I enjoyed the way in which you organized your own response comments, Ted, and was going to follow suit (with lots of mea culpas and yeps all around), but decided to organize my response thematically instead. Before turning to those themes, let me just say a word about context.

Ted comments at one point that I do a good job going after the neoliberal institutional (NLI) argument. This reminded me that while I had you read my realist-constructivist chapter, when it started out as a dissertation lo those many years ago, my main interest was in whether bureaucrats really behaved in the manner implicitly expected by NLI (and of course they didn't). The realist ideas and contributions followed rather than proceeded that critique, and the constructivist elements were the last to be incorporated.

I tell you this not as an excuse for the chapter's flaws, but as context for why I wanted you to read this chapter in particular. It was the least developed chapter of the manuscript, as much of it was written in the final pre-publication stage. Mind you I'm not complaining, since, without the publisher's speedy turn-around time demands, I would have failed in my tenure bid at UConn. But I cringed as I sent this chapter off, because I knew it had problems, and I didn't have time to fix them before it was going to be published. The upshot is that I've gotten very little feedback on this chapter in comparison to some of the others. Since I would like to further develop the ideas in it, I really appreciate that you were both willing to wade through this overly-long, eclectic, and in-desperate-need-of-at-least-one-more-revision chapter in order to give me comments. OK, let me turn to the thematic discussion.

**Autonomy**

Both of you picked up on the problem of autonomy in my argument, and Rawi, your comments about my attempts to redefine cooperation and the centrality of autonomy to that redefinition were dead-on. As you pointed out, in slamming the standard IPE dichotomy between cooperation and autonomy, I suggest that "the number of real situations in which the dichotomy is actually false is the majority," yet you could only think of one example and that's international monetary policy-making, which was my case study. Hence I have "built an entire, extremely sophisticated and elegant theoretical structure around an important, but unusual, stylized fact."

Yep…that’s pretty much what I thought too by the end of the book, and I really appreciate your having read the last chapter to pick up on my own reservations on this score. Worse yet, by the end of the book I felt I had done what Ido suggests in his discussant comments: I had written a book presupposing it was possible to generalize about cooperation, but which was really about US cooperation and reflected my "own embeddedness in a national and historical context."

Hence rather than offering a generalizable theory about cooperation, what I really had was a book about the on-going project of US autonomy, and it should have been written that way (although I am also loathe to produce yet one more navel-gazing volume by an American scholar). Both you and Ted also point to the dilemma that I continue to grapple with -- I don't
think the US is unique with regards to cooperation as a reification of state autonomy, yet empirical events continue to prove me wrong. It was extremely vexing to have the EU insist upon monetary union as I was writing a book that suggested such cooperation was all so much fluff and cover for state bureaucratic consolidation. I lost sleep worrying about how I was going to explain the EU, and in the final countdown to publication said, to hell with it, I need to get tenure. But your comments, Rawi, about France and monetary union are well taken.

I do think there is a case to be made, however, that within the context of the US the argument about autonomy does apply to other issues areas and is not exclusive to monetary policy-making. Environmental policy-making seems a case in point -- everything from the Kyoto protocols to UNCLOS suggests that the Americans do not respond to what is functionally efficient in the issue area and rely instead on cooperation as a mechanism for maintaining business as usual in the institutional strands of the state. So although the book was not written in that way, I think it may be possible to generalize the argument to other policy-making arenas in the American state.

In addition, although I recognize that there is a lot working against my ability to generalize about autonomy, I still think there is something to the argument that autonomy, as an institution, is a generalizable part of the present global political landscape. I managed to generate my own problems in this regard, because I was constantly conflated autonomy as institution with autonomy as policy (and using the latter as the empirical evidence for the former). So it’s relatively easy to counter my argument with questions such as: How come France gave up autonomous monetary policy-making? Or how come EU nation-states abide by decisions handed down by the European Court of Human Rights? And so on.

After thinking about this problem, I realize what I needed to do was keep institutions and policies separate to a greater extent. What I should have argued or made clearer is that the structure of the nation-state is such that it institutionalizes autonomy of decision-making. It does so by compartmentalizing territory, people, issue areas, transnational problems, and decision-making via bureaucratic structures, legislative structures, electoral structures, educational systems, medical systems, and so on. Within this list, my main focus has been on bureaucracies, because I concur with Iver in his response comments that "the study of bureaucratic action is often of key importance" and it’s been a disciplinary mistake to bracket them. And to really figure out whether autonomy as institution is generalizable to other states, I would need to take each case and study not just how decisions are derived from internal bureaucratic structures, but also how these structures are effected, if at all, by the decision to cooperate.

