

**Wars of Plunder: Conflicts, Profits and the Politics of Resources.** By Philippe le Billon. New York: Columbia University Press, 2012. 288p. \$30.00.

**Petro-Aggression: When Oil Causes War.** By Jeff D. Colgan. New York: Cambridge University Press, 2013. 324p. \$34.99.

**The Race for What's Left: The Global Scramble for the World's Last Resources.** By Michael T. Klare. New York: Picador, 2013. 320p. \$27.00 cloth, \$17.00 paper.  
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— Benjamin Smith, *University of Florida, Gainesville*

There is no longer much need to speak of a paradox of plenty or of the price of wealth. A generation of increasingly sophisticated scholarship on the resource curse has become conventional wisdom, with less pessimistic conclusions now on the offensive. The three new books under consideration here all focus on the conflict-inducing effects of natural resource wealth. As I discuss below, some variants of the resource curse approach have yet to catch up to the new state of the art in analyzing the resources-conflict nexus, especially in teasing out the conditional relationships. And, the empirics in these gloomier public-audience accounts often themselves suggest a much more modest role for resources in catalyzing conflict. The three books I review here run from mostly mass audience orientation (Klare) to largely scholarly and policy (Colgan) with a hybrid between them (le Billon). I order my discussion of them along that spectrum.

Michael Klare's *The Race for What's Left* begins with the gloomy claim that resource politics will be driven in the future by "pervasive, unprecedented resource scarcity" (p. 8). His claim relies largely on the observation that the increasing willingness of oil companies, and their host countries to pay ever higher extraction costs for more-difficult-to-get oil, illustrates what one reviewer of the book called "one last burst of profiteering [by] our corporate empires." Klare argues that land, oil, water, and most other crucial resources will be "largely exhausted in the not-too-distant future" (p. 12). This growing scarcity will be the source of both intrastate and interstate violence as governments are driven to protect their security and their populations.

This starting point is the cornerstone of the argument in the book; without it the argument has a much weaker foundation. What might be more empirically useful is to ask: Why are so many more resource areas under exploration today than a decade ago? This is the real research question at hand, but Klare takes his own answer to it as given rather than exploring it. It is appropriate to note here that Klare's first example—increasingly harder means of oil extraction—is a function of the last 14 years of consistently higher global prices. Those prices have spurred innovation in new technologies such as hydraulic fracturing ("fracking") and a willingness to exploit

previously known but unprofitable reserves. This take-away message—that today's new reserves are economically viable because prices are high—is much less eye-catching than a pervasive scarcity thesis. But it is the more accurate one and reflects not just the growing middle classes (and car drivers) in Brazil, China, India and elsewhere but also the growing wealth in the developing world.

Klare suggests that "virtually all accessible resource zones are still in production" (p. 16). However, we actually know now from recent research in development economics and political economy that in fact the world is likely to continue uncovering resource reserves for the indefinite future. The reason for the current bulge in exploration is not pervasive scarcity but a combination of better governance and investment climate in those countries and higher prices. Better conditions on the ground make for more willingness to commit the capital necessary to extract what lies beneath it. Higher prices mean that more reserves become profitable. Again, the point is not that we should encourage limitless extraction of carbon fuels or any other finite resources—quite the contrary. Rather, it is that we should understand the dynamics of resource wealth as accurately as we can and plan accordingly. Paul Collier (*The Plundered Planet*, 2010), among others, has compellingly shown that as more developing countries become stable and more prosperous, they are likely to attract increasingly effective resource exploration and extraction from foreign investors. As prices remain high, the benefit to cost ratio rises still more.

The remainder of the vignettes Klare selects to illustrate how pervasive scarcity is breeding conflict gloss over the relative unimportance of resource issues driving political conflict (e.g. in Zimbabwe and Georgia, pp. 220–21) or give scant attention to equally or more likely alternative explanations. For example, a brief discussion of the relative lack of military buildup by either Canada or Russia in the Arctic (as a result of cost) is deployed to make the broader point that the region is on the verge of conflict as a result of a resource rush. More likely, to my mind, neither state feels much of a security threat and subsequently feels little need to deploy large numbers of troops. In short, it is often difficult to figure out whether resource scarcity or abundance is supposed to be the more conflict-inducing force and how to know when resources, as opposed to something else, are at work in a given situation.

Since Klare's analytic lens takes nearly all resources valuable to humans—land, water, agriculture, sub-soil—as equivalent, it is more likely to be the case that ones like land and water, which are finite on the planet, might fit his forecast better. But, since the focus of most of the conflict anecdotes is on oil or minerals, it seems appropriate to assess the argument in light of those sectors, and here the evidence is ambiguous.

