

ANALYTICAL

A Sociological Analysis of the Decline of American IR Theory

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In recent years the centrality of grand, paradigmatic theory in American IR has eroded, with the vacuum being filled by largely atheoretical “hypothesis-testing” research. Although a heated debate has emerged on whether it is good or bad for IR, hardly anyone has tried to analyze this trend. I offer an analysis grounded in a conceptual framework elaborated by sociologist Richard Whitley. In the 1980s and 1990s IR approximated the type of social organization Whitley labeled “polycentric oligarchy”—a hierarchic structure dominated by leaders of competing schools, toward which scholars orient their research. In recent years the field has become more of a “fragmented adhocracy.” Its reputational hierarchy has become more fluid and, concomitantly, its intellectual output has become more fragmented, more empirical, and less oriented toward a theoretical center. To account for this change, I discuss three external forces that reduce American IR’s “reputational autonomy”: the corporatization of American higher education; a surge in the availability of research funding from the Pentagon and other defense agencies; and the enduring embeddedness of American IR in the political science discipline. To strengthen my argument, I compare American IR to its Australian counterpart on these three dimensions.

Keywords: IR theory, sociology of science, Government funding of IR

In recent years, the centrality of theory in the field of International Relations (IR) in the United States has been eroding. Although many course syllabi continue to be organized around theoretical “paradigms” or “isms” (Maliniak *et al.* 2011, 441), and theorists such as Robert Keohane, Alexander Wendt, and Kenneth Waltz are still regarded by their peers as the field’s most influential scholars (Jordan *et al.* 2009), American IR scholarship is becoming increasingly indifferent to theoretical paradigms and disinclined to engage in abstract theorizing. Surveys conducted in 2004, 2006, and 2008 have shown a marked decrease over time in the percentage of American IR scholars who identify with one of the field’s major paradigms or characterize their research as paradigmatic (Jordan *et al.* 2009).

Furthermore, according to a content analysis of leading IR journals, whereas in the 1980s and 1990s “it seemed impossible to publish an article . . . if the researcher did not situate the work within and choose a side in the ongoing paradigm wars,” in recent years the proportion of “non-paradigmatic articles” has increased considerably (Maliniak *et al.* 2011, 441–45). Most of these non-paradigmatic articles

consist in, as John Mearsheimer and Stephen Walt (2013, 428) put it, “hypothesis testing. Theory usually plays a minor role in this enterprise, with most of the effort devoted to collecting data and testing empirical hypotheses.” This scholarship’s primary mode of testing is statistical. The surveys reveal that in 2002, “quantitative overtook qualitative” approaches in top journals even as most scholars still describe their work as primarily qualitative (Maliniak *et al.* 2011, 451).

As Benjamin Cohen (2010, 889) observed, the decline of theory-centered research entails “an increasing fragmentation of journal contents. We get isolated bits and pieces—a statistically significant regression here, a thoroughly documented case study there—but little sense of how all the pieces fit together.”¹ The point is not that this fragmentation was preceded by theoretical unity. On the contrary, the intellectual life of IR theory has been marked by “inter-paradigm debates” (Wæver 1996, 149) and even “paradigm wars” (Maliniak *et al.* 2011, 441). Still, for better or worse, these debates constituted a common core to which scholars oriented their research, specialized though their research questions might have been. Thus, even as it consisted of multiple, warring schools, IR theory operated as an integrative intellectual force. It gave IR a common conceptual vocabulary and structure that marked the field off from cognate disciplines and coordinated knowledge production across subfields and specialties.

The decline of grand theorizing has been encouraged and reinforced by prominent calls to move IR “beyond paradigms” (Sil and Katzenstein 2010) or beyond paradigmatic “sectarianism” (Lake 2011). In response, other scholars have defended IR’s “isms” (Nau 2011), urging the field to “restore theory to its proper place” (Mearsheimer and Walt 2013, 449). Thus, a new debate seems to be brewing, pitting scholars who welcome the decline of grand theory against those who lament it.² While I think that, on balance, the declining centrality of paradigmatic theorizing and the concomitant rise of “hypothesis testing” are bad for IR, the objective of this essay is not to argue against this trend but to conceptualize it and explain why it has occurred. My analysis is grounded in the conceptual framework elaborated by organizational sociologist Richard Whitley in his influential book, *The Intellectual and Social Organization of the Sciences* (2000).³ Notwithstanding Whitley’s dense writing style, this oft-cited book has justly earned praise for its “original” and “penetrating” analysis (Barber 1986, 349; Camic 1996).

But before I outline Whitley’s framework, a clarification is in order. Some readers might question the premise that IR theory is in decline. They might cite the recent creation of the journal *International Theory* (IT) and the recent establishment of a Theory section of the International Studies Association (ISA). But the creation of institutional spaces devoted exclusively to IR theory indicates trouble as much as success.⁴ In the 1980s and 1990s, there was no felt need to dedicate professional space to theory because, even when their research was primarily empirical and their research questions were substantively narrow, IR scholars tended as a matter of course to situate their claims within grand-theoretical debates. The importance of theoretical engagement went without saying. Today, on the other hand, even as the production of theoretical knowledge may be thriving, it appears to have become increasingly confined to self-styled theorists and increasingly irrelevant to the research of empirically inclined scholars. Indeed, we may be witnessing within American IR an echo of the development of American political science after World

¹Feminist scholar Christine Sylvester (2010, emphasis added) offers a similar if not identical assessment: “IR today exists as *knowledge fragments* that take shape as a camp whenever a group of like-minded scholars take exception to the already existing array of IR knowledges. The overall result is a minoritarian IR”

²A prominent manifestation of this emergent debate was a series of panels on “The End of IR Theory?” at the 2012 meeting of the International Studies Association. The *European Journal of International Relations* sponsored the panels, and the journal published some of the papers in its September 2013 issue.

³I thank Ole Wæver for bringing this book to my attention.

⁴I thank Stefano Guzzini for this insight.

War II, when political theory's emergence as a distinct subfield signified its relegation to the margins of an increasingly empirical discipline (Gunnell 1993).

The production of theory appears not only to be increasingly fenced off from IR as a whole, but also increasingly concentrated outside the United States. Two of the four officers of the ISA's Theory section and two-thirds of its advisory board members are affiliated with non-American universities.⁵ Likewise, two of IT's three coeditors and two-thirds of the journal's advisory board members are employed by institutions outside the United States.⁶ Moreover, of the thirty-four authors and coauthors who contributed to IT in 2014 (volume 6), twenty-four (71 percent) were affiliated with non-American universities. By contrast, only 23 percent of contributors to *International Organization* (IO) in 2014 (volume 68), and an identical proportion of contributors to *International Security* (IS, volume 39, 2014–15), were based outside the United States.

In sum, with the aid of Whitley's framework, I seek to interpret not the decline of theoretical IR scholarship *per se* as much as the declining *centrality* of theory, that is, its diminishing function as an integrative core of the discipline. I furthermore attempt to explain why in *American IR*—the main object of my analysis—the declining proportion of research oriented toward the paradigmatic core is concomitant with a rise in quantitative “hypothesis-testing” research. As part of my analysis, I will sketch comparisons between the socioeconomic environment of American IR and its counterpart in Australia, where hypothesis testing has made little headway and theoretical (if not paradigmatic) research appears to flourish.

Whitley's Framework: The Sciences as Reputational Work Organizations

Whitley played a pioneering role in incorporating into the sociology of science insights from theories of organization. Specifically, he argued that in some respects scientific fields resemble “craft” systems of work administration (Stinchcombe 1959, 168).⁷ In these organizations, workers receive certification of their skills and enjoy at least partial autonomy from employers in performing their work tasks. Similarly, scientists become certified to conduct research by earning a doctoral degree and often enjoy considerable autonomy from employers in determining what research to pursue and how (Whitley 2000, 14–16).

But scientific work organizations also have unique features. Whereas craft workers continually apply knowledge and work methods acquired in training, scientists are expected to produce novel knowledge and methods. The commitment to innovation renders the “task outcomes” of scientific work more variable and less predictable than those of other crafts (Whitley 2000, 17). Often it is not self-evident how novel contributions fit within the structure of existing knowledge. Scientific work, then, is characterized by considerable *task uncertainty* (Whitley 2000, 17–19).

Another peculiarity of the sciences is that they are “*reputational work organizations*,” that is, “systems of knowledge production that are organized around the competitive pursuit of reputations for published contributions to collective intellectual goals” (Whitley 2000, 25). Scientists' reputations depend crucially on their ability to persuade powerful colleagues of the competence of their research procedures and the significance of that research for collective goals (Whitley 2000, 12).

⁵See “Theory Section,” International Studies Association, accessed July 22, 2015, <http://www.isanet.org/ISA/Sections/THEORY>. The four officers are Section Chair Annette Freyberg-Inan (University of Amsterdam), Vice-Chair Daniel Levine (University of Alabama), Treasurer Shannon Brincat (University of Queensland), and Secretary Kathleen Brennan (University of Hawaii–Manoa).

⁶See “Volume 8 Issue 1 Cover and Front Matter,” *International Theory*, accessed June 8, 2016, <http://journals.cambridge.org/action/displayFulltext?type=1&pdfType=1&fid=10154523&jid=INT&volumeId=8&issueId=01&aid=10154522>.

⁷Whitley uses “scientific field” and “intellectual field” interchangeably.