In this regard, the dissertation this book was based on also examined Japanese monetary policy-making, German and British agricultural policy-making in the EU, and German and British foreign policy-making in the EU, and in each of these instances I found a similar pattern. That is, in each case leaders and bureaucrats all talked the talk of cooperation, and meanwhile the bureaucrats worked very hard to ensure that decision-making remained firmly in their department's hands and not someone else's. There was even collusion in that foreign counterparts would work together to ensure that at home they each retained control of their own spheres of decision-making. Hence cooperation was reifying the state's institutions, not dissolving them, but to make this case empirically, I would need to examine internal politicking, bureaucratic jurisdictions, and bureaucratic control, not the content of foreign policies as I was arguing in the
book. The focus would then shift from trying to explain why France relinquished autonomous monetary policy-making to whether its decision to do so paradoxically strengthened its state apparatus and its structural autonomy from the institutions of other states.

Now whether autonomy is an institution specific to the nation-state or a component of the ingroup/outgroup dichotomy, and hence generalizable across time and space, is a real dilemma for my argument. I ended up saying its part of the dichotomy and hence built into human relations over time, but Ted argues that its "a variable, not a constant." It is true that I have not made a sufficient case for it as an ahistorical constant. Rawi's point that my argument may fit Germany better than France raises an interesting possibility that it’s the powerful who are most obsessed with autonomy. If the pursuit of power is linked to the formation of groups, then autonomy may be more a function of relative power than group formation. That is, the more powerful may be inclined to create autonomous decision-making institutions, the less powerful less so (think of Belgium or Luxembourg desire for greater integration, for example), but the latter end up developing autonomous institutions too due to the pressures of anarchic imitation.

As an aside, Ted, the assistance Russia seeks from the EU in writing their domestic legislation can't just be a function of Russia's domestic identity discourse, because plenty of other states do this sort of thing too. Nation-states have always studied one another's internal structures and copied what they thought had assisted other's in achieving military strength and wealth. What identity determines is who they imitate, but it doesn't explain why there is a generalizable motivation to copy the institutions and practices of others. This also fits with your point, Rawi, that states want to maintain autonomy from specific states, which might actually induce them to give up autonomy to a lesser evil. They make such choices based on what is internal to them (both institutionally and socially), not what would be the most rational choice according to some objective standards of power or wealth preordained by anarchy. Hence the study of domestic identity can tell us why Russia would imitate Europeans and never the Americans, but it can't tell us why this is a behavioral pattern one finds throughout human history.

**Anarchy and Death**

Speaking of anarchy, Ted, this was clearly the main problem you saw in my argument as most of your questions and points are related to it. "Why is anarchy the only environmental constraint?" you ask, and "why aren't identity, institutions, and discourse of primary causal importance, rather than anarchy?" In reply I have to admit that I wasn't exactly being honest when I wrote the chapter. At the time I had already begun to think in evolutionary biological terms, but I wasn't ready to fully commit and lay my cards openly on the table just yet. There are obvious hints of this, particularly in the footnotes, but I tried to skirt it rather than deal with it head-on. The result is that the chapter may have generated more confusion than if I had just been direct about it from the start.

Let me be more explicit. If I were rewriting the chapter today, I would say that the concept of "anarchy" in realism is actually based on and a substitute for Darwin's natural selection process. That is, when realists talk about anarchy, what they really have in mind is something akin to environmental selection processes, though few of them would voice it in this way. So when I
refer to anarchy as an environment, I do quite literally mean environment. But I don't mean environment today or in any moment of human recorded history. What I'm talking about is human evolutionary history and the role that the natural environment played in species competition for resources, species survival, and species reproduction and which produced us as a social species. I used the term “anarchic environment” since anarchy is a term most IR readers recognize, but it would be more accurate to use the term "environmental natural selection" instead (note, however, that anarchy trips more easily off the tongue).

