Global Governance to the Rescue: Saving International Relations?

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International relations teeters on the edge of an abyss of irrelevance. As an academic pursuit, it has become disparate and fragmented. Those of us in the discipline have ceased to pursue greater clarity in the way that we understand the world around us. Moreover, we have failed as agents of change; that is, as purveyors of opinion and proposals about a better and fairer world order. As such, we no longer serve our students and those practitioners who seek our advice, or, for those of us who take on policy jobs, to push out the envelope of what is considered acceptable. Global governance offers one potentially compelling way of “saving international relations”—though it is not without its problems. This article outlines how and why. The argument unfolds in three parts. The first outlines why and how IR teeters on the edge of an abyss. The second offers a proposal for moving beyond the fragmentation and atomization that afflicts international relations. We suggest that one way of encouraging reengagement is to return to debating grand questions that used to be the sustenance of IR. The third part argues that global governance—appropriately and specifically framed to make it fit for purpose—offers an opportunity to return to these questions and, in so doing, reinvigorate our fragmented and atomized field. Keywords: international relations, global governance, world politics, international organization, theory, praxis.

Our purpose in this article is to provoke and propose. We aim to provoke a reaction from our colleagues who, at worst, have not yet awakened to the fact that international relations (IR) teeters on the edge of an abyss of irrelevance or, at best, have yet to be spurred to refresh a field much in need of revitalization. IR as an academic pursuit has become disparate and fragmented. Those of us in the field have ceased to pursue greater clarity in the way that we understand the world around us. Furthermore, we have failed as agents of change; that is, as purveyors of opinion and proposals about a better and fairer world order. As such, we no longer serve our students and those practitioners who seek our advice—or for those of us who take on policy jobs, we no longer push out the envelope of what is considered acceptable. We too seldom offer a set of tools for understanding how the world works, a grounding on which socially beneficial policy can be created, and a framework for thinking about change.

How we have arrived at this point is easier to explain than moving beyond it. We nonetheless have a proposal: to move back toward the future—
to the table of grand disciplinary debate—by applying the not yet fully utilized concept of global governance.

Our argument unfolds in three parts. In the first part, we outline why and how IR teeters on the edge of an abyss. Here, we show that the field’s precariousness is theoretical, methodological, pedagogical, and linguistic. In the second part, we offer a proposal for moving beyond the fragmentation and atomization that afflicts international relations. We suggest here that one way of encouraging reengagement is to return to debating grand questions that used to be the sustenance of IR. Questions come no grander than asking how the world is governed, how we have ended up with the global governance that we currently have, and what kind of order we ought to put in place to correct the myriad ills that afflict humanity and the planet that we so willfully neglect. Indeed, understanding the precise shape of the current world order is perhaps the fundamental question of international relations, but one on which we have tended to turn our backs in recent years.

Shunning big questions of world order has certainly not resulted because we have solved the riddle of how the world is governed or the concomitant puzzles of how power and authority are exercised, what the consequences of particular formations of organization and governance are, and how we might best engage in meaningful reform.

In the third part of this article, we argue that global governance—appropriately and specifically framed to make it fit for purpose—offers an opportunity to return to these as well as other questions and, in so doing, to reinvigorate our fragmented and atomized field. We are, of course, not blind to the problems that global governance itself brings. It has rightly been criticized as a catchall term. Lawrence Finkelstein asked in the first volume of this journal, “what is global governance?” He provocatively replied, “virtually anything.” What we suggest, however, is that questions of global governance can be a catalyst for a rejuvenated field as long as we resist the temptation to fall back into our old habits of asking and answering questions within the intellectual silos that we now inhabit. We conclude by showing how we can move fruitfully beyond this to reclaim global governance’s potential as a critical scholarly endeavor and—in our role as provocateurs—as the savior of IR.

The Edge of the Abyss
The field of IR—and those of us within it—should be proud of its success. Barely thirty years ago, “international relations” was a vague appellation that referred to those areas of political science that dealt with politics “beyond the border,” or “over there,” or “in foreign climes.” Those who practiced it became members of our faculties, but they were somehow different—they did African and Asian politics; the communist world; and, in North America
at least, European integration. Larger departments also had faculty members who studied forms of international organization—almost exclusively, the United Nations—and international law. However, with a few notable exceptions, the bulk of political science (and the areas that carried professional prestige) remained more squarely focused on the core executive, representation, and enfranchisement; national and local bureaucracy; comparative politics; and classical and neoclassical political theory.

The technological advances and economic forces that have propelled disparate peoples and places together, along with the rise of new forms of insecurity, have underpinned a growing and serious student demand for IR, resulting in a fundamental change in the complexion of political science departments worldwide. Not only is it now common that IR faculty—along with applicants for limited numbers of PhD positions—outnumber their more traditional counterparts, but also that students clamor to find internships and eventually work for one global institution or another and have careers with an international orientation. This demand has fundamentally altered the content of course offerings. Whereas once it was the Cinderella attracting the attention of the mainstream only when international applications spoke to core political science endeavors, research in IR attracts at least as much attention and prestige as its older siblings and its professional associations have grown as a result. For instance, the International Studies Association (ISA) has seen its membership grow from approximately 200 just after its establishment in 1959 to 1,000 by 1970, 1,900 by 1973, 3,000 by the mid-1990s, and over 6,000 by 2013, with participation at its annual meetings also having increased similarly.  