More than other organizations, then, reputational work organizations are characterized by “task uncertainty” and by “mutual dependence” among their members. Whitley subdivided each of these concepts into two forms. “*Technical* task uncertainty” refers to the degree to which work techniques produce reliable, stable empirical results that fit unambiguously with extant findings. To the extent that scientists do not agree on the precise properties of their objects of study, empirical results are likely to be ambiguous and open to multiple interpretations even if the methods being employed are highly technical (Whitley 2000, 121–22). “*Strategic* task uncertainty” pertains to theoretical more than empirical recognition. It describes the uncertainty scientists face regarding whether the larger community will take up their work: does it tackle a problem the community considers significant or high-priority? Does it employ the right analytical approach? Absent a unified theoretical core, scientists may find it particularly hard to answer these questions (Whitley 2000, 123).

Whitley’s second key concept, “mutual dependence”, is subdivided into “functional” and “strategic” aspects. “Functional dependence” describes the need of researchers “to use the specific results, ideas, and procedures of fellow specialists in order to construct knowledge claims which are regarded as competent and useful contributions” (Whitley 2000, 88). It means that, to publish results and harvest reputations, researchers must convince specialists in their subfield that their research meets the subfield’s norms of technical competence and that their findings are useful to them. The second aspect, “strategic dependence”, pertains to the scientific field as a whole more than its subspecialties (Whitley 2000, 88–89). It refers to scientists’ need to persuade colleagues of the significance of their research problem and analytical approach. Scientists gain high reputations to the extent that professional leaders recognize their research strategies and/or intellectual concerns as central to the field’s agenda and congruent with its intellectual priorities (Whitley 2000, 88–89).

Whitley’s most original insight is that scientific fields vary in their organizational features and, consequently, in the structure of the knowledge they produce. While they generally exhibit relatively high levels of task uncertainty and mutual dependence, scientific fields nonetheless differ from each other and vary over time in their degree of (technical/strategic) task uncertainty and (functional/strategic) mutual dependence. Some fields may combine a high degree of technical and/or strategic task uncertainty with a low degree of functional and/or strategic uncertainty; others may exhibit the opposite pattern or be characterized by uniformly low or high levels of all variables. To capture the variety of possible combinations, Whitley constructed a taxonomy whose types can be arrayed along a continuum.

On one end are intellectual fields combining high functional and strategic dependence with low functional and strategic task uncertainty. In such fields, empirical objects are unambiguously defined and work techniques well understood, producing stable and predictable results whose added value can be assessed with minimal controversy. Research problems are prioritized relatively clearly and collective professional goals are relatively stable (Whitley 2000, 124). Although most scientists toil in highly specialized subfields, competence standards are widely shared and effectively policed. Moreover, scientists from various subfields jockey to gain recognition for their specialty and convince gatekeepers of the significance of their research problems for the field as a whole. These fields typically feature a unified theoretical framework to which scientists across subfields orient their work (Whitley 2000, 93–94). In sum, “conceptually integrated bureaucracies,” as Whitley labels them, produce specific, unambiguous empirical knowledge integrated by a unified theory (Whitley 2000, 158). Twentieth-century physics epitomizes this type.

On the continuum’s opposite end is “fragmented adhocracy” (Whitley 2000, 159). This type contrasts with conceptually integrated bureaucracies on all dimensions, combining low functional and strategic dependence with high technical

and strategic task uncertainty. Fragmented adhocracies lack both a conceptual vocabulary that sets them apart from lay language and standardized definitions of empirical objects. Because empirical objects are typically referred to in everyday terms (e.g., democracy, corruption), the use of specialized techniques hardly guarantees that research results would be accepted without controversy. Empirical results tend to be ambiguous and open to multiple interpretations. Furthermore, there is no clear hierarchy of research priorities and little centralized theoretical integration. These fields thus produce diffuse knowledge that tends to be shaped by external pressures more than internally determined research priorities. Management studies and many of the human sciences approximate this type (Whitley 2000, 158–60).

For my analysis, it is not necessary to recapitulate all the types along the continuum, save for one: “polycentric oligarchy” (Whitley 2000, 160). Polycentric oligarchies exhibit significantly higher strategic dependence than fragmented adhocracies even as the two types share three other attributes: high technical and strategic task uncertainty and low functional dependence. Because in polycentric oligarchies empirical results remain ambiguous, there is no systematic accumulation of empirical results, especially across specialized research areas. However, high strategic dependence means that “scientists become more oriented to the views and intellectual ideas of a small group of intellectual leaders who control scarce resources” (Whitley 2000, 160). These “oligarchs” exercise control of resources (e.g., journals, jobs) “locally and through personal knowledge” (Whitley 2000, 160). Thus, in such fields:

research is organized into competing schools based on leadership entrenched in employment organizations and control over journals. Knowledge here tends to be more theoretically oriented and coordinated than in [fragmented adhocracy], as scientists have to demonstrate the importance of their contribution to the school’s overall program rather than simply claiming reputations on the basis of competence in empirical structures. (Whitley 2000, 160)

In sum, although they exhibit nowhere near the degree of theoretical unification featured by, say, twentieth-century physics or even economics, polycentric oligarchies do feature a significant degree of theoretical coordination revolving around competing schools. Whitley (2000, 160) cites German psychology before 1933, British social anthropology, and German philosophy as examples.

In addition to constructing a typology of scientific organizations, Whitley elaborated three contextual conditions that shape the degree of a field’s task uncertainty and/or mutual dependence. I will discuss one of these conditions below. For now, it is time to return to American IR: where does it fit within Whitley’s taxonomy?

IR: A (Former) Polycentric Oligarchy

I argue that the social and intellectual organization of American IR in the 1980s and early 1990s approximated a polycentric oligarchy and that the subsequent decline of IR theory can be understood as a move toward (if not necessarily all the way to) a fragmented adhocracy.

As noted, polycentric oligarchies combine low functional dependence with high strategic dependence, technical task uncertainty, and strategic task uncertainty. I will discuss how, in the 1980s and early 1990s, American IR exhibited these attributes.

High Technical Task Uncertainty

In sciences in which technical task uncertainty is low, physics for example, scientists typically apply standardized, well-understood research techniques to empirical objects whose properties are restricted and whose definitions are widely shared

and precise, often stated in formal language (Whitley 2000, 122). Under such conditions, a scientist can reasonably expect that, should she competently apply the field's standardized techniques and confirm the research hypothesis, the result's novelty and place within existing empirical knowledge (if not necessarily its theoretical significance) would be readily recognized by colleagues.

This was not the case, however, in American IR. The field's empirical objects are often phenomena whose names in IR's vocabulary are common in lay language: war, peace, power, stability, cooperation, and so on. Therefore, when a scholar claims that, say, peaceful relations between two countries were caused by their shared democratic norms, she is open to challenges by laypersons and colleagues on the grounds that country X was not "truly" a democracy or that relations between X and Y were not "truly" peaceful. Furthermore, even when they study objects whose names are not common in everyday language, IR scholars rarely share a precise, restricted definition of these objects' properties. For example, there is no consensus in IR on the definition of "distribution of power" or cognate concepts like "polarity" or "hegemony." Therefore, it is unsurprising that there is disagreement on whether the nineteenth century was characterized by multipolarity (Waltz 1979) or hegemony (Gilpin 1981), nor on whether the post-World War II period was hegemonic (Gilpin 1981; Keohane 1984) or bipolar (Waltz 1979). Consequently, when an IR scholar advances an empirical claim, even if she subjected it to a technical test, she cannot be even remotely certain that the claim would be recognized widely as a specific, cumulative contribution to the field's stock of empirical knowledge.

High Strategic Task Uncertainty

This concept refers to the uncertainty of researchers regarding whether their research problems, theoretical ideas, and/or analytical approaches would be widely recognized as significant and congruent with the field's intellectual priorities. As Ole Wæver (1998, 717) pointed out, although task uncertainty is rather low within most IR subfields—one usually has a good sense of which questions and approaches are considered appropriate—"Across subfields there is very high task uncertainty. A specialist in strategic studies who suddenly decides to write a feminist analysis of immigration policy . . . might have learned the techniques of this type of analysis in a general sense, but their application is sufficiently conventional to disable the writer from using the appropriate style and making the expected inference."

Strategic task uncertainty in IR is reinforced by the instability of the field's intellectual priorities. Because IR's research agenda is at least partly shaped by changing international political circumstances, researchers can hardly be certain that the problem they just began to work on will still be significant when the project comes to fruition. In the mid-1980s, nuclear deterrence, Soviet foreign policy, and arms races were high on the agenda of American IR, but researchers who only then began working on these problems found that, by the time they completed their dissertations or manuscripts (coinciding with the end of the Cold War), their colleagues had largely lost interest. Similarly, terrorism has become a high-priority research problem after 2001, but a PhD student who commences a dissertation on terrorism today cannot count on the problem remaining "hot" in a few years.

Low Functional Dependence

Functional dependence refers to the degree to which scholars depend on one another within subfields or research areas: how hard is it to gain fellow specialists' recognition of the competence of one's research and usefulness of one's results for future research? With regard to IR in the 1980s and 1990s (as well as today), the answer is not terribly hard. As Wæver observed, becoming recognized as competent

contributors to specialized research areas is not difficult. “Most subfields are relatively tolerant, welcome new members, and are not terribly competitive” (Wæver 1998, 718). Although getting an article accepted by one of IR’s specialized journals (e.g., *Foreign Policy Analysis*; *Global Environmental Politics*) is no child’s play, it is significantly easier than publishing in the leading field-wide journals.

High Strategic Dependence

In the 1980s and early 1990s, American IR featured a pyramid-like hierarchical structure. The top was unmistakably dominated by theorists, with Kenneth Waltz and Robert Keohane—authors of the canonical texts of “neorealism” and “neoliberal institutionalism”—clearly standing above everyone else. To a significant extent, reputation seekers needed to persuade the field’s elite that their work was significant and central to the field’s research agenda. For better or worse, advancing one’s career was, in other words, highly dependent on receiving validation from the profession’s leading theorists in the form of favorable peer reviews of manuscripts, book blurbs, references to one’s work, letters of recommendation, and tenure and promotion reviews.

