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*Identity, Emotions, and Nuclear Proliferation: My Dissertation Story*¹

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This is the story of the winding path from my arrival at grad school to my dissertation and first book, *The Psychology of Nuclear Proliferation: Identity, Emotions, and Foreign Policy*.² My hope is that a step-by-step account of my journey will serve as useful comparative data for young scholars embarking on their own paths.

Part I: Finding a Topic

When I entered the doctoral program of the Harvard Government department in the fall of 1994, I did not intend to focus on nuclear weapons, or even international security. My first idea was to study the politics of European integration. But I soon became bored with the literature's squabbles over the relative institutional power of the European Commission versus the Council of Ministers. Meanwhile, I was enraptured by Donald Horowitz's *Ethnic Groups in Conflict*, an assigned reading in the Comparative Politics field seminar.³ Reading Horowitz's depiction of ethnic relations as a contest of status and identity felt like a liberation from the suffocating rationalism that was then dominating the IR field.

IR in those days heavily prioritized theory, chiefly rational choice theory. I liked thinking theoretically, but I could not stomach rational choice's fundamental assumption that people systematically act based on coherent logic and careful calculation. Therefore, I was especially intrigued by Horowitz's references to Social Identity Theory (SIT), a mainly European-based line of social psychology research that had not appeared in my other coursework.⁴ SIT showed how people's actions can stem from even the shallowest of group identifications. Moreover, Horowitz himself remarked that ethnic relations and international relations bear a strong resemblance.⁵ Inspired, I started spending entire days in the basement of

¹ Thanks to Diane Labrosse, Ronan Fu, Myrna Hymans, and Rieko Kage for their valuable comments on earlier drafts of this text.

² Jacques E. C. Hymans, *The Psychology of Nuclear Proliferation: Identity, Emotions, and Foreign Policy* (Cambridge: Cambridge University Press, 2006).

³ Donald L. Horowitz, *Ethnic Groups in Conflict* (Berkeley: University of California Press, 1985).

⁴ Horowitz, *Ethnic Groups in Conflict*, 144-147.

⁵ Horowitz, *Ethnic Groups in Conflict*, 31, but cf. 95.

Pusey Library, reading all the SIT research that I could find. I felt sure that I could use SIT to challenge the hegemony of rationalist IR.

Another grad student in my cohort, Ted Brader, also had an active interest in the findings of experimental psychology. We started meeting together to discuss our respective independent readings. He was planning to do experiments to demonstrate how emotional cues can affect Americans' voting preferences. I was glad to discover the literature on the psychology of emotions, but skeptical about political scientists donning white lab coats. I thought it more appropriate to remain as consumers of the psychologists' insights, blending them with other theoretical ideas to unpack the complexity of the real world. Brader's experiments ended up producing a fabulous dissertation, and soon thereafter political scientists started streaming into the lab.⁶ But the choice I made felt right to me.

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After general exams, in the summer of 1996 I wrote my first dissertation prospectus. It proposed a Waltzian *Theory of International Politics*-style IR theoretical revolution based on the findings of SIT.⁷ I proudly showed it to several professors. They all summarily rejected it. They were right. A grad student with no empirical research experience had no business writing a theory of everything.

I had earlier been offered a fellowship award to spend a year studying at the *École Normale Supérieure* (ENS) in Paris, and now with generals in the rear-view mirror, I intended to take it. Harvard professors counseled me not to leave Cambridge before I had a clear research plan. But I believed that thinking was also possible outside the 02138 Zip code.

When I arrived in Paris in the fall of 1996, the ENS assigned me to a historian of contemporary Europe named Gilles Pécout. (In the French academic system, history is rightly classified as part of the social sciences.) Upon hearing of my interest in national identity and foreign policy, he suggested that I study French school history textbooks' narratives of France's foreign relations. There had been very intense debates over how to present the history of Franco-German relations ever since the 1920s. Pécout added that the *Institut national de recherche pédagogique*, with its rich library of textbooks, was located literally a block away from the ENS on the rue d'Ulm. Soon I was wading deeply into the history of French history education.

