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### A Comment on the Comments

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## A Comment on the Comments

MARC TRACHTENBERG

At the beginning of his famous 1994 paper on audience costs, James Fearon noted that the theory he was about to lay out was motivated by an “empirical claim, namely that crises are public events carried out in front of domestic political audiences and that this fact is crucial to understanding why they occur and how they unfold.”<sup>1</sup> It was perhaps natural, in a theoretical paper of this sort, that he did little to back up that empirical claim. One might have thought, however, that other scholars would have made a real effort to see whether his argument held up in the light of the evidence. But it was hard to examine some of the key claims associated with the theory using statistical methods—a point Erik Gartzke and Yonatan Lupu make in their contribution here—and until very recently there were no works that tried to see just how important the audience cost mechanism was by looking at historical cases. So for many years there were no satisfactory tests of Fearon’s key empirical claims, and this point applies in particular to one prominent part of his argument, his conjecture that democracies had an advantage in international bargaining because of what he supposed was their superior ability to generate audience costs. As Robert Jervis noted in 2004, many scholars treated that conjecture “as though it were a proven fact and built more elaborate arguments around it,” but Jervis himself was struck by the fact that “no evidence for the claim has been produced.”<sup>2</sup>

So my goal in doing the work that led to the paper we are discussing here was simple. I wanted to look at the historical evidence and see what kind of role the audience cost mechanism played, if any, in a whole series of major international crises. I identified these cases by using a set of criteria defined by what I saw as the central thrust of the Fearon theory, and in examining those historical episodes I was particularly interested in two questions which seemed to relate in a fundamental way to what Fearon was saying.

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<sup>1</sup>James Fearon, “Domestic Political Audiences and the Escalation of International Disputes,” *American Political Science Review* 88, no. 3 (September 1994): 577.

<sup>2</sup>Thierry Balzacq and Robert Jervis, “Logics of Mind and International System: A Journey with Robert Jervis,” *Review of International Studies* 30, no. 4 (October 2004): 573.

First, did democracies deliberately try to take advantage of the audience cost mechanism in order to get an edge in international bargaining? And, second, did the opposing powers understand that the democracy's threats "were credible because audience costs would be incurred if that power gave way in the dispute?" That latter question lay at the heart of the analysis in the article. The adversary, it seemed clear, had to be able to see *why* the democratic leaders' hands were tied, for otherwise it "would have no reason to conclude that they were not bluffing."<sup>3</sup>

I thought at the time I wrote the paper that Fearon's argument was fairly straightforward, but it later became clear to me that different people interpret it in different ways. Kenneth Schultz, for example, in his characterization of the theory in his comment here, emphasizes a point I did not talk about in my paper. This was the idea that "the costs conjectured to accompany public commitments were seen not simply as a way of increasing bargaining leverage in a crisis: they were also crucial to the decision whether or not to select into a crisis in the first place."<sup>4</sup> But I think that that idea about audience costs figuring into a state's decision to "select into" a crisis runs against the grain of the basic Fearon theory. For that theory, as Fearon laid it out, rested on the assumption that the ability of a state to generate audience costs had *not* already been factored into the policy decisions that had led up to the crisis, but rather came into play only after a crisis developed. Fearon said explicitly that "rational states will 'select themselves' into crises on the basis of observable measures of relative capabilities and interests and will do so in a way that neutralizes any subsequent impact of those measures."<sup>5</sup> In other words, capabilities and interests, to the extent they were observable in advance, should already have been taken into account and should therefore play no further role in determining how the crisis runs its course. That, in turn, could be affected only by *new* information generated during the crisis, and he clearly assumed that audience costs fell in that category.