Now at one point, Ted, you critique what you claim is my argument regarding "fear of death" as a motivator for behavior. This surprised me since I tried to be relatively clear that I was not making such an argument. The only reason I discussed its role in prior realist theory was in order to reveal its flaws and dismiss it. I don't know if you were using the email attachment copy I sent you or a copy of my book, but I do this on pages 4-5 on the attachment and pages 71-73 in the book. Several constructivists had successfully convinced me that the "fear of death" argument was fundamentally flawed, and Dan Nexon pointed out to me that I actually didn't need "fear of death" to make my argument work. I just needed death as the selection mechanism for species evolution, whether the species be humans or chipmunks.

Death is suppose to effectively drops out of my argument thereafter, although Ted, you are correct that in discussing imitation I do argue that groups that fail to imitate can select themselves and their social practices out of the system overtime. I agree that this sounds like a "death of states" argument, although I was thinking of examples such as Dynastic China and the American Indians’ respective encounters with Europeans when I wrote it. These groups continued to exist in some fashion – there are still American Indian tribes today, there is still a China – so what did I mean by “selected out” if not death of groups? I think what I had in mind was related to the autonomy issue as discussed above: that these groups were being confronted by more powerful groups, and, due their inability (the Indians) or refusal (China) to adapt their social practices by imitating those of the more powerful, they unwillingly lost the ability to make autonomous choices for themselves. The Chinese case was particularly pertinent to Japanese leaders, who watched this unfolding and were quite vocal about their desire to proactively imitate European institutions and lifestyles in order to avoid the same fate.

Now its an interesting point that both of my examples are drawn from the Westphalian system and the obsession with autonomous decision-making institutions may be specific to that system (hence what I had in mind with regards to “selected out” may be specific to it and not historically generalizable). That is something I need to really think about. What is also interesting here is that both these groups later recouped some level of autonomous decision-making, yet only within the context of the Westphalian nation-state, ie: China eventually became one, American Indians were allowed some sovereignty within the borders and on terms specified by the American state. But ultimately, anarchy as environmental natural selection has nothing to do with why particular states or groups form or disappear or lose their decision-making autonomy, so I’m really not making a “death of states” argument here. Obviously this is something I need to either disconnect or make more clear in my subsequent work.

Actually I think Rawi better captured my intentions on this point, when he said that by the time we get to the discussion of imitation (and social practice or identity for that matter), "we are two
steps removed from death." Yes, that is correct. Natural selection operated on pre-humans through the mechanisms of resource availability, death, and reproduction. It produced a highly social species. One of the attributes of the species is the tendency to imitate one another's social practices. Hence death has no direct influence on whether groups imitate or fail to do so, because social context is essential to the choices groups make. Since analytically this indicates that groups choose their own paths, there is not much I could say with this theory about the death of states/groups as a general pattern. To understand it one needs history, context, and specificity. Death does, however, play a considerable role at that evolutionary moment when natural selection forces operated to produce a species capable of creating its own social realities and manipulating its natural environment.

When you argue, Ted, that at birth we rely on others, on caregivers, to survive, I was surprised that you were raising this as a counter-argument to my own, since I had argued exactly this same point on pages 74-77 in the book, pages 6-7 in the attachment. In other words, I never said infants have some sort of "fear of death." If you look more closely at my argument, I think you'll see that what I argued was that as a social species we need each other in order to survive infancy. Hence to be human is to be social and to develop social practices, as Onuf has argued (1989), and it is that which keeps us alive both as individuals and, more to the point, as a species. This is also the realist connection to constructivism in that human beings create their own social realities (constructivism), but why they are social in the first place is due to a natural selection process over which they had no control (realism). It was a misnomer to call this "self-help" without clarifying that self-help lies at the nexus between the two (between being and becoming) and is referring to the human production of social practices in general, not human evolution itself or the specific content of social practices.

Now about your concerns that I keep insisting that anarchy/natural selection is primarily causal, there is a difference between primary and direct. In bio-political lingo anarchy would be referred to as an "ultimate" cause in that natural selection is an ultimate cause for why we are social. But ultimate causes don’t tell one very much either in biology or in political science. What this particular ultimate cause tells us is that sociability is fundamental to the human species, and this means sociability is a broadly bounded pathway, bounded by our evolutionary past and the role that reproduction and death (not "fear of death") played in the natural selection process. Sociability carries with it general attributes that distinguish us from a species like turtles. Among social species, our capacity to communicate through language and construct our own social realities is unique, but our high degree of sociability is also why we are unfortunately very good at waging war.