Tenure-track and tenured academic jobs were a major scarce resource whose distribution was significantly influenced by IR’s elite theorists. The point is not that they controlled the job market in any direct, formal fashion, but rather that they were probably asked to review tenure and/or promotion dossiers more frequently than other scholars and that their judgments significantly influenced, if by no means determined, the outcome of these cases. Similarly, a letter of recommendation or a phone call from, say, Keohane likely gave a candidate an edge on the job market. In those years, both Keohane and Waltz attracted a large number of ambitious and smart graduate students or mentees, many of whom became major scholars in their own right.⁸ These two leading theorists thus became the linchpins of informal networks of former students, mentees, and protégés. These networks, in turn, reinforced the informal influence that their chiefs exercised in the academic job market.

Journal space was another important scarce resource whose distribution was significantly shaped by the field’s elite. To earn a strong reputation and harvest the rewards that come with it (e.g., salary raises, speaking invitations), it was essential for American IR scholars to publish in a small number of highly competitive top-tier journals (Wæver 1998, 717–18). The fierce competition for space meant that, for their ideas to be taken up by the IR research community, scholars were dependent on a rather small group of peers who edit these journals or influence journal content in indirect ways (e.g., serving on editorial boards; participating in the selection of editors; being consulted informally by editors).

In the 1980s and 1990s, the top-tier journals were “mainly defined, structured, and to a certain extent controlled by *theorists*” (Wæver 1998, 718; emphasis added). Nowhere was this theoretical bent more evident, perhaps, than in what was arguably the field’s most prestigious journal: *IO*. The front matter of the Autumn 1985 issue shows that the editorial board included an impressive roster of highly visible scholars whose reputations were based largely on theoretical contributions—primarily to liberal or liberal-institutionalist IR theory. The board’s chairperson (and former editor) was Keohane (in a recent survey of IR scholars, he topped the list of the field’s most influential scholars; Jordan *et al.* 2009, 43). The editor was Peter Katzenstein, whose theoretical analysis of the links between domestic and international political structures has been highly influential

⁸Keohane trained or helped train, among others, Lisa Martin, Beth Simmons, Helen Milner, Andrew Moravcsik, John Owen, and Ronald Mitchell. Waltz trained or helped train, among others, Barry Posen, Stephen Van Evera, Stephen Walt, Christopher Layne, Shibley Telhami, James Fearon, Robert Powell, Benny Miller, and Avery Goldstein (see Walt 2013).

(Katzenstein ranked ninth in the survey). And, the “rank and file” of the board included luminaries such as Robert Jervis (ranked seventh), Robert Gilpin (thirteenth), John Ruggie (fourteenth), Ernst Haas, Harold Jacobson, and Peter Gourevitch. Under their leadership, *IO* has almost entirely ceased publishing empirical studies of actual international organizations (Kratochwil and Ruggie 1986), becoming instead an outlet for some of IR’s most influential theoretical ideas (e.g., international regimes; embedded liberalism; and agent-structure).

A similar, if not quite as strong, pattern can be seen in *IS*. When *IS* was launched in 1976, its sixteen-member editorial board included only one prominent IR theorist, Hedley Bull. By the late 1980s, even as the majority of board members still consisted of security experts from outside IR, the presence of IR theorists had increased significantly with the addition of Jervis, John Mearsheimer (ranked fourth), Samuel Huntington (eighth), Alexander George, Stanley Hoffmann, and Barry Posen.⁹ While *IS* has always maintained a strong commitment to policy-relevant research, in the late 1980s and 1990s it published a significant number of articles that addressed major theoretical debates (e.g., the articles on the democratic peace later compiled in Brown *et al.* 1996)

In sum, in the 1980s and early 1990s, the fortunes of scholars who aspired to “make it big” in American IR depended highly on their ability to persuade a narrow professional elite of the significance of their research problems and appropriateness of their analytical approaches. The elite was predominated by scholars whose reputations rested primarily on theoretical contributions.

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Whitley (2000, 160) argued that the intellectual output of “polycentric oligarchies” is characterized by three features: (1) “Research is organized into competing schools based on leadership entrenched in employment organizations and control over journals.” (2) Knowledge tends to be “theoretically-oriented and [theoretically] coordinated”—scientists “have to demonstrate the importance of their contributions to the school’s overall program rather than simply claiming reputations on the basis of competence in empirical structures.” (3) Empirical results are “diffuse” and “relatively idiosyncratic to local conditions of their production and interpretation.”

In the 1980s and early 1990s, American IR nicely exhibited all three features. That IR research was organized into “competing schools” (“paradigms”), or that reputations depended on demonstrating the significance of one’s research to the agenda of these schools, is so evident (at least to those of us who began their careers in that period) that it requires no elaboration (see Maliniak *et al.* 2011, 441).

The “diffuse” and “idiosyncratic” nature of empirical knowledge in IR has been testified to by numerous laments of the lack of “progress” and/or “accumulation.” “Nothing seems to accumulate” in IR, Waltz (1979, 18) quipped, “not even criticisms.” This was by no means confined to qualitative research. Leading quantitative methodologists articulated some of the most trenchant critiques of IR’s failure to produce stable, standardized, cumulative knowledge. Beck *et al.* (2000, 21), for example, complained that

Despite immense data collections, prestigious journals, and sophisticated analyses, empirical findings in the quantitative literature on international conflict are frequently unsatisfying. Statistical results appear to change from article to article and specification to specification. Any relationships usually are statistically weak, with wide confidence intervals, and they vary considerably with small changes in specification, index construction, and choice of data frame.

They added that, as a result, “those with deep qualitative knowledge of the subject are rarely persuaded by conclusions from quantitative works” (Beck *et al.*

⁹This list is based on the front matter of the Winter 1988/89 issue.

2000, 21), Translated into Whitley's terminology, this statement reads: in quantitative IR, the degree of technical task uncertainty is high.

Interpreting the Changing Character of American IR

Recent changes in the intellectual landscape of IR in the United States—the declining identification with paradigms and rising proportion of largely atheoretical “hypothesis-testing” research in leading journals—mean that IR's current intellectual output no longer fits the polycentric oligarchy type, at least not as neatly as in earlier years. While the empirical knowledge generated by IR scholars remains “diffuse” and unstable—the criticisms of Beck *et al.* remain on the mark notwithstanding the growing popularity of quantitative techniques (Mearsheimer and Walt 2013)—the degree to which American IR exhibits the other features of a polycentric oligarchy has been attenuated: “competing schools” (or “paradigm wars”) have become less central, and research has become less oriented to the theoretical concerns of these competing schools.

This is associated with a change in the field's social structure. While technical and strategic task uncertainty in IR remains high, and functional dependence remains low, strategic dependence has been decreasing. In other words, the diminishing intellectual centrality of debates among competing schools is associated with a decline in the degree to which established fellow scholars control the reputations of aspiring IR scholars. More specifically, the top of the field's hierarchy pyramid appears to have widened somewhat relative to the bottom. And it has become less obvious who the leading “oligarchs” are.

To some extent, the flattening and blurring of hierarchy in IR has been a function of biology. Kenneth Waltz (1924–2013)—who was crowned “King of Thought” by the department of international politics at Aberystwyth University (Booth 2009, 179)—retired from the University of California–Berkeley in the early 1990s and, though he remained intellectually active, his involvement in the profession subsided. Waltz continued passionately to defend neorealist theory on the intellectual front (in published articles, interviews, and speaking engagements), but his ability to advance its fortunes on the social front—through training graduate students and gatekeeping activities such as letter writing—diminished after retirement.

Waltz's chief theoretical rival, Robert Keohane (b. 1941), is also approaching retirement. Much like Waltz, Keohane became “a giant,” as Ruggie put it, “not only because he has produced innovative ideas and approaches, but also because of his teaching and mentoring” (Milner and Moravcsik 2009: back cover). But even as he continued advising PhD students after his departure from Harvard to Duke University in 1996, Keohane's teaching and mentoring output appears to have diminished. Keohane's presence in IR's intellectual landscape remains imposing, but his social clout may have diminished somewhat in recent years.

Now, no discussion of American IR's contemporary landscape is complete without acknowledging the dramatic rise of the constructivist paradigm in the 1990s. Like neorealism and neoliberal institutionalism, constructivism has an iconic figure: Alexander Wendt. In a recent survey of IR scholars from ten countries, Wendt was identified as the field's second most influential scholar, behind Keohane (Jordan *et al.* 2009, 43).¹⁰ But two factors have diminished Wendt's ability to function as the chief “oligarch,” the ultimate gatekeeper, of a constructivist “school.” First, Wendt did not establish deep roots or identify with any single academic institution. Over the two and a half decades of his post-PhD career, Wendt wandered from Yale University to Dartmouth College to the University of Chicago to Ohio State University. Second, Wendt has wandered not only physically but also intellectually. Unlike Keohane and Waltz, who persisted in defending and advocating for their

¹⁰Waltz ranked third.

theoretical schools, Wendt self-consciously decided to “get out of the constructivist business: I’m trying to stay alive by focusing on other issues as well, like for example the quantum perspective of social science I’m currently investigating” (Wendt 2008). Thus, although Wendt certainly has the intellectual cachet required of a leading “oligarch,” he seems to lack the desire to exercise such leadership.