Philippe le Billon's *Wars of Plunder* takes as a starting point the large literature positing that resource wealth makes violent conflict more likely. To his credit, le Billon immediately notes how much variation there is across the resource-rich countries of the world—while some are highly stable, others are highly conflict prone. Le Billon's book is an ambitious theoretical effort to bridge multiple natural resource types (oil, timber, diamonds, and coffee—to take just four among others) and it offers a framework to hypothesize which kinds of resource configurations are most likely to trigger which kinds of conflicts. Building on a consideration of the vulnerabilities, risks, and opportunities that resource endowments create for rebels and rulers (pp. 24–29), he specifies four combinations of resource characteristics that make different conflicts—coups, secession, mass rebellion and warlordism—more or less likely (p. 29). The remainder of the book explores different constellations of resource qualities and cases intended to illuminate their conflict dynamics.

It is in the selection of cases and their linkage to this theory that some problems emerge. Rwanda, for example, is used as a case of coffee generating or exacerbating conflict due to the collapse of coffee prices in the late 1980s. This is the first mention I have seen of coffee playing a causal role in the Rwandan genocide, and generally the resource curse is rarely thought to include agricultural commodities (a major critique of the original Collier-Hoeffler resource curse model was that it lumped agricultural and sub-soil commodities). Many later examples seem chosen for reasons other than a strong focus on careful case selection and sometimes verge on anecdotal.

Another issue is the book's critique of econometric research and of the material motives thesis—the “greed” variant of the resource curse. Here le Billon is on solid ground, and even Collier and Hoeffler have walked their more recent work away from their earlier, cruder approach. Le Billon's efforts to distinguish his own approach by contextualizing resources in history and culture, though, are often difficult to hold together in a clear, book-wide message. Without doubt particular episodes of conflict in which natural resources have played a role contain their unique features, but in the narratives these features periodically lead the chapters away from the more systematic theory stated in the first chapter. As a result we sometimes lose track of which configuration any specific case is supposed to be demonstrating, leaving it much less easy to figure out the policy implications of a particular case.

Jeff D. Colgan's *Petro-Aggression* is by far the most clearly structured and argued of these three books. In it he tackles a type of violence not yet systematically analyzed in an otherwise flooded research program on oil and conflict. Colgan seeks to explore the origins of interstate aggression among oil-exporting countries and links that dynamic to

the emergence in the last half of the 20<sup>th</sup> century of a number of “petro-revolutionary” regimes like Qaddafi's in Libya, Saddam's in Iraq, and Khomeini's Islamic Republic of Iran. The basic argument of the book is that although oil creates incentives for both international system stasis and challenges by exporting countries, revolutionary regimes tend to amplify the aggression effects of petroleum wealth. This argument is a welcome addition to recent studies by Thad Dunning, (*Crude Democracy*, 2008) Michael Ross (*The Oil Curse*, 2012) and Benjamin Smith (*Hard Times in the Lands of Plenty*, 2007) and it pushes the boundaries of the research agenda in an innovative way.

Chapter 2 develops the argument, first building the case for why we would expect revolutionary leaders to create trouble in the international system, then why oil has both stabilizing and destabilizing effects, and finally why oil in the hands of revolutionary leaders tends to mute its stabilizing effects. The chapters that follow test various implications of the argument, first against an array of cross-sectional time-series data and then against a set of country-regime cases. I would note first that Colgan is one of a growing number of scholars who are laudably archiving replication data publicly very soon after the publication of the books and articles based on those data. This is a real service both to scholarship and to the goal of policy-relevant research that is grounded in careful empirical inquiry, and it represents a valuable contribution to the study of the politics of resource wealth.

In any large-scale data collection/coding project, there are tough choices to make, all the more when no one has previously made them. Drawing on the Archigos leaders project, Colgan creates a binary dummy variable for revolutionary regimes. He does the same for petro-states, coding them as such if oil income makes up 10% or more of GDP (he also later recodes them if over 20% as a robustness check, more on this below). The interaction effect of the two variables (the result when they are multiplied together, yielding a value of 1 only when both component measures also take a value of 1) is the book's main independent variable, both in the quantitative analyses and in the qualitative narratives that follow. The implied questions follow: would revolutionary regime A be this aggressive if it had no oil? Would petro-state B be more aggressive if led by revolutionaries? These questions get some treatment but not always as much as the reader might hope.

Measurement questions arise here as well. For example, an ordinate scale from least to most revolutionary might have been employed as a check on one decision for binary coding. And the alternate oil wealth measurements (pp. 48–49) are not necessarily the best option for the question at hand, and the absence of any robustness tests using annual changes is surprising. After all, we

would intuitively expect a revolutionary regime to become more aggressive with more oil income at hand, and less so with less.