One evening at dinner in the ENS cafeteria, I mentioned to a French student friend that I sensed a strong anti-American bias in the history textbooks I was reading. He retorted that in his experience, French history education was actually pretty soft on America. Well of course you would think so, I replied, because your education programmed you to think negatively about the United States. Indeed, I thought that exposing that kind of nationalist brainwashing was going to be a major contribution of my dissertation research. But he still resisted, so I decided to convince him by tallying up the book's negative and positive statements about the United States. The next day, after a few hours of furious counting, I had a clear finding: he was right. From then on, I have highly valued quantitative content analysis as a check against motivated reasoning.

By the end of the fall semester, I had an entirely new prospectus to show the professors back in Cambridge. My basic idea was to use SIT to predict how French elites' primary and secondary schooling in international history could affect the diplomatic policies they favored in their adulthood. Having thus married the theory to an empirical research agenda, I thought I was home free. But my main Harvard faculty mentors at that time, Jorge Domínguez and Stanley Hoffmann, criticized the imbalance between my detailed plans for quantitative content analysis of school textbooks and my vague ideas about how French history education could have shaped French foreign policy. Domínguez frankly told me, "This

⁶ Ted Brader, *Campaigning for Hearts and Minds: How Emotional Appeals in Political Ads Work* (Chicago: University of Chicago Press, 2006).

⁷ Kenneth N. Waltz, *Theory of International Politics* (Reading: Addison-Wesley, 1979).

prospectus is fine if you want to become a professor of education. But I thought you wanted to become a professor of political science.”⁸

I should clarify that Domínguez and Hoffmann generally supported my idea of bringing together the literatures on national identity and foreign policy. After all, some of their own work also focused on that intersection.⁹ In retrospect, I’m glad they pushed me to show that national identity matters for IR outcomes instead of simply assuming that it does. But their judgment was very hard for me to accept at the time.

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In Paris, besides the ENS I was also attending Bertrand Badie’s master’s seminar on “Sovereignty and Responsibility in International Relations” at the *Institut d’Études Politiques* (“Sciences Po”). Badie slated me and another student to give a class presentation on the recent 1995 unlimited extension of the Treaty on the Non-Proliferation of Nuclear Weapons (NPT). I was not especially interested in the topic and had no relevant background. It was simply my turn to present. But, having just been commanded by my Harvard advisors to be more “IR,” the two days I spent frantically cramming on nuclear weapons issues for the Badie seminar caused an epiphany. I fantasized about confronting the professors back in Cambridge: “National identity isn’t ‘IR’? Well, what if national identity determines if a country gets nuclear weapons? Is that ‘IR’ enough for you?”¹⁰

That brainstorm led to prospectus number three. Its basic hypothesis was that countries are only likely to build nuclear weapons if they are “oppositional nationalist,” meaning that their nationalism is based on enmity toward a foreign Other. My research plan was to use quantitative content analyses of history schoolbooks to measure a country’s level of oppositional nationalism, and then to conduct historical archival research to connect the dots from national identity to nuclear policy. At long last, my advisors indicated that I was on the right track. The full dissertation committee—now fortified by the addition of Alastair Iain Johnston and Stephen P. Rosen—formally approved the prospectus in December 1997, while also making clear that the research plan still needed fine-tuning.

Which country cases I should choose was a bone of contention at the prospectus defense, and the question remained unsettled far into the following spring. In the end, I picked France, India, Argentina, and Australia: two countries that had built the bomb, and two that hadn’t. The fact that I had the necessary language competencies to do primary research in those places was a major consideration. India was a last-minute addition that Rosen urged on me after its nuclear tests of May 1998. I was nervous about taking on such a complex country that I had never even been to, but he convinced me that political scientists should try to address burning contemporary issues. That summer Kanchan Chandra, a brilliant India specialist one year ahead of me in the doctoral program, patiently endured my flights of fancy about Indian history and politics and diplomatically offered tips on how to avoid sounding completely ignorant.

Part II: Producing the Dissertation

⁸ In fact, “history issues” were just about to blossom as a major focus of IR research, with MIT students such as Yinan He, Jennifer Lind, and David Mendeloff leading the way. But I had not yet met them then. The kindred souls I found on this topic at Harvard were mainly historians and sociologists, and notably Yasemin Soysal.

⁹ Notably in Stanley Hoffmann, Charles P. Kindleberger, Laurence Wylie, Jesse R. Pitts, Jean-Baptiste Duroselle, and François Goguel, *In Search of France* (Cambridge: Harvard University Press, 1963), and Jorge I. Domínguez, *Insurrection or Loyalty: The Breakdown of the Spanish American Empire* (Cambridge: Harvard University Press, 1980).

