

## **“Room to Move: International Financial Markets and National Welfare States”**

*International Organization*, Fall 2000

Layna Mosley

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This article, which was my first peer-reviewed publication, came directly from my dissertation research. The idea for my dissertation (co-chaired by Bob Keohane and Peter Lange, with Beth Simmons also on my committee) came from a graduate seminar I took during my second year of graduate school. That seminar was on Globalization and Domestic Politics, or something like that, and it was co-taught by Peter Lange and Beth Simmons.

We read a couple pieces which suggested that financial markets disliked left leaning governments (Beth Simmons’ *Who Adjusts*, which includes a discussion of market reactions to left-leaning governments during the interwar years; and Paulette Kurzer’s *Business and Banking*), and that this pressure from financial markets made it very difficult, given the financial openness of the 1990s, for social democratic governments to enact their preferred policies. A discussion in seminar led to me to ask whether we had evidence that investors did, in fact, hold these views of left leaning governments. I wrote an incredibly rotten seminar paper on this topic (attempting to assess the statistical relationship between government partisanship and short-term capital flows), but my advisers were very forgiving, and the topic became the core of my dissertation idea.

I was awarded funding from the SSRC to conduct research on this topic during my 4<sup>th</sup> year of graduate school. My plan was to interview professional investors to get a sense of how they responded to government policies and partisanship – to figure out some of the causal mechanisms that affected asset allocation and the pricing of risk. I also hoped to speak with some government officials (in central banks and finance ministries) to discuss their perceptions of financial market pressures. At the time (no longer true), SSRC awards were regionally focused (mine was for western Europe), and SSRC funding awards did not offer the option for (comparative) fieldwork in the US. I divided my six months of fieldwork between London and Frankfurt, with short trips to Basel, Brussels, and Paris.

It’s fair to point out that I didn’t really have much idea what I was doing with respect to interview-based research. My initial plan had been to contrast the way in which short term portfolio market (equity and bond) investors evaluate governments with the way in which longer term (foreign direct) investors assess government policy. This was the core of the dissertation proposal I had defended a few months before, and it had been the basis for my grant proposals. I asked my advisers for suggestions on how to do the interview piece, and they suggested that I “talk to people” and figure it out from there. So, there was quite a lot of learning by doing. (A side note: that experience, of not feeling like I had much training in the use of interviews, and not feeling like we do much to train our PhD students in that still, was what motivated the project that produced *Interview Research in Political Science*, a 2013 book I edited. Extra side note: the conference for that book was co-funded by the Browne Center,

because Ed Mansfield heard my complaining about a lack of training, and offered to pay for me to do something about it. Duke University's Center for International Studies also contributed, many years after I had finished my Duke PhD!).

I arranged my interviews the old-fashioned way (this was in 1997), sending letters by mail to institutional investors (located using professional directories as well as word of mouth and suggestions from financial journalists), describing my project, and hoping they'd phone or write (occasionally by email, often by snail mail) back to set a meeting. They didn't all, but I did manage to get access to a reasonable number of investment professionals. I took lots of notes during and immediately after the meetings, but I didn't record the interviews (it didn't really occur to me to ask, given the clunky technology available at the time; but I also recall worrying that recording would change the tenor of our conversations).

[On the challenges of doing interviews, I probably have lots more to say. One observation is that gender was very much on my mind, as nearly everyone I interviewed was male – partly reflecting the structure of finance, especially the upper echelons of finance, at the time. Sometimes, being the young woman probably worked to my advantage, as senior men were eager to explain to me how things worked. But it does lead one to reflect on positionality – on how one's real or perceived identity affects the access and answers one gets. My experience doing interviews as a more senior scholar has been quite different, probably for a host of reasons].

One thing I was careful about was keeping in touch with my committee during my time in the field. I'm not sure if they really needed or wanted me to do this, but it made me feel better to write every couple weeks, with a summary of what I'd been doing and what I'd been learning from the interviews. At a fairly early stage in my time in the field, I realized that it might be better to focus on a single type of (portfolio market) investors, and to consider how the ways in which they assessed countries varied (across types of countries, based largely on perceived creditworthiness). This meant leaving aside the foreign direct investment piece; despite a fun interview visit to the Honda plant in Reading, I was realizing that those foreign firms who had chosen to be in the UK were a rather specific subset of FDI, and that the sovereign bond market was what really interested me. (As it turned out, I came back to FDI a few years later, with my work on labor rights).

I did a follow up round of interviews in fall 1998, funded by an NSF Dissertation Improvement grant (yes, the NSF funds qualitative fieldwork in political science). The timing was lucky, in that these interviews came right after the Asian financial crisis (the main interview work was done January-June 1997). And they allowed me to dive a bit more into the question of how investors evaluate emerging market (versus developed country) borrowers – something that I discuss at greater length in my 2003 book.

I wrote my dissertation as a book-length narrative (which was what nearly everyone did at the time!). I had pulled out material from the theory and one of the empirical chapters to use as the basis for my job talk, so I had some sense of how I might craft an article from some of the

dissertation material. I drafted this article around the same time I defended, in mid-1999, and I sent it out for review (again, times have changed: I mailed four paper copies of the piece to the editors, and my paper had no data appendix).

One of the issues I thought about in writing the article (and the dissertation) was how to cite the interviews: I wanted to give enough information to establish credibility (that I'd talked to people who really could tell me something about investors' assessments of sovereign debt), but I didn't want to violate the guarantees of confidentiality I'd made to interviewees. I settled on citing the interviews by number, with a list of interview dates and financial institutions included. The reviewers and editors never commented on this – perhaps because there wasn't really a sense that there were best practices for reporting about interviews. (In retrospect, I'd have provided more information about how I recruited interviewees, about who did versus who didn't agree to meet with me, and perhaps about the context – what was happening politically or economically at the exact time of the interview – in which the interviews took place). Of course, the fact that I relied on statistical analyses as well as interviews probably helped reviewers to trust the interview-based evidence: the interviews and the cross-sectional time series analyses offered results that were rather consistent with one another.

Finally, I was incredibly lucky with this piece: the editors of IO responded a few months later, soon after I had begun my faculty job at Notre Dame, with a revise and resubmit. I didn't have much experience of the publication process – there was far less pressure to publish while in grad school than there is today; and while my advisers were fabulous in many respects, we just didn't talk much about that element of the profession, nor did we co-author work during my time in grad school. So perhaps I didn't appreciate just how rare this outcome was – an R&R on the first try, and at IO! Moreover, as best I can recall, the reviewers' suggestions were very helpful and very consistent with one another, so revising was straightforward. When it came time to publish my book, presses did want assurances that the IO article (which was already out; my book went out for review in early 2001) would be an advertisement for the book, rather than a substitute for it. Especially because I had added quite a bit of substantive material (including some archival work from the pre-WWI era of globalization) to the book as I revised it, I could make that case. And it turns out that the book now has twice as many citations as the article, so perhaps I was correct!