Schultz himself certainly understood this point when he dealt with this issue in the section on the Fashoda crisis in his book *Democracy and Coercive Diplomacy*. Why was it, in his view, that theories emphasizing relative power could not explain why crises get resolved the way they do? The "core problem" with that sort of argument, he wrote, is that it "depends on factors about which decision makers had complete information at the outset of the crisis." To explain why the crisis ran its course the way it did, one had to point to some new information that was revealed in the course of the crisis that "could not have been foreseen" when the crisis began.<sup>6</sup> So if we say

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<sup>3</sup> Marc Trachtenberg, "Audience Costs: An Historical Analysis," *Security Studies* 21, no. 1 (January-March 2012): 7.

<sup>4</sup> Kenneth Schultz, "Why We Needed Audience Costs and What We Need Now," *Security Studies* (this issue).

<sup>5</sup> Fearon, "Domestic Political Audiences," 586.

<sup>6</sup> Kenneth Schultz, *Democracy and Coercive Diplomacy* (Cambridge: Cambridge University Press, 2001), 177-78.

a state's ability to generate audience costs was a key factor in determining whether that state and its rival would "select themselves into" a crisis, we are saying that that factor too should already have been taken into account before the crisis began. We would be putting it in the same category as relative power and indeed anything that could be observed in advance; but if audience costs are in that category, they too should play no further role in determining how the crisis runs its course.

Jack Levy makes this very point in his comment here. Audience costs, he notes, like the "balance of interests and forces," should already have been "priced into states' earlier decisions to initiate or respond to threats."<sup>7</sup> Indeed, if audience costs are as important as many theorists think, everyone should have been aware of that fact before the crisis developed. And if everyone already understood what the balance of power was, and what each country's interests were, then rational actors should have been able to predict whether, and to what extent, the audience cost mechanism would come into play.<sup>8</sup> It should therefore play no independent role in determining how the crisis runs its course. But that conclusion would run counter to Fearon's basic argument in his audience cost paper.

Schultz also points to what he sees as a second problem with my interpretation of the theory. According to Jack Snyder and Erica Borghard, he writes, the theory suggests that "audience costs provide a tempting way for democratic leaders to 'safely get their way in a crisis,' and he thinks I share this view. He quotes in this context a claim I made to the effect that since taking advantage of the audience cost mechanism can be risky, using that tactic is "not nearly as attractive to statesmen as audience costs theorists sometimes suggest." But this view, he says, "overlooks the point that audience costs could only have their hypothesized signaling effect by being *risky*, not by being safe."<sup>9</sup> And it is certainly true that theorists like Fearon often argued in that vein. But in practice they often made it seem that the audience cost mechanism provided leaders with something more than just a way to create risk. For if that was all the audience cost mechanism did, then it is hard to see how the leaders of democratic states would get any special bargaining advantage by being able to exploit it, since no one doubts that other states (and indeed democracies themselves) can generate risk in other ways. The theorists, however, clearly assumed this mechanism gave the regimes that were able to use it a special edge in international bargaining. They wrote as

<sup>7</sup> Jack Levy, "Coercive Threats, Audience Costs, and Case Studies," *Security Studies* (this issue).

<sup>8</sup> One thinks in this context of how Fearon, following John Harsanyi, defined rationality: "If two rational agents have the same information about an uncertain event, then they should have the same beliefs about its likely outcome. The claim is that given identical information, truly rational agents should reason to the same conclusions about the probability of one uncertain outcome or another." James Fearon, "Rationalist Explanations for War," *International Organization* 49, no. 3 (Summer 1995): 392.

<sup>9</sup> Schultz, "Why We Needed Audience Costs" (emphasis in original). For the original quotations, see Jack Snyder and Erica D. Borghard, "The Cost of Empty Threats: A Penny, Not a Pound," *American Political Science Review* 105, no. 3 (August 2011): 454.

though this bargaining advantage was a kind of bonus countries got simply by being democratic. The element of risk thus faded into the background; the theorists, it seems, sometimes lost sight of their own basic point that the effectiveness of the strategy is tightly bound up with the fact that it is risky.