¹⁰ In fact, my imagined zinger was both unfair to my advisors and reflected my ignorance of the state of the field. In those early post-Cold War years, the nuclear age was widely thought to be over, and many recently-minted Ph.D.’s in arms control were finding that almost nobody was interested in what they had to say. It was only later that the topic of nuclear proliferation truly got hot—irrationally so—because of 9/11.

Harvard is a rich university. Therefore, it was not difficult for me to tap enough dissertation research funding to support an around-the-world tour from the fall of 1998 through the summer of 1999.¹¹ I traveled to France and the UK in September, then to Australia in October, over to India in November and December, back to France from January to May, and finally down to Argentina in July and August. This was an absolute thrill ride from start to finish. It was also a time of intense work and personal growth.

Along the way, I met legions of librarians, archivists, scholars, and policymakers who took my project seriously, patiently responded to my endless questions, and in some cases even found me a spare bedroom. It was really only during the field research that nuclear proliferation began to fascinate me, because the people I met were fascinating. There was the French politician who gleefully recounted how he had sneaked funding for the secret nuclear weapons program past Parliament in early 1955 by stuffing it into the Navy budget. The Australian scientists who bitterly resented the government's decision to stop funding their successful uranium enrichment research. The former chairman of the Indian Atomic Energy Commission who practically shouted at me that just because most Indians were poor was no reason to deny India the right to excel in nuclear science. In those moments, I felt nuclear policy come alive.

Exciting though the field research was, it also exposed big problems for my original theoretical framework. In the prospectus, I had assumed that there is an identifiable national identity that a country's top policymakers broadly share. But the historical record forced me to accept that different individual leaders can hold very different perceptions of their nation and its natural position vis-à-vis its external Others. This stumped me for a while, but I gradually realized that the problem was a blessing in disguise, as it brought me back closer to the project's original theoretical motivation. Most of psychology—including SIT—takes the individual as its level of analysis. Why, then, was I pitching my theory at the level of the national group? A much more defensible application of psychological theory was to assess different politicians' national identity *conceptions*, and to show how those different conceptions affected their nuclear policy preferences. In the long run, this turn to individual leaders would be what the proliferation field found most notable about my work.¹² But regrettably, the pivot to the individual level also meant that I could no longer justify the original plan to make a detailed analysis of school history textbooks. My dissertation would end up relying instead on the more conventional data source of political speeches.

A potentially even more serious problem for the theory appeared during the field research in Australia. As mentioned above, the prospectus' main hypothesis was that oppositional nationalists will seek the bomb, with the term 'oppositional nationalism' somewhat loosely defined as fear and loathing of an enemy Other. The evidence, however, showed that although all of Australia's postwar prime ministers from Robert Menzies to William McMahon clearly did harbor a strong fear and loathing of the so-called red-yellow peril of Asian Communism, only one of them, John Gorton, strongly pushed for an Australian bomb. This was a real crisis: was my main hypothesis simply wrong?

The answer dawned on me during the flight from Sydney to New Delhi. Once again, the prospectus had not been true to the original theoretical motivation. One of Horowitz's central points in *Ethnic Groups in Conflict* is that there tends to be little conflict between "ranked" groups in a clearly institutionalized status hierarchy, while there is much conflict between "unranked" groups whose relative status is unclear. Thinking about Horowitz's insight, I realized that I should be measuring national identity conceptions along not one, but two key dimensions of comparison between Self and Other: (1) the leader's level of oppositional identity—i.e., the fear of the Other—and (2) the leader's level of nationalist identity—i.e., the pride to stand up to the Other on your own. Not fear alone, but fear *plus* pride is the explosive psychological cocktail that pushes state leaders to decide for the bomb.

¹¹ I also received about \$5,000 from the Washington, D.C.-based Institute for the Study of World Politics, a once-major source of IR dissertation research funding that unfortunately no longer exists.

¹² See Jonas Schneider, "The Study of Leaders in Nuclear Proliferation and How to Reinvigorate It," *International Studies Review* 22:1 (2020): 1-25.

