Comments on Fierke, Klotz, Hererra

Let me begin with general comments on the works of the three authors grouped in the “Methods and Identity” panel. As one whose scholarship has ranged from reliance on traditional “methods” to experiments, text analysis, and to what I would argue are social constructivist friendly computational models (coming out of a computational linguistics tradition), I applaud the variety of intellectual approaches adopted by these authors. As you will see below, my comments on the majority of work here focuses more on substantive factors than on methods. In particular, I find myself prodding two of the authors in this group to more overtly include psychological issues and underpinnings in their analysis. In the third case, I pay more attention to questions of technique and its promises and limitations.

I look forward to dialogue with each of the three authors in this group. Please tell me where I have misunderstood your work.

Fierke

In her EJIR piece, Karin Fierke presents an excellent analysis, to which my primary addition would be a serious consideration of political psychological factors such as problem representation. Fierke's framework is illuminating, but it doesn't address the issue of how the inconsistencies arrive in the first place. How do the parties adopt their initial understandings of the conflict or what creates the context for social interaction? The assumption (p. 340) that actors are engaged in the same game is challenged by the introduction of such concepts as ontology and problem representation. There is apparently some overlap between the concept of "typification" and problem representation, and this should be explored.

A minor comment concerning the use of the word "abduction" is in order here. My intellectual wanderings have introduced me to a different use of the term than Fierke employs. Josephson's treatment, grounded in the fields of philosophy and "artificial intelligence" focuses on an iterative midpoint between induction and deduction. For Josephson, abduction is certainly
not a logical error.

I am a bit uneasy with such assertions as (p.344) the existence of "the realist logic." I would be much more comfortable with a specific textual and psychological basis for such claims. Fierke is careful in her typifications to cite the source of her analysis, but doesn't soundly make the case that all such typifications can comfortably be lumped into a single "realist logic."

In short, I am impressed with this piece, but believe it would be enriched with a dialogue with political psychology.

Fierke's ISQ piece makes an important argument concerning the use and importance of language. My central response is to argue that some of the same ground with respect to the use of language that she stake out is also inhabited by those of us who describe ourselves as non-positivist political psychologists. There is some discomfort from this "post-positive political psychologist" with Fierke's emphasis on games and what I see as their rationslist basis. If this is not a careful enough read on my part, I seek dialogue.

Klotz

Klotz's basic contention that norms play a more important role in IR than many - especially realists - acknowledge, is an argument with which I agree wholeheartedly. Her push for a less static view of "interest" is also one which I applaud. In addition, I concur with her critique of the applicability of game theoretic approaches to address some contentious claims in IR. When Klotz asserts that interpretive theory "asks under which conditions regime theories might offer us useful auxiliary, not core, explanations" (p. 19), I agree, but feel the need to add two caveats. First, interpretive theories also have the capability to offer additional core explanations. Second, a comprehensive response to the query posed above by Klotz would be enhanced by overt consideration of psychological variables that have an impact on both regimes and decision makers in states and other entities involved in the functioning of regimes. In other words, I find that Klotz's Table 2 cell (and subsequent discussion) of "origin of norms" in interpretive theory is incomplete without overt consideration of the psychological underpinnings
of norms. Similarly, psychological variables can play a role in norm change as well.

**Hererra**

As one who has undertaken a fair amount of text analysis (not to mention supervising more dissertations using such an approach than I would care to count), I approach Hererra’s work cognizant of both the potential and the limitations of this class of techniques. This is one work that I do not critique for lack of attention to psychological variables. The consideration of cognitive models is a step which I applaud. The underlying assumption that cognition plays a role in forming the content of a collective identity, but that identity is not only cognitive is a position with which I am clearly in agreement. The advocacy here of multi-method approaches is also consistent with my own approach to scholarship in this and other areas.

I feel the need to add a few caveats to the section on quantitative content analysis. I respond to the claim that “Quantitative content analysis is analytically quite similar to discourse analysis since it is used to describe the content of texts that, in turn, reveal insights into collective identities” (p. 9) cautiously. I am supportive of that statement only if it is stated contingently. As I see it, the asserted similarity depends upon the unit of analysis in the quantitative content analysis, and the sophistication of the dictionary (computer or human) that is accessed in the analysis. While I am familiar with the specific computer content analysis examples cited, and concur that “advances in computing over the years have led to faster and more sophisticated performance...” even the most zealous computational linguists and political scientists would urge caution, noting that the systems cited haven’t yet reached the sophistication of an average undergraduate student. They are, admittedly, more consistent, however.

My work on problem representation hints at my affinity for cognitive mapping approaches, but with a caveat there, too. Most such approaches limit themselves to positing only “causal” linkages. English and other languages offer us many more “connectors” than “cause.” This is acknowledged by such systems as those developed by Michael Young, but not by most others labeling themselves as cognitive mapping. These additional connectors are particularly
important for most understandings of collective identity.

Surveys, interviews, and experiments all offer the opportunity to gain some insight into collective identity. Their main trap is constraining the meaning of identity before investigating the nature of the phenomenon. As Hererra understands, though, the other approaches advocated have the potential to offset these disadvantages.