These points, of course, have to do with how the theory should be assessed on its own terms—that is, with what we are to make of it in terms of its own internal logic. But in the paper I was not interested in looking at it from that point of view. I just wanted to see whether it stood up in the light of the historical evidence—whether the kind of mechanism it said was so important actually played a major role in real great power crises. Does that mean that the way I approached the problem was of limited value because it was essentially destructive and does not help us “move forward” in our thinking about those core issues? Schultz, in fact, seems a little frustrated with the sort of analysis I did in that paper. He does not say that my basic historical arguments were wrong, but he points out, quite correctly, that I offered “no consistent positive theory to explain the outcomes in [my] cases, instead relying on different arguments for each episode.” What alternative theory, he wonders, could have survived the sort of evidentiary standard I used to assess the audience cost argument? And if none of them could, what are we supposed to do? Become totally atheoretical?<sup>10</sup>

Schultz goes on to say that my explanations are not totally consistent with each other. Sometimes I argue that the audience cost effect is so small that leaders do not have to worry about it; at other times I suggest the effect is so large that leaders, reluctant to opt for a risky bridge-burning strategy, shy away from taking advantage of it. Schultz thinks my claim that governments can use certain strategies to avoid paying audience costs is not consistent with my claim that “these sorts of pressures could not be turned on and off like a faucet.” But there is no real contradiction here. My point was that governments can *often* avoid paying audience costs when they moderate their policies, but that *sometimes* those costs are not avoidable. I claimed that for a variety of reasons the audience cost effect is *as a rule* very small but that *on occasion* (as in the case of the Cuban missile crisis—on both sides, incidentally) a political leader clearly would pay a major price for backing down. One might challenge those claims on empirical grounds, but they are certainly perfectly consistent with each other.

Similarly, Branislav Slantchev thinks I contradict myself by arguing both that democratic leaders prefer not to tie their hands by generating audience costs and also that “even when these leaders do commit, their opponents ignore them.” If that first argument is correct, he writes, then it is hard to see how the second argument could hold—it is hard to see, that is, why the opponents “would fail to understand,” when “such a commitment does

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<sup>10</sup> Schultz, “Why We Needed Audience Costs.”

take place,” that the democratic leader’s hands were tied. But Slantchev’s criticism here should be directed at the actors in question and not at me. When I looked at the Cuban missile crisis and saw Khrushchev ignoring what his advisors were saying prior to the crisis about how strongly Americans felt about this issue, what was I supposed to do? I obviously had to point it out, but this does not mean I was contradicting myself. Slantchev finds it “troubling” that whereas I cite “as evidence Shuvalov’s apparent lack of concern about British audience costs” in 1877, I dismiss “Gromyko’s concern about American audience costs in 1962.” But I certainly did not suggest that what Gromyko was saying was wrong; it was not me, but rather Khrushchev, who “dismissed” the concern being raised. But even putting this sort of point aside, it is obvious that the relative importance of particular factors varies from case to case. Why should it be “troubling” for a scholar to recognize this?<sup>11</sup>

The simple answer to objections of this sort is that we have no choice but to accept reality as it is—if it is complex, we should not pretend otherwise. If different explanations have to be used for different cases, then so be it. And if this approach creates problems for the theorist, then it is the theorist’s job to sort them out. We historians should not be expected to do their job for them. We have a hard enough time just doing our own job.

But I would hate to just leave it at that. I think, in fact, that the kind of analysis I did in that paper was not purely destructive in nature, and that this sort of approach can be of enormous value to the theorist, if he or she reacts to it the right way. Schultz points out that a theory can help us identify puzzles, and I certainly agree. But one deepens one’s sense for what the puzzles are by examining the theory in the light of the empirical evidence. When the theory seems to fall short of accounting for what you actually see going on, you should not be irritated by that finding; this should instead be viewed as an opportunity. When you come across this sort of thing, it is like finding gold in your hands. An anomaly can serve as a kind of springboard—as a focus for further theoretical analysis and perhaps as a source of clues about why existing theories had gotten it wrong.