Of course, chief “oligarchs” are not irreplaceable. John Mearsheimer may arguably have succeeded Waltz as the leader of the realist school, but Mearsheimer is nearing retirement and has no heir apparent. One of Keohane’s former students who went on to become major scholars—Helen Milner, Beth Simmons, and Lisa Martin come to mind—may perhaps succeed him as the undisputed leader of the liberal school, while prominent scholars such as Martha Finnemore and Kathryn Sikkink have the potential to emerge as top “oligarchs” of the constructivist school. But even if these scholars (or a young, emergent realist scholar) were to pursue actively oligarchic leadership of their respective schools, they would be greatly constrained by emergent socioeconomic forces that reduce the dependence of IR scholars on one another for reputations and thus dilute the influence of central gatekeepers. Coupled with the generational change discussed above, these socioeconomic forces have been pushing IR in the direction of a fragmented adhocracy, that is, a scientific field with a relatively “fluid” social hierarchy, producing knowledge that is fragmented and diffuse on the theoretical as much as empirical side (Whitley 2000, 159).

Contextual Factors Affecting the Decline of Theory and Rise of “Hypothesis Testing”

In addition to generational change within the field, the recent decline in “strategic dependence” in American IR and the intellectual developments associated with it—a decline in the centrality of competing theoretical schools and concomitant rise in “hypothesis testing” research—have been shaped by two recent trends and one enduring condition *external* to the field: (1) growing pressure on the American academy to adopt corporate modes of management; (2) a large increase in availability of research funds from defense-related agencies of the US government; and (3) American IR’s enduring embeddedness in the discipline of political science. By chipping away at and limiting the “reputational autonomy” (Whitley 2000) of American IR, these factors diminish the dependence of IR scholars on each other for reputations.

I will recapitulate Whitley’s concept of reputational autonomy and then discuss how the three external forces compromise the reputational autonomy of IR and, in turn, affect its intellectual output. To help flesh out my argument, I will sketch a partial comparison between American and Australian IR. Much like American IR, Australian IR is situated in an increasingly corporatized system of higher education but, unlike its American counterpart, it does not appear to be supported by significant sums of defense dollars, nor is it enmeshed in a behaviorally oriented political science. These contrasts help explain why in the United States paradigmatic research is being supplanted by atheoretical “hypothesis-testing,” whereas in Australia, though grand theory is in retreat, theorists are thriving.

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As I noted, Whitley (2000) elaborated three contextual factors that affect levels of task uncertainty and mutual dependence. One is particularly relevant: the degree of a field’s “reputational autonomy from competing intellectual organizations and the wider social structure in setting standards” (Whitley 2000, 220). More specifically, Whitley identified three dimensions of reputational autonomy: (a) the degree to which a field is free from external influence over “performance standards”

(that is, what counts as competent research); (b) freedom from external influence over “significance standards” (that is, what counts as an important research question and/or appropriate analytical strategy); (c) freedom to determine the field’s boundaries—to determine what problems are appropriate for members to work on and differentiate the field’s analytical vocabulary from everyday language. Naturally, the more external actors (administrators, funding agencies, other intellectual fields, laypersons, etc.) influence the assessment of a field’s domain or standards, the less the reputations of its members depend on one another’s judgment.

I argue that in recent years two sets of external actors have encroached upon IR’s reputational autonomy. One is a nexus of corporate interests, state government officials, elected politicians, and university administrators united by a quest to model American higher education after the business corporation. And the second consists of national security officials, epitomized by former Secretary of Defense Robert Gates, eager to align the research interests of social scientists more closely with US national security interests. The emergence of these actors and the pressures/incentives they create help explain why IR scholars have become less dependent on each other for reputations and thus why the content of IR journals has become characterized by “increasing fragmentation . . . isolated bits and pieces” (Cohen 2010, 889). However, to fully understand why the fragmented knowledge produced by IR takes the form of empirical hypothesis-testing, as opposed to, say, fragmented or highly pluralistic theorizing, we must consider a third external actor that exerts considerable influence over IR’s performance and significance standards: American political science. As the influence of theoretical “oligarchs” has waned, the importance of paradigmatic relevance and theoretical sophistication as performance standards has declined correspondingly and the “positivist” methodological canons of political science have become by default the primary standards for assessing IR research.¹¹

The Corporatization of Higher Education

The first external trend that erodes IR’s reputational autonomy is not unique to the field. In recent decades, American higher education has faced growing pressures to pattern university governance after the model of the business corporation. As Marvin Lazerson (2010) wrote, higher education’s tremendous growth in postwar America was accompanied by a rise in the influence of the professoriate: “The professoriate became the primary campus decision maker, as administrators routinely articulated the basic principle of academic life: The faculty is the heart of the institution.” By the end of the twentieth century, however, a new ethos had established itself—“the ethos of the market”—which engendered

an enormous shift in the balance of power in higher education. Institutional managers proliferated. Governing boards took on more power. The most influential board members came from the world of business because it was assumed they understood economic markets. Students became more important than before because their ever-increasing tuition payments balanced budgets and their brains brought prestige. They expected to be treated well as customers and, more important, they increasingly assumed their degrees would be valuable in the labor market. (Lazerson 2010)

Faculty members, Lazerson (2010) added, have “lost power” and no longer make the “truly big decisions.” Concomitantly, academic disciplines and departments have lost power to university administrators and boards (Lazerson 2010).

¹¹I put positivism in quotation marks because, though it is in common usage in political science and IR, the methodological practices it is associated with do not quite correspond to positivism as understood by philosophers. As Jackson (2011) explained, *neo*-positivism is a more appropriate term for the methodological approach that dominates American political science and IR.

Armed with buzzwords such as productivity, accountability, rankings, and metrics, administrators, trustees, and state legislators and governors have been pressuring professors to simultaneously emphasize teaching (especially in ways that improve the institution's *US News and World Report* ranking), teach more students, chase grant money and, last but not least, raise the institution's prestige by doing more research (Jaschik 2009). One significant consequence of this productivity-oriented culture is that quantitative assessment of research has gained momentum at the expense of qualitative assessment. A vignette from a book by sociologist Gaye Tuchman (2009)—a participant-observer study of a flagship public university—nicely illustrates this development. When Tuchman asked a young professor why she deserved tenure, the professor

didn't tell me about the topics of her research; instead she listed the number of articles she had written, where they had been submitted and accepted, the reputation of the journals, the data sets she was constructing, and how many articles she could milk from each data set. The only number she forgot to supply was the impact ranking of each journal to which she had submitted an article. (Tuchman 2010)

It is not, of course, as if quantity did not matter previously. "In the 1970s and 1980s," Tuchman recalled, "my friends and I also talked about how much we had written and where it had appeared, but we discussed why our work was important, too" (Tuchman 2010). Translated into Whitley's vocabulary, Tuchman's observation implies that the professoriate's "reputational autonomy"—its control over performance and significance standards—and, concomitantly, the degree of "mutual dependence," have declined. Professors used to depend more on each other for assessing research: is it significant? Is it creative? Is it original? The answers call for peer judgment and cannot readily be found in standardized metrics. Under the corporate model of governance, however, administrators have been relying more on "objective" metrics and less on peer judgment to assess research. And, as this vignette suggests, rather than resist this trend, faculty members have often internalized it.

There is no reason to think that IR scholars have been less responsive than other academics to the growing quantification of research assessment. As Mearsheimer and Walt (2013, 447) pointed out:

the more universities rely on "objective" measures to evaluate scholars, the greater the incentive to adopt a research strategy that produces many publications in a short time. These incentives are apparent to today's hyper-professionalized graduate students, who worry that getting a job requires them to publish as soon and as often as possible. They are understandably drawn to simplistic hypothesis testing, which allows them to take a data set and start cranking out articles, either by varying the research questions slightly, employing a series of different models, or using new estimation techniques.

Additionally, the reputational autonomy of IR faculty has been reduced by market-oriented pressures or incentives created by journal publishers. Academic journal publishing is a big business and, to maximize revenue, publishers are eager to introduce and aggressively market new journals. It seems that almost any determined group of IR scholars can find a publisher willing to launch a journal devoted to its intellectual agenda, and who would then use journal bundling and other marketing techniques to generate revenue. IR journals have thus proliferated rapidly. Think of the many journals that did not exist when the "paradigm wars" were at their peak some 25 years ago, including *IT*, *International Political Sociology*, *Critical Studies on Security*, *Review of International Political Economy*, *Global Governance*, *Journal of Narrative Politics*, *International Feminist Journal of Politics*, and this very journal. As one of this essay's referees commented, "There are now so many different places where one can send a paper that the 'gatekeepers' at the

central journals have had their influence reduced by a simple quantitative phenomenon: if a piece does not get accepted at journal X, it can just be sent to journals Y, Z, A, B, etc. and it is bound” to be published somewhere.¹²

Combined with the growing quantification of research assessment discussed above, the result is, as the same referee added, that “If an IO article was once the golden ticket to tenure, the new order of things [is] that enough publications in enough places that show up on the Thomson/ISI ranking list is sufficient. Hence one need not genuflect in the direction of high theorists in order to get published.” Restated in Whitley’s language, journal proliferation has contributed to a reduction in strategic dependence in IR. Reduced dependence on “high theorists” in turn helps account for the declining production of research oriented toward the traditional paradigms associated with these theorists.

Economic considerations motivate publishers to not only launch new journals but also limit article length. Production cost is significantly shaped by the journal’s competitiveness (the more articles are submitted, the greater the cost of peer-reviewing them) and article length. Because article acceptance rates in the humanities and social sciences are far lower than in natural science or medical journals, and because, on average, humanities and social science articles are longer, “it costs more than three times as much to publish an article in a social science or humanities journal” (Howard 2009). To cut costs, publishers have naturally pressured professional associations and editorial boards to impose stricter limits on article size. And journal editors appear to have responded by increasingly refusing to review submissions that exceed the word limit, and reducing the limit itself. For example, *IO*’s current guidelines state that the journal “will not review a manuscript containing more than 14,000 words,” whereas the 1997 guidelines set the limit at 16,000.¹³ Firmer size limits discriminate against theoretical contributions, which tend to be discursive in style. By the same token, downward pressure on article length privileges the technical and abbreviated writing style characteristic of quantitative empirical articles.

