say: "It's just like taking questions from an American audience, because these students here in Turkey or in Japan have read the same literature and effectively speak the same language as US students". As I said, we all know what the "security dilemma" is, we all know how uncertainty about intentions can lead to security competition among great powers, and so forth and so on.

The third development, which is probably more true of the United States than any place else, is that there is much less interest in policy issues among IR scholars at the top universities than there was in the mid-1970s. I would argue that back in the 1970s and 1980s, and even into the early 1990s, many IR scholars saw a close connection between IR theory and the policy world. They were seen as bound up with each other. Now, most young IR scholars, especially at elite universities, are not interested in engaging with big policy issues and engaging with the real world more generally, certainly compared to the way things were when I was beginning my career.

The fourth big change – and I think it is global in scope – is that the study of IR has become highly professionalized. IR scholars today view themselves as professional political scientists, a development that I think is closely tied up with the great emphasis we see today on

methods. You get training in methods so that you can become a scientist, a political scientist – you are part of a real profession. I think professionalization was nowhere near as pronounced back in the mid-1970s as it is today. And not surprisingly there was much less emphasis on methods back in the day than there is today. In the past, one was much more interested in becoming very well educated on a wide variety of substantive political issues so that you could think deeply about international politics not only in terms of the academic debates of the day, but also in terms of the policy debates. If you came up with an interesting argument or theory in those days, you would be anxious to spread the word in the policy world as well as academia. I think that IR scholars today have much less interest in engaging with the policy world. They are mainly interested in talking to each other, and that is because of professionalization.

So, again, I am making four general points. First, the number of IR scholars – especially security experts – is much greater today than it was when I started graduate school. Second, students of international politics operate in a global village. Three, IR scholars are much less interested in engaging with the policy world, especially in the US, than they were back in the mid-70s. Finally, professionalization has come to have a profound influence on how we do business.

**I.I.:** *Through the years, you have made a number of bold predictions that ran against conventional wisdom. They often seemed to have little impact on the government's decision-making process, although your predictions often proved to be right. To what extent does academic expertise affect a country's foreign policy?*

**J.M.:** It is a very difficult question to answer because it is hard for any scholar to have a good sense of how much influence he or she has on the policy-making process. There is no question that my outspoken opposition to the 2003 Iraq war had virtually no impact on the Bush Administration's thinking about whether to invade that country. The opponents were at most a speed bump on the road to war. The Bush Administration just ignored us. But, there

are instances where IR scholars have meaningful influence. One of the best examples of academics influencing policy involves the democratic peace theory. Hardly anyone had ever heard of the democratic peace theory until Michael Doyle wrote about it in a famous article in 1983. The idea that democracies do not fight other democracies quickly gained traction in the policy community and has had a profound influence on American foreign policy since the Cold War ended in 1989. That simple theory, which was barely known before 1983, came out of academia. While Michael Doyle was the first scholar to push that argument forward, others like Bruce Russett followed in his footsteps. Moreover, the democratic peace theory sits at the core of Francis Fukuyama's famous and influential article – "The End of History". This is surely a case where academics have had significant influence.

In my own case, I wrote a controversial article in 2014 on the Ukraine crisis, which broke out that year and has had poisonous effects on relations between Russia and the West. I argued that the crisis was largely the result of the West's policy of expanding the EU and NATO eastwards, coupled with promoting the "color revolutions" in Georgia and Ukraine. Moscow was not responsible for precipitating the trouble over Ukraine. I believe my article influenced the thinking of a good number of people about who was responsible for causing that crisis. I base that claim on numerous conversations with people who have told me that their views on the matter had been shaped by my article. Now, has the article helped to change the West's policy toward Russia? I think the answer is clearly no. But it may contribute in a small way down the road to working out a solution to that crisis, although I would not bet a lot of money on it. Moreover, even if the article eventually has that kind of influence, it is not clear we will be able to detect it, as the spread of ideas usually works in subtle and opaque ways.