Scrawling on an airplane napkin, I found it easy to sort the Australian prime ministers according to the new two-by-two table of oppositional fear on one axis and nationalist pride on the other. In this table, Gorton stood out as the lone oppositional nationalist. The revised concept also helped me recognize key points about France that I had previously misunderstood, and notably that what differentiated the pro-bomb French nationalists from the anti-bomb French Europeans up to the mid-1950s was not different levels of fear of Germany, but different levels of confidence in France's ability to stand up to Germany alone. The same typology of national identity conceptions would later serve me well when it came to explaining the nuclear choices of Argentina and India, too.

When I arrived in Argentina, the last country on my world tour, I thought I understood the broad outlines of its nuclear history. The literature generally assumed that the military dictatorship that ruled Argentina from 1976 to 1983 had pursued nuclear weapons, and I was expecting to confirm it. To gather the necessary information, I did two to three interviews a day with Argentine policymakers past and present. At the end of one interview with a senior nuclear bureaucrat, he invited me for lunch at his home in a leafy Buenos Aires residential neighborhood. When we finished eating, my host said he had something I might want to have a look at. He went down to the basement and hauled up a dusty old cardboard box of secret documents from the military period. Those documents—including the minutes of high-level policy meetings, detailed budget plans, and even nuclear plant blueprints—led me to conclude that Argentina had never pursued the bomb.¹³ The discovery also reinforced my already strong tendency to doubt the standard American assumption that if a state is seeking advanced nuclear capabilities, ipso facto it is seeking the bomb.¹⁴

Part III: Writing the Book

I started hunting for a good, tenure-track job while writing up the dissertation in the fall of 2000. I remained on the hunt until 2003. Being rejected over and over again by dozens of departments all over the country—and outside it, too—was painful and exhausting.¹⁵ But the many practice job talks, and the few real ones, that I diligently prepared during those years turned out to be crucial for the project's further development. Repeatedly hearing the same questions and witnessing the same quizzical looks on many different faces helped me immensely to turn an unwieldy dissertation into a sleek 45-minute lecture, which later served as the outline for revising the dissertation into a book.

The changes were not merely cosmetic. I even changed the dissertation's fundamental research question. I had been framing the research as a *theoretical investigation*: "Why do states build nuclear weapons?"¹⁶ But the job talk workshopping showed that this theoretical framing narrowed the project's appeal to those few people who were already interested in nuclear issues. So I started posing the question instead as an *empirical puzzle*: "What explains why only some states have built nuclear weapons, although many more states would be capable of doing so?" To hammer the point home, I created a graph showing the gap over time between the fast-growing number of states with the technical wherewithal to build the bomb and the slow-growing number of states that had really made one. Using this puzzle framing at the start of my talks grabbed the audience's

¹³ My conclusion has been confirmed in subsequent research, esp. Diego Hurtado, *El sueño de la Argentina atómica: Política, tecnología nuclear y desarrollo nacional (1945-2006)* (Buenos Aires: Edhasa, 2014).

¹⁴ I used the Argentina case to point to broader shortcomings of U.S. proliferation intelligence and policy in Jacques E. C. Hymans, "Of Gauchos and Gringos: Why Argentina Never Wanted the Bomb, and Why the United States Thought It Did," *Security Studies* 10:3 (March 2001): 153-185.

¹⁵ I did receive one job offer during my second year on the job market, but from a department whose atmosphere was so lugubrious that even during the interview I felt suffocated and just wanted to go home. After I got the call, a friend who had also been interviewed for the same position expressed relief that they hadn't chosen him! I swiftly turned it down, to the consternation of my advisors.

¹⁶ I borrowed this question from Scott D. Sagan, "Why Do States Build Nuclear Weapons? Three Models in Search of a Bomb," *International Security* 21:3 (1997): 54-86.

attention and helped them see why my research was necessary. Indeed, I came to realize that this was at least half the battle. If people also found my proposed answers persuasive, so much the better, but the puzzle alone was sufficient to stimulate a productive conversation. I have strongly advocated for puzzle-driven social science research ever since.

Such improvements were only possible thanks to back-to-back-to-back fellowship years from 2000 to 2003 at Stanford's Center for International Security and Cooperation, Harvard's Olin Institute, and Ohio State's Mershon Center. These wonderful experiences gave me the necessary time, resources, and intellectual stimulation to keep the research going forward even as I fretted about my professional future. The year at Mershon from 2002-3 was especially fulfilling. There I found a big and interdisciplinary community of scholars of identity, psychology, and foreign policy—people like me. I felt like Frodo returning to the Shire. But Mershon was surely the last cushy post-doc that I could hope to receive, and I decided that if no tenure-track job came through this time, I would leave the profession before my 2001 Ph.D. degree became a liability instead of an asset. Luckily, it did not come to that. In early 2003, Smith College in Northampton, Massachusetts offered me a tenure-track position, which I eagerly accepted. I started teaching there that fall.