In sum, with the corporatization of academic governance, administrators increasingly rely on metrics to monitor professors’ research productivity, reducing the importance of peer assessment of research. Market forces such as the proliferation of journals have also contributed to the erosion of mutual dependence, particularly on a small elite of gatekeepers. The decline in mutual dependence is associated, in turn, with a decline in centralized theoretical coordination.

But this decline did not have to result in a rise in quantitative research. The decentralization of IR knowledge could have taken other forms such as the growth of highly diverse *theoretical* scholarship, characterized by a proliferation of theoretical perspectives and narrow theoretical specializations that thrive alongside each other. Indeed, only some of the socioeconomic forces discussed above favor quantitative work. Other market forces “incentivize” IR scholars to maximize *any* research publications. For example, while Mearsheimer and Walt (2013, 447) may be right that the growing use of metrics motivates young scholars to pursue data-based projects (which can easily yield multiple publications), theoretically inclined scholars, too, seem to have developed strategies to multiply publications; and it is not as if the administrators who use the metrics care whether the output is empirical or theoretical, so long as it is copious. Similarly, the proliferation of new journals enables the increased publication of empirical and theoretical research alike. Indeed, many new journals are friendly to theoretical research.

¹²The comments provided by the two anonymous referees were insightful and helpful. I hope they forgive me for quoting from their commentaries without their permission, which I was unable to request for obvious reasons.

¹³The current guidelines are available at “Guidelines for Contributors,” *International Organization*, accessed July 30, 2015, http://journals.cambridge.org/images/fileUpload/documents/INO_ifc.pdf. The 1997 guidelines were included in the front matter of the first issue of volume 51.

That the corporatization of the academy cannot adequately explain the rising importance of hypothesis-testing (even as it helps explain the growing fragmentation of IR knowledge) can be seen more clearly by comparing the United States to Australia. In Australia, much like in the United States, the idea has taken hold that universities should be run like businesses. As Australian National University (ANU) legal scholar Margaret Thornton (2012, 16–17) explains, “At the instigation of the government, universities began to be corporatized . . . This includes not just the commodification of education but a change in the style of governance from collegiality and consultation to top-down managerialism.” The augmentation of the power of managers correlated with a “decline in that . . . of the professoriate, the traditional scholarly elite” (Thornton 2012, 17).

And yet, even as the Australian and US higher education systems have been buffeted by similar corporatizing pressures, Australian IR, unlike its American counterpart, is “awash in theorists” (to quote the other referee of this essay). At the University of Queensland’s School of Political Science and International Studies, ten of the fourteen IR faculty members list IR theory as their first research expertise and at least one additional member, Ian Clark, appears to be primarily a theorist even as he does not state it explicitly.¹⁴ At other leading IR programs in Australia, the number and proportion of theoretically oriented scholars is also significant, if not as high as Queensland: five of eleven IR scholars at both Griffith University’s School of Government and International Relations and ANU’s School of Politics and International Relations; four of nine at Monash University’s Politics and International Relations Program; and five or six of nineteen at the University of Sydney’s Department of Government and International Relations.¹⁵ According to Colin Wight (2015) of the University of Sydney, “There is no doubt that Australian IR is more open to theory than the US.”

It is important, however, not to conflate the proliferation of theorists with the dominance of grand theory. As Wight (2015) further observes, in Australia, “the requirements to get external funding, generate high citation counts, etc., have all led to a retreat from Grand Theory.” He adds that Australian IR theorizing has “no theoretical focus/center and it’s very fragmented” (Wight 2015). Jacqui True (2015) of Monash University, while stressing the “sense of unity and identity” shared by Australian IR theorists, did not characterize this unity in terms of a theoretical consensus or center either so much as in terms of “strong value placed on diversity and richness of perspectives and acknowledgment that there are different debates we are all addressing.”¹⁶ These comments echo a shrewd observation offered by the second referee. The “proliferation of IR theory around the world,” s/he noted, “has led to a fracturing of theory (IR and Walzer, IR and Bourdieu, IR and Foucault, etc.) that has made *all* theoretical traditions less dominant.”

¹⁴See “School of Political Science & International Studies,” University of Queensland, Australia, accessed July 15, 2015, <http://www.polsis.uq.edu.au/>. While the Queensland website was highly informative and up to date, websites at other Australian schools were often of lesser quality. When faculty research interests were not explicitly stated, I did my best to classify them based on their short bios (when available) and publication profiles. Throughout, I counted only regular faculty members, excluding visiting/research fellows, honorary appointments, and adjunct lecturers.

¹⁵Information about Monash University was provided by True (2015). For the other universities, I gleaned this information from the following websites: “School of Government and International Relations,” Griffith University, accessed July 15, 2015, <https://www.griffith.edu.au/business-government/griffith-business-school/departments/school-government-international-relations>; “School of Politics & International Relations,” Australian National University, accessed July 15, 2015, <http://politicsir.cass.anu.edu.au/>; And “Department of Government and International Relations,” University of Sydney, accessed July 15, 2015, http://sydney.edu.au/arts/government_international_relations/staff/index.shtml.

¹⁶With the caveat that it is a small sample, note the diversity of the six contributions by Australian scholars to volume 6 (2014) of *IT*. Two were part of a forum seeking to “theorize international politics from a postcolonial perspective, understood not as a unified body of thought or a new ‘ism’ for IR, but as a ‘situated perspective’” (Epstein 2014, 294). A third contributed to “critical international theory” through engagement with the history of political thought. And the three other contributions were part of a forum on “emotions and world politics.”

While at the global level this fracturing may partly (as the referee suggests) reflect the multiplicity of national intellectual traditions, the Australian case suggests that IR theorizing may be characterized by “diversity and richness of perspectives” even within a single nation.

In sum, in Australia, much like in the United States, the erosion of IR’s reputational autonomy by corporate and market forces, and attendant erosion of “strategic dependence”, correlate with a “retreat from grand theory” and the production of fragmented knowledge. But the fact that in Australia this fragmentation has taken a largely theoretical form suggests that the corporatization of higher education alone cannot account for the empirical and often quantitative turn that has characterized the retreat from grand theory in American IR. To comprehend this phenomenon, we need to discuss two other external factors that impinge far more on American IR than its Australian counterpart.

Increased Research Funding by US National Security Agencies

The second external development that has impinged on American IR’s reputational autonomy in recent years is a substantial increase in funding from the national security agencies of the US government. Inasmuch as the funders set research priorities that diverge from IR’s internally generated agenda, and IR scholars are naturally attracted to the new riches, this is tantamount to an external intervention in the setting of IR’s significance standards and intellectual domain. And, inasmuch as the new funding is distributed without fully adhering to customary peer review practices, this development decreases the mutual dependence of IR scholars.

From the 1950s to 1970s, the Department of Defense (DOD) and other national security agencies generously funded social science research that was expected to aid the government in prosecuting the Cold War (see Oren 2003, ch. 4). The magnitude of this funding paled in comparison to DOD funding of natural science and engineering research, but it was substantial by social science standards. Some of the government’s largesse was directed toward systematically gathering and coding data on international political events.¹⁷ Government funding thus helped create the infrastructure necessary for the quantitative study of world politics.

However, “after a relatively short period of early spring in the 1960s and early 1970s, behavioral approaches retreated to the background of the [IR] field” (Maoz 2010). By the early 1990s, “quantitative research . . . had trouble seeing the light of day” (Kadera and Zinnes 2010). In significant part, this retreat was caused by the drying up in the 1980s of DOD support for social science research, including IR (Lambert 1989, 99). By 1990, the development of international event data was “essentially dead” after the DOD ceased funding these projects (Schrodt 2012, 564). Notwithstanding some exceptions, in the 1990s financial support from defense-related agencies remained insignificant. The dearth of DOD funding in the 1980s and 1990s slowed quantitative IR’s momentum considerably and thus helped tilt the professional playing field toward its main intellectual rival: grand theory. The point is not that Waltz, Keohane, Gilpin, or Ruggie would not have developed their theoretical ideas had the government continued to generously fund quantitative IR. The point is rather that the collapse of government funding greatly reduced the appeal of data-based projects if for no other reason than that it nearly eliminated their ability to support graduate students and young researchers. By default, orienting one’s research to the ideas elaborated by the

¹⁷Between 1967 and 1973, the Defense Advanced Research Project Agency spent approximately \$1 million on event data projects (Schrodt 1995).

grand theorists became a more attractive career path for upwardly mobile, reputation-seeking young scholars in the 1980s and 1990s.

Things started changing after the terrorist attacks of September 11, 2001, declaration of a war on terror, and invasions of Afghanistan and Iraq. The federal government began pouring money into counterterrorism and counterinsurgency research, with the newly established Department of Homeland Security (DHS) leading the way (Sageman 2013). By 2008, the DHS Science and Technology Directorate's budget exceeded half a billion dollars, of which \$14.2 million was allocated to applied social and behavioral research. Much of this money was channeled through two academic Centers of Excellence: the National Consortium for the Study of Terrorism and Responses to Terrorism (START) at the University of Maryland and the National Center for the Risk and Economic Analysis of Terrorism Events (CREATE) at the University of Southern California (AAAS 2009). Both made substantial grants to IR scholars, sometimes in collaboration with scholars from cognate fields.