In addition, the petro-state dummy status might be endogenous to whether they are revolutionary. As with recent research (e.g. Christa Brunnschweiler, “Cursing the Blessings? Natural Resource Abundance, Institutions and Economic Growth,” *World Development* 36, 3 [2008]: 399–419; Daniel Lederman and William Francis Maloney, *Natural Resources: Neither Curse Nor Destiny*, 2007) demonstrating that poor institutions often prevent sound economic policy which leads to resource export dependence, we might well expect revolutionary regimes either to drive out foreign investment and/or to focus on command economics, and to expropriate (and subsequently undermine) or to neglect economic policy altogether in lieu of more politically charged priorities. The downstream result would be that the rest of the economy suffers, raising the share of GDP made up by oil income. As a result, revolutionary regimes might shrink the size of the non-oil economy, pushing the “petrostate” dummy from zero to one. Unfortunately, Colgan gives no real attention to this likely problem.

Another potential problem, this time with the case selection, is that all revolutionary petro-states are ones that overthrew monarchies. Venezuela is nowhere near as internationally aggressive as any of the main Middle East cases, and so it is very plausible to make the case that the post-monarchy regimes are unique cases. This would also suggest estimating the models in Chapter 4 without Iran or Libya to check whether potential outliers are skewing the statistical results. None of this is to say that Colgan’s argument is in trouble here. Quite the contrary, it is only because of the great analytic care that is in evidence throughout that it is even possible to engage these debates. To reiterate, this is a very good book, well worthy of deep scholarly engagement, and I am confident it will take a central place in the development of the research program on resource politics.

Because the United States has only recently withdrawn from Iraq—a revolutionary petro-state under Saddam Hussein—and because oil accounts for more than 90% of the commodity revenue in the global economy, oil will continue to attract attention from scholars, policy makers and the public. That, however, should not be reason for scholars to relax the standards that give our conclusions merit. Rather it is imperative that we engage this highly public research program with a solid commitment to making sure that our measures capture the concepts we intend them to, and that our conclusions rest on solid empirical footing. As research on the politics of resource wealth continues to expand and improve, and as the size of the global oil market continues to grow, a steady focus on solid social science foundations is essential.

**Worse than a Monolith: Alliance Politics and Problems of Coercive Diplomacy in Asia.**

By Thomas J. Christensen. Princeton: Princeton University Press, 2011. 318p. \$75.00 cloth, \$28.95 paper.

**China’s Search for Security.** By Andrew J. Nathan and Andrew Scobell. New York: Columbia University Press, 2012. 432p. \$32.95. doi:10.1017/S1537592713002740

— David Kerr, *Durham University*

Asia’s strategic politics are likely to be the growth sector in International Relations in the next 20 years as academics and practitioners attempt to keep analysis and commentary apace with regional developments in security and diplomacy. These two books provide important contributions to the study of this field, but with quite different perspectives and methodologies. Thomas J. Christensen’s book is the more ambitious since it has important hypotheses to test on the nature of successful and unsuccessful alliance management in East Asia. In contrast, the book by Andrew J. Nathan and Andrew Scobell largely eschews theory and aims for a rich empirical account of China’s contemporary security policy and capacity.

Christensen’s concern is with coercive diplomacy defined as “the use of clear and credible threats and assurances in combination to dissuade target countries from undesirable behaviour” (p. 2). It may seem evident that, the more unified and integrated an alliance is, the more challenging coercive diplomacy becomes, but Christensen argues the opposite: Intra-alliance divisions and rivalries greatly complicate the organisation and communication of threats and assurances from, and towards, such an alliance. In order to test this first hypothesis Christensen conducts detailed historical analysis of how the alliance systems interacted in East Asia across a 25 year period, from alliance formation in the late 1940s to escalation in Southeast Asia and the U.S.-China rapprochement of the early 1970s. His specific cases are: the lack of coherent signalling from both alliances during the period of alliance formation and how this contributed to misjudgements especially in the onset and escalation of the Korean war, 1949–51 (chapters 2 and 3); an interim period, 1951–56, when the communist camp was at its most ideologically and organisationally coherent and effective diplomacy was achieved in Northeast Asia and Indo-China (chapter 4); the onset and escalation of the Sino-Soviet dispute, 1957–72, and the opportunity this generated in the communist camp for subordinate members of the alliance to advance their objectives at the expense of stability in the regional system overall (chapters 5 and 6); and the decline of coercive diplomacy in the US-China relationship in the final decades of the Cold War, 1972–91, but the return of certain aspects of coercion in the post-Cold War period especially due to changes in the US-Japan relationship and on Taiwan (chapter 7). By careful and detailed examination of primary and secondary sources on the diplomatic and strategic calculations of key actors—particularly