Schultz himself gives a nice example of this when he talks about the implications of the argument that states are more interested in retaining a degree of flexibility than “extant theories” would suggest. Maybe an alternative framework, he suggests, could be developed to explain why this is so. A second example has to do with what for Levy was one of the main points that emerged from the empirical analysis, the point about the “fitnessability” of audience costs, a point Jonathan Mercer also talks about. Levy suggests, and I agree, that this point could be developed by drawing on prospect theory, since the way an issue gets framed is so important in this context. But

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<sup>11</sup> Branislav Slantchev, “Audience Cost Theory and Its Audiences,” *Security Studies* (this issue).

perhaps the main example that comes to mind here has to do with the fundamental assumption that lies at the heart of the “costly signaling” approach to international conflict, the assumption that the “incentive to misrepresent” is of central importance in international political life. One of the main conclusions I came to when I was working on the audience cost paper is that the problem of deception is not nearly as important as Fearon and others have suggested; this means that the question of why honest communication might be easier than people think is worth exploring more deeply. Mercer’s point that “rational people do not think the way rational choice theorists think they should think” is quite important; and if Mercer is right about this—and I think he is—one of our goals should be to figure out why this is the case.<sup>12</sup> As it turns out, there is a good deal of work done in other fields—especially economics and evolutionary biology—that is of real value in this context.<sup>13</sup>

So when done correctly, historical analysis can, I think, be a major source of insight, even for the theorist. And I do not think it is as hard to do as Gartzke and Lupu think. Their view seems to be that we never really have enough evidence to know what was in leaders’ minds when they took tough stands in public. But the evidence is often substantial enough to allow us to draw certain conclusions. To be sure, inference in this area rests on a certain set of assumptions: that political leaders need to convince each other that the policy being chosen makes sense and that they feel there is value (in both intellectual and domestic political terms) in arguing these things out and, indeed, in putting things down on paper. The records of policy discussions, secret at the time but often available many years later, can therefore tell us a good deal about the real motivations behind a particular policy. It is quite true that certain types of motivations—an interest in accommodating certain interest groups, for example—are likely to be systematically under-represented in the written sources, but it is hard to see why we should expect that to be the case with audience costs. Bridge-burning arguments, in fact, are made from time to time, and one does not have the sense that people

<sup>12</sup> Jonathan Mercer, “Audience Costs Are Toys,” *Security Studies* (this issue).

<sup>13</sup> In evolutionary biology, the argument about the effectiveness of low-cost signaling in cooperative relationships (and one should remember that the common interest in avoiding war gives international political relations a certain cooperative dimension) was laid out in an important article by John Krebs and Richard Dawkins, “Animal Signals: Mind-Reading and Manipulation,” in *Behavioural Ecology: An Evolutionary Approach*, 2<sup>nd</sup>ed., ed. J.R. Krebs and N.B. Davies (Sunderland, MA: Sinauer Associates, 1984), esp. 391. In the economics literature, that basic argument is supported by some quite extraordinary experimental findings, to the effect that “face-to-face communication” produces “substantial increases in cooperation,” well beyond what standard rationalist theories might lead one to expect. See especially Elinor Ostrom, “Collective Action and the Evolution of Social Norms,” *Journal of Economic Perspectives* 14, no. 3 (Summer 2000): 140–41. In our own field, as Jervis pointed out many years ago, empirical work done in the 1970s in fact showed that statesmen are less interested in abandoning control—that they leaned “more toward caution and prudence”—than leading deterrence theorists seemed to think. See Robert Jervis, “Deterrence Theory Revisited,” *World Politics* 31, no. 2 (January 1979): 303. I discuss some of these things in the longer version of the audience cost paper posted online at [http://www.polisci.ucla.edu/faculty/trachtenberg/cv/audcosts\(long\).doc](http://www.polisci.ucla.edu/faculty/trachtenberg/cv/audcosts(long).doc), esp. notes 1, 203, 204.

are particularly embarrassed when they argue in those terms.<sup>14</sup> To support their claim that it was “unlikely a leader would clearly document his or her plans” to take advantage of the audience cost mechanism, Gartzke and Lupu say neither I nor Snyder and Borghard “examine cases where audience costs are discussed but rejected as an option.”<sup>15</sup> But in my article I had in fact showed that Disraeli had considered using this tactic before dropping it in 1877.<sup>16</sup> And one does not get the sense that it had been particularly difficult for him to put the idea down on paper.