START's beneficiaries included University of Maryland scholars Paul Huth, Jonathan Wilkenfeld, and Marc Lichbach, as well as scholars based elsewhere. For example, Erica Chenoweth from the University of Denver received \$140,000 to study bargaining with terrorists (2008–2011) and then \$150,000 for a project titled "Countering Terrorism in the United States" (2011–2015).¹⁸ Other beneficiaries include Victor Asal (University at Albany), Martha Crenshaw (Stanford), and Risa Brooks (Marquette).¹⁹ CREATE funds went to investigators such as Robert Powell (UC-Berkeley), David Laitin (Stanford), Daniel Byman (Georgetown), Victor Asal, and Jacob Shapiro (Princeton). Shapiro was the Principal Investigator (PI) on a \$253,000 grant to develop "Models of Counterterrorism" and a Co-PI on a \$103,000 project on political violence in Pakistan.²⁰

Though substantial by social science standards, the DHS research grants came to be dwarfed by DOD grants. After Robert Pape of the University of Chicago published preliminary empirical findings on suicide terrorism (Pape 2003), he was showered with research funds from two DOD agencies: the Defense Threat Reduction Agency and the Office of Naval Research. These funds allowed Pape to found the Chicago Project on Security and Terrorism, which went on to develop a terrorism database and support more than three dozen faculty fellows, research fellows, research associates, external advisors, and language consultants.²¹ Another DOD agency—the Defense Advanced Research Project Agency—broke in 2007 the long drought in government support of data development. It "invested substantial resources" in the creation of an "Integrated Conflict Early Warning System" (Schrodt 2012, 547, 551).

In 2008, the DOD ratcheted up its commitment to funding IR research with the launching of the Minerva Initiative by then-Secretary of Defense Robert Gates. Gates sought to open a new page in the sometimes hostile relationship between social scientists and the defense establishment, inviting universities to apply for grants to study policy-relevant issues such as the Chinese military and counterinsurgency (Goldstein 2008). Although the DOD insists it has a

¹⁸"CV & Press Kit," Erica Chenoweth, accessed May 13, 2016, <http://www.ericachenoweth.com/cv/>.

¹⁹"CV," Risa Brooks, Marquette University, accessed May 25, 2013, <http://www.marquette.edu/polisci/documents/BrooksCV.pdf>. The other START beneficiaries appear on the Center's website, "Start Research Projects," National Consortium for the Study of Terrorism and Responses to Terrorism, accessed May 13, 2016, <http://www.start.umd.edu/projects>.

²⁰"Curriculum Vitae," Jacob Shapiro, Princeton University, accessed May 13, 2016, http://scholar.princeton.edu/sites/default/files/jns/files/jacob_shapiro_cv_dec_15.pdf. For the other names, see "Research," CREATE, accessed May 13, 2016, <http://create.usc.edu/research/>.

²¹See "Chicago Project on Security & Terrorism (CPOST)," University of Chicago, accessed May 26, 2013, <http://cpost.uchicago.edu/index.php>. Over time, the Carnegie Corporation of New York replaced the DOD as the project's principal funder.

“well-tested” in-house peer review process (Weinberger 2008, 577), and although this process involves input from “DOD-external academic researchers,” it appears that the influence of academic researchers on funding decisions is more than offset by that of “research program managers in the Army, Air Force, and Navy” and “defense policy and operations experts.”²² As leaders of social science associations have complained, the DOD selection process “undermines the practices of peer review that play such a vital role in maintaining the integrity of research in social-science disciplines” (Goldstein 2008). Translated into Whitley’s language, the newfound availability of DOD funds compromises the “reputational autonomy” of the social sciences and, concomitantly, the degree of “mutual dependence” within them.

Minerva was budgeted approximately \$75 million over five years, and in 2014 alone Congress allocated \$17.8 million to the program (Ahmed 2014). As *Science* magazine observed, “Minerva is a banquet for a field accustomed to living on scraps”—the magnitude of the grants “is on a scale most social scientists have only dreamed about” (Mervis 2009, 576–77). A significant portion of these riches went to IR—IR scholars headed three of the seven research teams who won Minerva awards in the program’s first round.²³ James Lindsay of the University of Texas was the PI on a \$7.6 million project on “Climate Change, State Stability, and Political Risk in Africa.” Nazli Choucri of MIT headed a team that received more than \$10 million to study international cyber-relations. And Jacob Shapiro was awarded \$9.5 million for a project titled “Terrorism, Governance and Development” (Mervis 2009, 577).²⁴ Shapiro’s team included James Fearon, David Laitin, and Jeremy Weinstein from Stanford, Jason Lyall from Yale, and Eli Berman from UC-San Diego (Patel 2010). As Berman explained, grants of this magnitude constitute game changers: “Instead of just a summer salary and a graduate student, I’ll be able to do surveys and experiments around the world, partner with additional organizations, and bring on postdocs as well as several graduate students . . . We’ll be able to accomplish things in a matter of years rather than decades” (quoted by Mervis 2009, 577).

Research teams headed or dominated by IR scholars continue to do well in Minerva’s grant competitions. Of the eleven funded projects announced in June 2015, American IR scholars head six.²⁵ For example, Tony Rivera of Duke University won a major grant to develop a “computational model of resources and resiliency”; Robert Pape won \$3.2 million to “analyze the brain activity of suicide terrorists as they watch recruitment videos produced by groups like ISIS”;²⁶ and a team led by Paul Hensel of the University of North Texas was awarded \$670,000 to expand the “Issue Correlates of War data set.”²⁷

²²See “How are awards selected?,” Frequently Asked Questions, Minerva Initiative, accessed August 6, 2015, <http://minerva.dtic.mil/faqs.html>.

²³The seven grants are listed in an attachment to a DOD news release. US Department of Defense, 2008, “First Minerva Research Initiative Awards Announced,” December 22, accessed May 26, 2013, <http://www.defense.gov/releases/release.aspx?releaseid=12407>.

²⁴The amount of Shapiro’s grant is listed on his CV (see fn. 20).

²⁵Brief descriptions of the winning projects, including the title, PI, and abstract, but not the dollar amounts, are posted at “Announcing the Projects Selected for 2015 Minerva Research Award,” University-Led Research, Minerva Initiative, accessed July 20, 2015, <http://minerva.dtic.mil/funded.html>. In addition to the three examples provided below, the IR scholars who head funded projects include Charles Glaser of the George Washington University, Zeev Maoz of UC-Davis, and Mariya Omelicheva of the University of Kansas.

²⁶“Social and Neurological Construction Martyrdom Project Receives \$3.2M Funding,” University of Chicago, accessed July 20, 2015, <https://socialsciences.uchicago.edu/announcement/social-and-neurological-construction-martyrdom-project-receives-32m-funding>.

²⁷“UNT’s Dr. Paul Hensel Acquires Prestigious Department Defense Grant,” UNT Castleberry Peace Institute, accessed July 20, 2015, <http://castleberry.unt.edu/news/unts-dr-paul-hensel-acquires-prestigious-department-defense-grant>.

IR's supremacy was even greater in 2014, when American IR scholars led or dominated nine of the twelve funded projects.²⁸ For example, a multi-university team led by Paul Huth was awarded \$2.5 million to collect and analyze data on the conditions under which foreign aid reduces (or exacerbates) armed conflict between developing countries. As many as 100 students at the University of Maryland alone are involved, including undergraduates who contribute to data development and graduate students and research fellows who participate in data analysis (Hassanein 2014). Similarly, the \$1.9 million "Complex Emergencies and Political Stability in Asia" project, headed by Joshua Busby, lists eleven research assistants affiliated with Busby's home institution, the University of Texas at Austin.²⁹ Another 2014 Minerva grant (\$1.5 million) went to a team headed by Robert Jervis that undertakes, in the words of co-investigator Gregory Mitrovich, "a comparative study of how the US and China have used soft power to aid their rise to great-power status." The project pairs qualitative case studies with "a very intensive quantitative study ... which will use computer-aided text analysis, or CATA" to sift through "millions and millions of pages of text." As Mitrovich added, the project will rapidly yield an abundance of publications: "It's a three-year timeframe. Victoria [Hui, University of Notre Dame] will have one book, I'll have two books, and we'll have articles related to CATA analysis that we want to publish in academic journals. Then we want policy analysis in policy journals, briefing papers for DOD, the State Department, the CIA."³⁰

As some of these examples illustrate, this massive surge in research funding has not only benefited lead investigators, but has also trickled down to scores of doctoral students, research associates, and postdoctoral fellows. One can reasonably assume that, without the opportunities created by this financial surge, at least some of these budding scholars would have oriented their research, much like their predecessors in the 1980s and 1990s, to paradigmatic debates as much as to the DOD's narrower practical concerns. These enormous grants are also helping enterprising young scholars such as Shapiro, Lyall, and Chenoweth efficiently produce large quantities of publications.³¹ Operating in a corporate-style academic culture that increasingly rewards measurable productivity, and situated in a field whose "oligarchs" have been aging, such young scholars have been able to rapidly build sizable reputations even as the shelf life of their publications might turn out to be as transitory as the policy concerns that these publications address. Unlike their counterparts two or three decades ago, who were highly dependent on the field's "oligarchs," the reputations of young scholars today can be greatly assisted, if by no means exclusively determined, by extra-academic actors such as DOD program managers and policy experts.

Although recipients of Minerva and other government grants enjoy full academic freedom (Mervis 2009, 576), their research topics were shaped by the US government rather than an intellectual dynamic internal to IR. And because the government's favored research problems closely track foreign policy concerns, such funding favors empirical explorations of practical, policy-relevant questions at the expense of abstract theoretical research. Furthermore, although in principle the DOD appears open to qualitative research, the vast majority of funded IR

²⁸"Announcing the Projects Selected for 2015 Minerva Research Award," University-Led Research, Minerva Initiative, accessed July 20, 2015, <http://minerva.dtic.mil/funded.html>.