Sociability, then, derives from the ultimate cause of anarchy/natural selection and produces certain behavioral tendencies that human creativity has a difficult (but by no means impossible) time overcoming. This is where in-group/out-group distinctions fit analytically, along with the tendency to trans-group imitation, a concern with relative power, and so on. These are broad brush strokes and I think one could easily challenge the linkages I make between these strokes, group formation, and social practices. But anarchy, as I define it, is quite clearly a primary or ultimate cause in the structure of my argument, it’s just not a direct cause in the present as you seem to believe I was arguing.
Alternatively, social practices and institutions are, in bio-political lingo, "proximate" causes. As noted above, ultimate causes don’t tell us very much about social reality or history. If you want to understand everything from the beginning of recorded human history to the present, you'll need to examine social practices and institutions in their historical contexts which are proximate, immediate, or direct causes. Hence you are right, Ted, when you say that my argument implies that to establish emulation, one must explore the "fit" between available practices and the institutional, "identity terrain in any given state." I’m not just implying it, that is my argument. But this does not contradict the point that anarchy/natural selection is the primary/ultimate cause for why groups tend to imitate one another's social practices. Nor does it contradict the point that all domestic social practices are derivable from anarchy, in the sense that ultimately we would not be social and have social practices had it not been for natural selection. This claim has nothing what-so-ever to do with the content of social practices.

Falsifiability

Ted, at one point you noted that "there is a falsifiability issue…lurking" in my theory regarding emulation and that "it is hard to know when the theory is working and when it is not."

Yep, in fact I think my own response comments thus far underscore this as well. But I have to admit to being relatively unfazed about this, because I am not overly wedded to the dominant Popperian/Behavioral discourse of the IR discipline or the clarion call for falsifiability (which, by the way, was the first thing Joe Grieco told me was wrong with my theory too). I share with James Richter the inability to entirely liberate myself from positivist constraints, and so I recognize that in side-stepping this as an issue it sounds like an excuse to commit bad theorizing or throw all standards of explanation to the wind. Since I start from a positivist base in the book, you are also right that I need to be held accountable for failing to live up to it (and I think you and Rawi have done a pretty good job skewering me in that regard).

But I have also been convinced by post-positivist critiques of the discipline and the power of discourse within it. Hence I have found it difficult, after reading authors such as Smith, et. al. (1996), Crawford and Jarvis (2001), Ricci (1984), Seidelman (1985), Lowi (1992), and Oren (2003), to return to the "Never-Never Land" of positivism to which so many of our IR and non-IR colleagues unthinkingly journey everyday in political science. I concur with post-positivists that there is no such thing as an objective fact (my own theory is more reflective of the social product I am than what is "really out there"), there is no actual "testing" conducted in the discipline, nor do we falsify theory according to the "scientific methods" handed down to us by our disciplinary founding fathers/mothers. There are all sorts of reasons why this is the case, as many of the authors I just listed can attest more fully, but the most seminal to IR, I believe, is that all scientific theories are based on prior ideological and philosophical commitments, and scientific methodology has no capacity to disabuse adherents of these commitments.

Take the realists and liberals, for example. They’ve been arguing for over 100 years about the same old stuff, and no matter how much they go after each other with scientific methodology to reveal the flaws of one another's scientific theories, they never manage to kill off one another’s ideas. Why not? Because at heart they are not scientific theories, they are philosophies

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masquerading as scientific theories and the commitment to them is closer to religious faith. Each new re-statement of realism or liberalism is simply a newer version of the same old thing, hoping to tell a good story that will resonate among the faithful and convert some agnostics. Morgenthau says its all about power-lust, Gilpin says its reputation, Waltz claims its structure, Mearsheimer argues its security-scarcity, Schweller says its power-lust again, and I claim its group formation. This is the same idea dressed up in new packages, and none of these packages make for particularly good science. But ideological faith is never abandoned on the basis of deductive logic, scientific methodology, and falsifiability alone. If it could be, we would have flushed both realism and liberalism down the toilet a long time ago.