How does all this relate to the fundamental questions we are concerned with here—questions about the value of theory in general, about the value of the audience cost theory in particular, and about the proper relationship between theory and evidence? Slantchev thinks that “the explicative and generative usefulness of Fearon’s original insight has been staggering, not simply in the number of references but in the development of new ideas.”<sup>17</sup> Schultz, for his part, thinks the audience cost theory accomplished two things. First, it helped us understand “what makes threats costly to issue.” And second, it helped us understand “why bargaining ends”—that is, why a crisis could develop into an armed conflict—it showed how leaders could “get locked into intransigent bargaining positions from which they cannot climb down.”<sup>18</sup>

My own views in this area are more mixed. I do not think it was ever particularly hard to answer the question of why threats might be “costly to issue.” The basic principle here is that the more deeply you get involved in something, the harder it is to pull back, and threat making might be an integral part—although by no means the only part—of that commitment process. It is certainly easy to understand how threat making can lead to a hardening of political positions. For one thing, it can help crystallize public thinking about the importance of the issue at hand and thus about the need to take a firm stand; political leaders, in such circumstances, would not want to pull back with their tails between their legs and not just because they would pay a price at the ballot box if they did so. They might also pay a price in terms of how their adversaries would view them, how third parties (including their allies) would view them, how posterity would view them, and indeed how they would feel about each other and how they would feel about themselves. Thus one might be limiting one’s freedom of action by making clear threats; one might in particular be sacrificing one’s ability

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<sup>14</sup> One important case has to do with America’s strategy during the Kennedy period for the defense of West Berlin. See Marc Trachtenberg, *A Constructed Peace: The Making of the European Settlement, 1945–1963* (Princeton, Nj: Princeton University Press, 1999), 286–95, esp. 291. Political leaders certainly feel quite comfortable explaining to their adversaries how their hands are tied.

<sup>15</sup> Erik Gartzke and Yonatan Lupu, “Still Looking for Audience Costs,” *Security Studies* (this issue).

<sup>16</sup> Trachtenberg, “Audience Costs,” 11.

<sup>17</sup> Slantchev, “Audience Cost Theory and Its Audiences,” *Security Studies* (this issue).

<sup>18</sup> Schultz, “Why We Needed Audience Costs.”



to keep the dispute from developing into an armed conflict. This sort of thing could certainly be viewed as “costly.” But my real point here is that all this is fairly obvious; one did not need the audience cost theory to see why threats could have this kind of effect. And I feel the same way about Schultz’s second point about how the audience cost theory helped us understand how countries could get locked into positions in the course of a crisis and about how this can bring on a war. This point, as Fearon himself noted, was quite familiar well before he wrote his audience cost article; I think especially in this context of the Sontag article I cited in the paper.<sup>19</sup>

On the other hand, as I said in that paper, I think Fearon’s contributions to the field have been quite extraordinary. But this is not because of the specific arguments he made. It has to do instead with the way he got us to rethink our whole approach to the problem of war and peace and especially with his basic point that what passes for theory cannot be just a long laundry list of “arguments and conjectures” about international politics—that those “diverse arguments” have to be developed into a “coherent theory fit for guiding empirical research.”<sup>20</sup> And this was exactly what he did in his famous “Rationalist Explanations for War” article. The theory was powerful because instead of simply presenting a hodgepodge of factors, it developed an argument about what the causes of war *had to be*. It was of value because it provided us with a framework for thinking about the fundamental issue of what makes for war that had not existed before. And it served that purpose whether one accepted the specific arguments Fearon made or not. In fact, it could be of particular value if it turned out that some of those specific arguments were wrong (as I think was the case with the audience cost theory). “Truth,” as Francis Bacon famously noted, “emerges more readily from error than from confusion.”<sup>21</sup>