²⁹"Research Assistants," Robert S. Strauss Center, accessed August 6, 2015, <https://www.strausscenter.org/cepsa-research-assistants>. Busby was a lead researcher in the above-mentioned \$7.6 million Minerva project, headed by James Lindsay, on "Climate Change, State Stability, and Political Risk in Africa," accessed August 6, 2015, "Joshua Busby," Lyndon B. Johnson School of Public Affairs, <https://www.utexas.edu/lbj/directory/faculty/joshua-busby>.

³⁰Q&A: Gregory Mitrovich," 2014, Columbia School of International and Public Affairs, June 23, <https://sipa.columbia.edu/news-center/article/qa-gregory-mitrovich>.

³¹In addition to the aforementioned DHS grants, Erica Chenoweth is PI on a \$468,000 Minerva grant (2012–2015) to study terrorist networks (see Chenoweth CV, fn. 18).

projects are in significant part (and often exclusively) mathematical and/or statistical, with many involving gathering, coding, and analyzing data for the purpose of testing policy-relevant hypotheses. Only one of the six IR projects whose selection Minerva announced in June 2015 (led by Charles Glaser) appears to be exclusively qualitative. It can be said, then, that the vast increase in funding of IR research by US national security agencies since 2001 has contributed significantly not only to the eroding centrality of theory in the field but also to the rising incidence of empirical “hypothesis-testing” research. Ironically, this financial support favors the production of precisely the highly technical IR scholarship that top national security officials then criticize for being irrelevant to policymakers (Avey and Desch 2014).

How does Australian IR compare on this dimension? I found no evidence that the Department of Defense in Canberra or other Australian national security agencies sponsor any public programs analogous to the DOD’s Minerva Initiative or academic centers of excellence created by the DHS. Colin Wight (2015) “is not aware of those kinds of agencies funding research in Australia. They may do but it’s not common knowledge, and nobody I know at either Sydney or the University of Queensland ever admitted to me they get any such funding.”³² With the caveat that a fuller investigation is beyond this essay’s scope (and is best left to an Australian scholar), it appears that Australian IR’s reputational autonomy has not been affected by defense and foreign policy actors to anywhere near the same degree as American IR.

It would be rash, however, to conclude the comparison without acknowledging that Australian IR does enjoy substantial support from the government, if not specifically its national security apparatus. In 2001, the Australian Parliament created the Australian Research Council (ARC), which currently reports to the Minister for Education and Training, and distributes on average 800 million Australian dollars (AUD) annually in competitive research grants (approximately \$600 million at the August 2015 exchange rate). A small portion goes to IR research. During the ARC’s first five-year cycle, 2002–2006, it funded forty-four projects classified (exclusively or in conjunction with cognate fields) as IR, totaling about 10.7 million AUD. During 2010–2014, ARC funding of IR research increased to sixty-three projects totaling approximately 15.9 million AUD.³³ Given the modest number of IR scholars in Australia, the magnitude of ARC support is significant.³⁴

To qualify for ARC funding, research proposals are expected to address issues of concern to Australian society.³⁵ An inspection of funded IR project summaries indeed suggests that they tend to be framed as relevant to Australian foreign relations or international organizations/initiatives in which Australia plays a significant role.³⁶ Inasmuch as ARC support thusly shapes the range of problems addressed by Australian IR scholars, it by definition constrains the field’s reputational autonomy.

Still, it appears that the ARC impinges on Australian IR’s autonomy to a far lesser degree than DOD funding impinges on American IR. Whereas Minerva funding decisions are crucially influenced by DOD experts and military program managers, “the majority of [ARC] funding decisions . . . are made on the basis of peer review” processes undertaken “by experts who assess individual research

³²Wight (2015) added that, if such non-public government funding existed, it was more likely to go to ANU or the University of New South Wales, both located in Canberra.

³³Australian Research Council, accessed August 10, 2015, <http://www.arc.gov.au/>. I calculated the funding data by searching the website’s grant database.

³⁴Cox and Nossal (2009, 290) identified 106 IR scholars in Australia. The designers of the 2012 TRIP survey identified 280 (Morgenbesser 2013, 219).

³⁵Sharman and True (2011, 162) note that the ARC has “placed increased emphasis on the need to show demonstrable national benefits or relevance in grant applications.”

³⁶Project summaries are accessible through the grant database on the ARC’s website.

proposals within their field of research or across a broader disciplinary area on the basis of established selection criteria.”³⁷ This resembles the peer review process maintained by the US National Endowment for the Humanities or National Science Foundation far more than the Pentagon’s in-house review process. Whereas leaders of US social science associations have vocally complained about Minerva’s inadequate peer review, I found no evidence that Australia’s social science associations have publicly criticized the ARC’s peer review process.

Given that Australian IR scholars play a key role in assessing the proposals submitted to the ARC by their peers, it should not be surprising that funded projects mirror to a significant degree the intellectual and methodological orientations prevalent in the field. Strikingly, in contrast with the empirical and technical bent of Pentagon-funded research, the ARC has been receptive to theoretical work. Theoretically inclined scholars such as Christian Reus-Smit, Jacqui True, and Anne Orford won more than one ARC award as lead or co-investigators. Of the seven IR projects funded in the ARC’s first year (2002), three explicitly described theory development as their objective. For example, Michael Wesley of the University of New South Wales was awarded 24,000 AUD to “develop a comprehensive theoretical account of the State as a conceptual necessity in international relations.” Similarly, three of the nine projects selected in 2015 display (judging from their brief summaries) a theoretical orientation, including a 2.5 million AUD project (larger than the other eight projects combined) led by Orford, to “develop new legal concepts to make sense of the responsibilities of external actors in civil war.”

Moreover, although many of the ARC-funded IR projects are more empirical than theoretical, a majority of the empirical projects appear to be qualitative, historical, or interpretive. While the incidence of quantitative projects has increased in recent years (suggesting that perhaps the mounting corporatizing pressures discussed above are finally beginning to combine with ARC-induced pressures to nudge Australian IR toward a more “Americanized” form), they are still outnumbered and out-funded by theoretical and/or qualitative projects—reflecting the methodological profile of Australian IR research as a whole. This contrasts sharply with the situation in the United States, where the DOD’s “in-house” review process heavily favors quantitative research even as two-thirds of American IR scholars describe their work as primarily qualitative (Maliniak *et al.* 2011, 453).

In sum, while both Australian and American IR have enjoyed a surge in governmental support since 2001, this surge has compromised American IR’s reputational autonomy to a greater degree because the funding originated from national security agencies that adhere to peer review practices only to a limited extent. It helped make it possible for enterprising American scholars to make reputational gains without depending on the profession’s “oligarchs” as much as their predecessors did, and thus without having to orient their research to the oligarchs’ favored theories. Moreover, while explaining this pattern is beyond the scope of this essay, DOD funders appear to exhibit a strong preference for empirical over theoretical IR research and quantitative-technical over qualitative approaches. This begins to explain why the void created by the decline of grand theory in American IR is largely filled by “hypothesis-testing” research. To provide a more complete explanation, however, we must consider another factor that differentiates American IR from its Australian counterpart: its relationship with political science.

IR’s Enduring Embeddedness in Political Science

As Andrew Abbott (2001, 122–23) pointed out, between 1890 and 1910 a unique social structure emerged in the American academy: a system of distinct disciplines

³⁷“Peer Review,” Australian Research Council, accessed August 10, 2015, <http://www.arc.gov.au/peer-review>.

that are simultaneously institutionalized nationally—with professional associations and a national labor market—and in individual universities, with distinct departments and undergraduate majors corresponding to each discipline. This disciplinary system has been remarkably durable. Few if any disciplines have perished, and hardly any have been born over the past century.

When the disciplinary map of the social sciences was drawn, IR's subject matter was placed under the "jurisdiction" of political science, where it remains (see Schmidt 1998, ch. 5). Although IR scholars later established a strong professional society, the ISA, IR still lacks key defining features of a discipline: distinct departments and a job market. The vast majority of American IR scholars received their doctoral training from political science programs and are employed within political science departments (Biersteker 2009, 310; Jordan *et al.* 2009).³⁸ When freshly minted PhDs in IR go on the job market, they turn to the APSA, not the ISA.³⁹

The scope of IR as an intellectual field in the United States is thus not coterminous with the work unit within which most IR scholars are employed. IR is but one of several major subfields comprising the discipline of political science, and IR professors almost never constitute a majority within their departments. For example, at Harvard's Department of Government, the profession's top-ranked graduate program, eight of the forty-five regular faculty members (18 percent) are IR scholars. At the top-ranked political science program at a public university, the University of Michigan (ranked fourth overall), ten of the forty-six faculty members (22 percent) are affiliated with the IR subfield; and at my own department at the University of Florida, IR scholars comprise 20 percent of the faculty (seven of thirty-five).⁴⁰ Therefore, the hiring, firing, promotion, salaries, and reputations of IR faculty in the United States depend significantly on judgments of colleagues from other subfields.