**I.I.:** *For realists, who place little emphasis on the importance of ideas and instead emphasize the influence of the structure of the international system on state behavior, how can the ideas of IR scholars affect foreign policy?*

**J.M.:** Well, to be clear, there were no great power politics during the unipolar moment, simply because there was only one great power in the world – the United States. There were no other great powers it could interact with or had to worry about as a potential threat. In this world, the United States was free to ignore the balance of power and pursue an ideological foreign policy. Indeed, it pursued liberal hegemony, where it was determined to remake the world in its own image between 1989 and 2017. This was a world in which ideas mattered greatly, but it was not a realist world, at least for the United States. So, there are instances, although they are rare in international politics, where a great power's foreign policy is motivated mainly by ideas. But when the international system is either bipolar or multipolar, great powers are motivated mainly by concerns about the balance of power, not ideas. This was the case during the Cold War and in the multipolar world we are now entering. Structure is largely determinative for great power behavior when there is more than one of them in the system. Save for the unipolar moment, I believe that the United States has acted according to realist dictates. For example, if you look at how the United States is now behaving toward China and Russia, it is clear that we are back in a realist world and that liberal hegemony has come to an end. Some people would say that this is because Donald Trump is in the White House. I don't think so. The main reason that liberal hegemony is finished is because the system has gone from unipolar to multipolar, where realism takes over and ideas take a backseat.

**I.I.:** *Your latest book – "The Great Delusion" – deals with liberal hegemony, America's foreign policy during the unipolar moment, which you have been criticizing for many years now. However, many scholars claim that the days of US predominance are numbered and hence attempts to establish liberal hegemony are doomed to disappear anyway. So, what motivated you to write your new book?*

**J.M.:** I wrote "The Great Delusion" in large part to understand why the US foreign policy was such a disaster during the unipolar

moment. In the first decade after the Cold War ended, there was a tremendous amount of optimism in the United States – and in the West more generally – about the future of international politics. Hardly anyone foresaw all the trouble that we face today. Most people thought that peace was breaking out and that the world would become more and more peaceful as time went by. But something went badly wrong and that was especially true with regard to American foreign policy. Just consider the horrible mess the United States helped create in the greater Middle East. Moreover, look at the terrible state of US relations with Russia, and the failure of America's attempt to “engage” with China. I wanted to explain these failures. I wanted to know what went wrong between 1989 and 2017. I wanted to explain the US foreign policy during the unipolar moment. What I eventually discovered is that Realism really does not explain American foreign policy during that period, mainly because the United States was so powerful that it did not have to worry about the balance of power. Thus, I came to understand that American policy was motivated by liberal ideology. So, that's what “The Great Delusion” is all about.

**I.I.:** *And perhaps, if I may, the last question. You have discussed future relations between China and the United States at great length in “The Tragedy of Great Power Politics” and other publications. You have also discussed Russian – American relations during the unipolar moment, when American policy-makers were pursuing liberal hegemony. How do you assess the current dynamic between Moscow and Washington and do you have any of your usual bold predictions on where that relationship is headed?*

**J.M.:** I think it is deeply regrettable that the United States now has terrible relations with Russia. I do not think that Russia is a threat to the West and I also believe that the United States and its European allies are mainly responsible for this hostility, which has pushed

the Russians into the arms of the Chinese. The troubles are largely a consequence of the Ukraine crisis. I believe, however, as a good structural realist, that Russia will eventually end up aligning with the United States against China, if China continues its impressive rise. I think that the idea of Russia aligning with the United States against China will make sense, because Beijing is a greater threat to Moscow than Washington. The main reason is geography: China shares a border with Russia and has historically had bad relations with Russia. Of course, Russia does not fear China more than the United States today, but that is because China is not yet all that powerful and also because the United States and its allies have poisoned relations with Russia, thus causing it to ally with China. But I think that situation will reverse itself with the passage of time. By the way, we had a similar situation before World War II. The United States refused to recognize the Soviet Union until 1933, and even after that it was hostile toward Moscow throughout the 1930s and the early years of World War II. But that all changed rapidly after Nazi Germany invaded the Soviet Union on June 22, 1941 and especially after Hitler declared war against the United States on December 11, 1941. Both Moscow and Washington were then at war against Hitler's Germany – a common threat that forced them to become close allies throughout World War II. Thus, those who believe that it is almost impossible to imagine the United States and Russia allying against China, should remember how the rise of Nazi Germany affected US-Soviet relations. Great powers do what they have to do to survive. And I believe that from both an American and Russian perspective, it will make good sense to join forces against China if Chinese power grows steadily in the years ahead.

*Thank you so much. It seems like a perfect ending for a very thought-provoking and greatly inspiring interview.*