Now Ted you are quite right that in many instances my explanations fail on the very positivist terms with which they start, and so I should be held accountable for claiming I can explain things I cannot. My realist-constructivist theory can be taken to task for being a scientifically unconvincing theory, because it’s illogical, under-specified, and unfalsifiable. Fair enough. But I have yet to encounter an ideological or philosophical commitment that can be scientifically falsified. And I've heard of no card-carrying neoliberal who has jumped ship on the basis of the sheer deductive force and evidence of my argument against NLI in the book. Nor would I have expected any, since NLI is just the latest version of an on-going and deeply embedded philosophical commitment to liberalism in some portions of the discipline.

Reification of Groups and More

This is my equivalent of Ted's "whew, that’s only the half of it; in fact there is more wrong than you intimated."

Did anybody notice how it was possible to substitute the word "nation-state" for "group" in some areas of my chapter? True, there are theoretical constructs that are specific to "groups" as in the in-group/out-group dichotomy that one does not find in theorizing about the nation-state. And I relied on those alternative theoretical constructs to develop my arguments. True also that I tried to dissect the American state to some degree and reveal the multiple institutional identities that comprise it. But as I was theorizing in the abstract, and in the realist sections of the chapter in particular, I found myself constantly thinking of examples drawn from the nation-state, and not thinking in terms of other types of groups either today or historically or all the nuances that my argument involved.

Ted, you touched on some of these problems when you noted that ingroup/outgroup needs to be "deeply contextualized" and also under your comments for Dorry, when you observed that I tend to speak only in terms of ingroup homogenization while ignoring ingroup heterogeneity. And Rawi, you too come close to revealing these problems in your last paragraph about the process of identification. But I don't think either of you realized just how bad my arguments got in this regard. In fact, in the realist sections "group" and "nation-state" are practically synonymous!

In other words, in mapping out realism's take on groups and identity, I didn't grapple with the presence of multiple, overlapping group identities that Ferguson and Mansbach have written about in their polities work (1996). I didn't grapple with how one defines or identifies the larger,
overarching group identities that exist today and in the past. I didn't grapple with how one establishes or recognizes the boundaries of large groups such as nation-states, and how overarching groups or polities such as the nation-state are held together or break apart. I didn't grapple with what the relationship is between various sub-group identities and the larger group identity or polity (with the exception of some discussion about bureaucrats). And I didn't grapple with what the relationship is between the overarching group and the multiple, overlapping group identities that are above and beyond the larger group.

I knew I wasn't confronting any of these tough questions because, as I was writing the chapter, I kept thinking about empires and how they were probably a complete foil to everything I was taking for granted about overarching group and subgroup identity. In particular, how did the identity of subgroups work within empires and in relation to the empire builders who contained or conquered them? How were these alternative identities incorporated both cognitively and institutionally and from both sides of the aisle? If anyone knows of some literature about empires and identity that would point me in the right direction I would really appreciate it. I would be particularly interested in what a comparison between the Roman and Chinese empires would reveal.

And, of course, I wasn't grappling with why my "larger" groups were automatically nation-states and not other types of polities and identities that exist transnationally or sub-nationally in the world today. The easy answer to this is because the nation-state retains an overarching grip on the cognitive imaginations of its participants (which it reinforces with material benefits and institutions), and hence it is still first among equals with regards to group identity. But I do is right again – that’s such a typical American statement! Obviously such an answer does not hold for many places in the world or for many subgroups within particular nation-states. We spent the latter half of the 20th century referring to a nation-state called "Yugoslavia," and we'd have no way of understanding why it no longer exits if we automatically assumed that the larger group, a.k.a. nation-state, is always central to contemporary global analysis. Yet too frequently in my theoretical arguments I found myself doing exactly that.

Bottom line: I let the "groups" concept do my heavy "innovative" lifting for me, but in many instances I was simply reifying the concept in much the same way that the nation-state is reified in the IR literature. That is, the nation-state is treated as a unitary actor whose boundaries from other nation-states and centrality to world politics is so obvious that its not worth discussing. At the end of the day it appears that in some cases all I did was change the language, ie: "lets not call them 'nation-states,' lets call them 'groups' instead but…hey wait, gee, wow, look at that, these groups behave just like nation-states!" UGH!

References