If there is an area that dominates the discipline, it is not IR but American Politics. Americanists in leading political science departments almost always outnumber IR scholars, and often comprise the largest group, if not an absolute majority, among the faculty. At Harvard, 30 percent of the faculty study American politics; at Michigan, 41 percent; and at Florida 48 percent.⁴¹ Furthermore, Americanists exercise immense influence over the leading general political science journals. Two of the discipline's three top journals, the *American Journal of Political Science* and the *Journal of Politics*, are nearly exclusively edited (and their editorial boards dominated) by Americanists. Equally important, more than any other group, the Americanists set the intellectual tone within political science. It is not that they try to shape the research problems addressed by IR (or other subfields), but they do have strong views regarding *how* these problems should be approached and analyzed. In Whitley's (2000, 220) terms, Americanists threaten IR's "reputational autonomy" by setting "performance standards" (what counts as competent research) and "significance standards" (what counts as an appropriate analytical strategy) to which IR scholars are expected to conform. The standards

³⁸The 2008 TRIP survey asked respondents to name their primary and secondary subfields "within politics or political science" (Jordan *et al.* 2009, 29). It did not question respondents about their subfield affiliation within any other discipline.

³⁹Whereas the APSA sponsors a comprehensive jobs database and a placement interview service at its annual meeting, the ISA offers a rudimentary jobs bulletin board and no placement service. Compare "eJobs," American Political Science Association, accessed January 25, 2016, <http://www.apsanet.org/eJobs>; and "ISA Job Board," International Studies Association, accessed January 25, 2016, <http://www.isanet.org/Programs/Job-Board>.

⁴⁰See "Faculty," Department of Government, Harvard University, accessed May 13, 2016, <http://gov.harvard.edu/people/faculty>; "Faculty," Department of Political Science, University of Michigan, accessed December 24, 2015, <https://www.lsa.umich.edu/polisci/people/faculty>; "Faculty (by Field)," Department of Political Science, University of Florida, accessed December 24, 2015, <http://polisci.ufl.edu/people/faculty-by-field/>. When subfield affiliations were not clearly stated, I inferred them from short bios and CVs. Rankings are from "Political Science," *US News and World Report*, accessed December 24, 2015, <http://grad-schools.usnews.rankingsandreviews.com/best-graduate-schools/top-humanities-schools/political-science-rankings>.

⁴¹See fn. 40.

that dominate American politics again revolve around “hypothesis testing” and quantitative methods. It is extremely hard to make a career, let alone become a star, in American politics by doing research that diverges from this neopositivist, “large-*N*” orthodoxy.

Even when IR’s theoretical “oligarchs” were still at the height of their professional influence, and even as they surely have taught their students to ground their analyses in grand-theoretical ideas, these students were also being exposed by other political science professors to the logic of hypothesis testing. And this logic began to find expression in their research even as that research remained firmly rooted in their teachers’ paradigms. Contrast, for example, Kenneth Waltz and his student Stephen Walt. Waltz, as Jackson (2011, 212–13) explains, construed his *Theory of International Politics* (1979, 8) not as a source of hypotheses to be tested against independently existing reality, but rather as a “picture, mentally formed, of a bounded realm,” that is, a mental construction of a reality. Walt’s dissertation-turned-book, *The Origins of Alliances* (1987), however, though firmly grounded in the realist paradigm, employed theory as a source of hypotheses that were then subjected to qualitative empirical testing.

If for Walt’s generation the pull of empirical hypothesis-testing standards was balanced by the appeal of the paradigms elaborated by their teachers, for subsequent generations, who entered the field when these teachers had retired or neared retirement, the narrow methodological standards transmitted from American politics were no longer balanced by a strong demand for theoretical breadth and originality. For them, once the lure of the paradigms had diminished, the default intellectual position became the rather atheoretical and quantitative mode of analysis that is the gold standard in American political science, especially in American politics.

In sum, IR’s embeddedness in political science, which is dominated by a neopositivist and quantitative orthodoxy emanating from the field of American politics, is an important reason why the void created by the decline of paradigmatic research is being filled by largely atheoretical and increasingly quantitative hypothesis-testing research. This can be seen more clearly by comparing American IR to its Australian counterpart.

As Cox and Nossal (2009, 298–99) point out, Australian universities have “de-departmentalized” their teaching, combining multiple “areas” into larger units, usually “schools.” Under this structure, IR scholars and political scientists typically work together within units such as the “School of Political Science and International Studies” (Queensland), “School of Politics and International Relations” (ANU), or “School of Government and International Relations” (Griffith). The inclusion of IR in these titles—which contrasts with the United States, where IR scholars typically belong to a “Department of Political Science”—is telling. In Australian schools of politics and international relations, IR scholars tend to form the largest faculty group and their proportion within their units is significantly larger than the share of IR specialists within US departments of political science. At Queensland, fourteen of the twenty-eight faculty members are associated with the IR “study area” and three more are associated with “peace and conflict studies.” At ANU and Sydney’s Department of Government and International Relations, IR specialists comprise 46 percent; at Griffith, 44 percent; and at Monash University’s Politics and International Relations Program, more than 50 percent.⁴² These relative numbers reflect the popularity of IR among students. Jacqui True (2015) reports that “IR brings in the most students, so it is seeing the most hires—universities are highly corporatized and they know where the dollars come from.”

Because of its relative numerical strength and the bureaucratic clout that Australian IR enjoys thanks to its attractiveness to students, it enjoys greater

⁴²See fn. 14 and 15.

autonomy vis-à-vis political science than its American counterpart. Colin Wight (2015) reports that “the IR people have a high degree of autonomy. If it was a major IR position, I or one (or more) of my IR colleagues would be on the appointments committee and our views may not take precedence, but they couldn’t be ignored either.” True (2015) goes further, noting that, at her program, appointment committees “defer to the [IR] field expert/professor.”

But even if the dependence of Australian IR scholars on their political science colleagues were as high as in America, this would not likely produce the same intellectual effect as in the United States. For unlike its American counterpart, Australian political science has not undergone a behavioral revolution and its intellectual output is largely qualitative and/or normative. As a 2009 study noted, the “entire history of the discipline in Australia” has been characterized by a strong orientation toward the “mother country” (the UK), whose political science has more in common with Europe and the British Commonwealth than American political science. “There appears to be a growing gap,” the authors concluded, “between a US-style political science, often positivist and quantitative, and a European approach that is certainly more qualitative. Australian political scientists belong mostly in the second group” (Sharman and Weller 2009, 610–11). Thus, Australian IR specialists do not face the same pressures to align their scholarship with the canons of neopositivist, quantitative social science. In a 2008 cross-national survey of IR scholars, only 30 percent of Australian respondents (and 33 percent of UK-based respondents) characterized their work as “positivist,” compared with 65 percent of American respondents. Furthermore, only 3 percent of Australian respondents use primarily quantitative methods, compared with 23 percent in the United States (Sharman and True 2011, 151). Like Australian political scientists, “Australian IR scholars generally exhibit a ‘British’ or ‘Commonwealth’ identity,” and their research does not exhibit “a tendency . . . to ape US epistemology and theoretical and methodological norms” (Sharman and True 2011, 157, 164).⁴³

In sum, Australian IR enjoys greater reputational autonomy from political science than its American counterpart does. And, to the extent that Australian political scientists do take some part, minor though it may be, in shaping the reputations and careers of IR scholars, they do not share the tendency exhibited by American political scientists to favor empirical and preferably quantitative research. This pattern helps explain why, in the United States, the decline of paradigmatic research is accompanied by an increase in “hypothesis-testing” research, whereas in Australia it is accompanied by a proliferation of diverse theoretical perspectives, with no appreciable increase in quantitative empirical research.

Conclusion

In this essay, I interpreted the eroding centrality of theoretical paradigms in American IR by drawing on the conceptual framework elaborated by sociologist Richard Whitley. In the 1980s and early 1990s, IR approximated the type of social organization Whitley labeled “polycentric oligarchy”—a hierarchic structure in which scholarly reputations are highly dependent on a small number of

⁴³One wonders, however, whether this relative insulation from US political science will withstand the continuing pressures on Australian academic programs to boost their international rankings. There are signs that Australia’s leading School of Politics and International Relations, at ANU, is moving in a more Americanized direction. The School lured a significant group of political scientists who pursue behavioral research, most of whom received their PhDs from US universities. For example, high-profile statistical methodologist and electoral scholar Simon Jackman joined ANU in July 2015. “New Staff at The School of Politics and International Relations,” School of Politics and International Relations, Australian National University, accessed August 19, 2015, <http://politicsir.cass.anu.edu.au/news/new-staff-school-politics-and-international-relations>. If this trend continues and spreads elsewhere, Australian political science may gradually become more Americanized and Australian IR may face growing pressures to produce more empirical and even quantitative research publishable in leading US-based journals.

reputational leaders associated with competing schools. This “strategic dependence” entailed incentives for scholars to situate their work within these schools and/or debates among them; this was thus conducive to grand theory becoming the intellectual center of the field. In recent years, however, IR has moved toward becoming a “fragmented adhocracy”. Its reputational hierarchy has become more fluid and ambiguous. Concomitantly, its intellectual output has become more fragmented, less coordinated by a theoretical core, and more quantitative. Three external factors have contributed to this development by reducing/limiting American IR’s “reputational autonomy” and, in turn, the dependence of IR scholars on each other for assessing reputations: the rise of the corporate mode of university governance; the availability of large-scale research funding from the Pentagon and other defense agencies; and the embeddedness of American IR in political science.

Of these three external forces, the first, the “marketization” of the American academy, shows few signs of abating. Likewise, the embeddedness of American IR in political science and hegemony of neopositivist methodology within that discipline are likely to endure well into the future. As to the abundance of DOD funding for IR research, it may well dwindle as the US defense budget faces downward pressures, fueled by the drive to cut the federal deficit. For now, however, it seems that the DOD remains strongly committed to supporting social science research. In fact, Minerva’s annual budget has increased since 2012 despite the imposition by Congress of automatic spending cuts on the federal government, including the DOD (see [Marshall 2012](#)). Given the persistence of these external forces, the erosion of American IR’s theoretical core and the concomitant rise of “hypothesis-testing” research are likely to continue.

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