Let me try to explain what I mean here by talking a bit about the history of the costly signaling idea. It was Robert Jervis, I think, who introduced that idea into the international relations field in 1970 in his book *The Logic of Images in International Politics*. The idea made its first appearance there in a footnote and was presented almost as an afterthought: “As will be discussed later, high costs are often involved if it is later discovered that the actor’s signals were designed to be misleading. Indeed, if there were no such

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<sup>19</sup> Raymond Sontag, “The Last Months of Peace, 1939,” *Foreign Affairs* 35, no. 3 (April 1957): esp. 507, 524; Fearon, “Domestic Political Audiences,” 577. His model, Fearon wrote in that passage, “captures a common informal story about international crises—that their danger and tension arise from the risk of positions hardening to the point that both sides prefer a fight to any negotiated settlement.”

<sup>20</sup> Fearon, “Rationalist Explanations for War,” 382. Note especially what he says here about “guiding empirical research.” Fearon, in fact, dealt very intelligently in his dissertation with the historical evidence, especially the evidence about the coming of the First World War in 1914. James Fearon, “Threats to Use Force: Costly Signals and Bargaining in International Crises” (PhD diss., University of California, Berkeley, 1992), available online through subscribing libraries from ProQuest.

<sup>21</sup> Francis Bacon, *The New Organon*, ed. Lisa Jardine and Michael Silverthorne (Cambridge: Cambridge University Press, 2000), 173.

costs associated with issuing misleading signals, there would be no reason for receivers to place any faith in them.”<sup>22</sup> Previous writers, even Thomas Schelling, had not quite made that point.<sup>23</sup> But Jervis took care not to push the costly signaling argument too far; and one of the things that kept him from claiming too much for the idea was his sense that theoretical analysis had to be firmly grounded in empirical reality. Indeed, the basic distinction he made in that book between signals and indices suggested there was a good deal more to learning in a crisis than just costly signaling: an index could be anything thought to be beyond the ability of the sender to control (like intelligence information gathered in the course of a crisis) and was thus inherently believable. It followed that non-cost-incurring indices could also provide new, important, and credible information in a crisis.

But other scholars, when they took up those ideas, pushed the costly signaling argument much harder. The economist Michael Spence is the main case in point. He borrowed the term “index” from Jervis but used it in a rather different way. For him, indices were “observable, unalterable” attributes, like the sex of a job applicant, as opposed to signals, which were manipulable. It was thus natural for people like Fearon who adopted Spence’s basic approach not to view indices as playing a role in crisis situations; if they were fixed and unchanging, they should already have been taken into account before the crisis started and could not provide new information. And given this basic approach, it was easy to lose sight of the fact that non-cost-incurring indices could be generated in the course of a crisis and thus provide new information that could play a key role in shaping outcomes.<sup>24</sup>

Now, one of the main points in my paper was that in a crisis situation new information is generated in all sorts of ways and not just through costly signaling, let alone essentially through the one type of costly signaling highlighted in the audience cost theory. Jervis, in other words, had gotten it right in 1970: the picture of international politics that emerged from his work was much closer to reality than the picture Fearon’s audience cost paper gave.

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<sup>22</sup> Robert Jervis, *The Logic of Images in International Relations* (Princeton, Nj: Princeton University Press, 1970), 18, n. 2. Jervis went on to develop it in the text. See especially *ibid.*, 19–20.