The time had come to turn the dissertation into a book. I couldn't simply send it off to publishers as is. It was extremely long—574 pages—including four painful-to-read chapters measuring various dimensions of top leaders' national identity conceptions. The content analysis rules were baroque elaborate, and the data they produced could be interpreted in different ways. Yet the measurement of national identity conceptions was the essential foundation of my social-scientific approach to the study of nuclear proliferation. I couldn't just strip out those chapters, baldly assert that leader X was or wasn't an oppositional nationalist, and expect people to believe me.

I got great help at this stage from Yoshiko Herrera, a Harvard assistant professor who had used quantitative content analysis to measure identity in her own recent dissertation on the economic policies of the Russian regions.¹⁷ Thanks to her, I saw that the basic problem was—once again—that my work had deviated from the original theoretical inspiration. The big attraction of SIT was its elegant simplicity. My theoretical framework was similarly streamlined. Yet when it came to measurement, I was trying to capture every last wrinkle in the texts. Worse still, I was trying to do that with quantitative content analysis, which is better for capturing broad patterns than delicate nuances.

A new and greatly simplified coding scheme came into my head in the darkness of a long-distance night bus traveling back to Northampton from Cambridge, where I had met with Herrera earlier that day. I did not relish remeasuring the main independent variable almost from scratch, but luckily, the discrepancies with the prior findings turned out to be mostly minor. With the new data, I could also now make a single graph of different leaders' national identity conceptions that mirrored my theorized two-dimensional space of levels of 'opposition' and 'nationalism.' The oppositional nationalists clustered in one quadrant, while the others were scattered elsewhere. A picture is worth a thousand words.

The road to book publication remained rocky to the end. Princeton University Press rejected my manuscript due to negative peer reviews. Then Cambridge University Press took more than a year before coming back with good news. *The Psychology of Nuclear Proliferation* finally appeared in print in 2006, twelve years after I began my graduate studies. I was gratified by its reception in the policy world as well as academic IR. Being invited to Paris to explain my findings to the French Ministry of Defense felt especially meaningful, like coming full circle. But I was more interested in ideas than in sales, so not long after the book came out, I started asking new research questions that demanded different answers.

Many faculty put strong explicit and implicit pressures on graduate students to strategically configure their research agendas for success on the academic job market. There's nothing sinful about making concessions to practicality. But psychologists have found that in the long run, work performance is much less responsive to the "extrinsic" motivations of high pay or a fancy title than the "intrinsic" motivation of believing in what you do. Furthermore, the more that workers are induced to

¹⁷ Yoshiko M. Herrera, *Imagined Economies: The Sources of Russian Regionalism* (Cambridge: Cambridge University Press, 2005).

seek extrinsic rewards, the weaker their intrinsic motivation becomes. These patterns are most pronounced among knowledge workers.¹⁸

The literature on intrinsic and extrinsic motivation reinforces my introspective perception that it would have been a grave error to allow my intellectual development to become hostage to my future career ambitions. If I had followed a more “rational” career strategy, I would quickly have become alienated from my work and cynical about the world. Instead, I viewed my dissertation research as a chance to nurture ideas that resonated deep inside me as truth. Even as I avidly sought feedback and advice from professors and fellow students, the goal was always to develop my own point of view. Treating the project as a journey of self-discovery may have slowed my progress toward completing the Ph.D. and landing a job, but—much more importantly—it fueled me to keep going. And the romance of ideas that inspired me then, still inspires me now.

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¹⁸ See Beth Hennessey, Seana Moran, Beth Altringer, and Teresa M. Amabile, “Extrinsic and Intrinsic Motivation,” in Sir Cary L. Cooper, ed., *Wiley Encyclopedia of Management*, v. 11: *Organizational Behavior*, 3rd ed. (Hoboken: John Wiley & Sons, 2014), online. I also used the concept as a key part of the theoretical argument in Jacques E. C. Hymans, *Achieving Nuclear Ambitions: Scientists, Politicians, and Proliferation* (Cambridge: Cambridge University Press, 2012), esp. 46-48.