<sup>23</sup> Schelling had suggested in passing that the fact that an action was costly might make it more credible, and some of his arguments even had a certain audience cost flavor. But by later standards his claims were fairly mild—he did not argue that a threat *bad* to be costly to be credible—and sometimes the way he treated this issue seemed to point in the opposite direction. The credibility of a threat, he noted in one essay, might “depend on the costs and risks associated with fulfillment for the party making the threat,” implying that for threats to be credible, they could not be too costly to fulfill. See Thomas Schelling, *Arms and Influence* (New Haven, CT: Yale University Press, 1966), 150, quoted in Jervis, *Logic of Images*, 19. Note also Thomas Schelling, *The Strategy of Conflict* (Cambridge, MA: Harvard University Press, 1960), 6 (for the quotation), 27–30, 102.

<sup>24</sup> See especially Spence’s seminal article, “Job Market Signaling,” *Quarterly Journal of Economics* 87, no. 3 (August 1973): 357, 369, 374. Note also Spence’s Nobel Prize lecture, “Signaling in Retrospect and the Informational Structure of Markets,” *American Economic Review* 92, no. 3 (June 2002): 434, n.1.

But does this mean that the Fearon approach was devoid of value and that we could have saved ourselves a lot of trouble by just accepting the picture Jervis had given us in 1970? The answer, to my mind at least, is clearly no. We learned a lot by trying to come to terms with Fearon's more extreme claims. He made a strong argument about the central importance of the "incentive to misrepresent" in international political life and thus about the crucial role costly signaling plays, especially in crisis situations; and he tried to flesh out his argument here by highlighting the role played by one form of costly signaling, the sort that relies on the audience cost mechanism. If it turns out, as I think it does, that that mechanism does not count for much in the real world, then this forces us to rethink the whole costly signaling argument and to try to figure out why, if at all, it falls short. We would have to think about how important the problem of deception really is in international politics. We might reach certain conclusions, indeed conclusions that might be quite similar to what our thinking was before Fearon published his article, but now, because we see those conclusions in this particular context, we are better able to understand their importance. You cannot, in other words, understand why the arguments that bring out the limitations of the costly signaling approach are so important without first having grasped the power of the basic costly signaling argument. And I think this point applies in particular to an article Jervis himself published a few years ago on "Signaling and Perception"—an article, which, to my mind at least, is one of a handful of works anyone interested in these issues should make a point of reading.<sup>25</sup>

The French have an expression about someone having "*les défauts de ses qualités et les qualités de ses défauts*." The basic idea is that someone's strengths and weaknesses might be two sides to a coin; what seems to be a weakness in one context is actually a strength in another, and vice versa. This was certainly the case with Fearon's audience cost argument. That argument was powerful precisely because it was extreme, and this is perfectly normal in science. We expect a powerful theory to provide only a stylized picture of reality. "The more like a reflection a map becomes," the philosopher of science N.R. Hanson once pointed out, "the less useful it is as a map."<sup>26</sup> Or as Kenneth Waltz once pointed out, "To say that 'a theory should be just as complicated as all our evidence suggests' amounts to a renunciation of science for Galileo onwards."<sup>27</sup>

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<sup>25</sup> Robert Jervis, "Signaling and Perception: Drawing Inferences and Projecting Images," in *Political Psychology*, ed. Kristen Monroe (Mahwah, NJ: Lawrence Erlbaum, 2002).

<sup>26</sup> N.R. Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge: Cambridge University Press, 1958), 28.

<sup>27</sup> Kenneth Waltz, "Evaluating Theories," *American Political Science Review* 91, no. 4 (December 1997): 914. The internal quotation is from the well-known guide to method by King, Keohane, and Verba.

So the problem here was not that Fearon presented a theory which, as it turned out, does not stand up in the light of the evidence. The real problem has to do with what happened after he published his audience cost paper. As I said in my article, it is simply a question of balance: an interest in theory needs to be balanced by an interest in seeing how much of a role the mechanism one is theorizing about actually plays in the real world. And I find it hard to understand, given what was shown in the article, why anyone would want to dispute that conclusion.