**The Way We Think**

However, we are victims of our own success. The burgeoning growth in IR as an intellectual pursuit has encouraged the community not to focus on overlaps and interactions, but instead on showing how different we are from one another, illustrating how our novel value-added distinguishes us from an all too often imagined orthodoxy. Perhaps we should not be surprised. We teach our students to be critical of conventional wisdom. In their graduate work we demand that they develop frameworks and pursue empirical enquiries that show how what we thought we knew to be true is not actually quite so. In a less frenetic field of study (i.e., one that has not grown so quickly and struggled for recognition), the result would be a steady advance of knowledge; the development, refinement, and critique of a canon; and a constant reflection on big and important questions.

That has not been the case for IR. The number of new entrants, along with the necessity to be different or to get a job or to secure tenure, requires making a name for oneself. The result has been fragmentation. As such, we have lost sight of the need (and, indeed, perhaps the capacity) to interact
more productively with one another, more often than not finding ourselves conversing with fellow converts and eschewing dialogue. Our intellectual splits are legendary—the British and US schools in international political economy (IPE) and the divide between rationalists versus reflectivists are just two of the more notable—but so are the intellectual silos that we now inhabit: poststructuralist, structural realist, constructivist, neo-Gramscian, feminist, solidarist, communitarian, cosmopolitan, pluralist, postmodernist, behavioralist, postcolonialist, and institutionalist, among many others. We do not interact in our journals (indeed, we seldom publish in the same places), we do not mix well at our conferences, and we are disparaging of what it is that we imagine others do.

This unhealthy intellectual state of affairs infects our students. We teach them our favored ways of looking at the world, deriding the wisdom of those whom we view as “the Other.” We encourage them—and in growing numbers as the demand for IR shows no sign of flagging—to seek out new ways of thinking about the world by combing other disciplines for novel approaches and harvesting the wisdom found therein as the new next best way of understanding the world around us. Ironically, this incentive makes us even less likely to talk with one another; it reinforces our intellectual silos; and it ensures that we are less, rather than more, able to think through how we could make the world a better place. Indeed, many of us have become more able to talk with colleagues in other disciplines than those within IR.

The Way We Research
Our problems are not, however, just theoretical. They are methodological too. IR has always had an unusual problem when it comes to how we conduct research. It has never been practical to expect that we or our graduate students can easily gain access to a war cabinet to study decisionmaking; hang out with trade delegates expecting that they will share their negotiating secrets; or spend time with combatants, victims of rape and torture, and perpetrators of crimes against humanity and acts of terrorism. So, we have pursued other research methods. For some, IR had to become more scientific; we needed to have formal theories and to count what we could to make sense of the world. For others, archives, interviews, and secondary sources became standard operating procedures.

The methodological divides are well known, and we have talked about them since at least the 1950s. Yet we have continually failed to bridge the qualitative-quantitative divide, let alone learn from a blend of methodological insights. Our rush to harvest approaches from other disciplines has compounded rather than attenuated this problem. We have imported, adapted, and adopted the methods from other disciplines; but in so doing we have reinforced the walls between us. In IPE, for instance, there is almost no bridging the quantitative and qualitative divide. The same can be said of the absence
of a conversation between historical materialist and poststructural IPE. These methodological differences, and absence of a dialogue that they tend to justify, are replicated across the entire field of IR—to which formal and constructivist approaches to the problems of war and peace provide ample testimony. Thus, our methodological and theoretical tools make us less inclined and less able to scale these barricades, let alone constructively engage in grand debates about big topics.

The Way We Teach

It is no surprise that IR’s theoretical and methodological fragmentation should be reflected not only in what we teach, but also in the way that we teach. The classroom instruction of IR has moved away from a common canon toward an approach to the international that typically starts from a favored theoretical and methodological position. Of course, exceptions exist; but it is not uncommon for North American universities to eschew teaching poststructural and postcolonial approaches, especially at the undergraduate level, for a focus on broadly realist and liberal institutionalist approaches, with an emphasis on rationalist and reflectivist methodologies. The converse can also be said of the way that IR is taught in British and Australian universities, where critical and postmodern approaches are likely to be taught to students at all levels while ignoring more traditional mainstream theories or formal methods.

Yet pedagogy is one place where interesting cross-disciplinary efforts exist. Daniel Drezner’s *Theories of International Relations and Zombies*—mirroring a general trend in political science, sociology, and philosophy to use popular culture and television programs in particular as vehicles for exploring specific questions—is a brave and welcome move. Its problem so far, however, is that rather than genuinely facilitating a broad-based conversation, it has illustrated what one approach or another might have to say about an outbreak of the “undead”—the point of Drezner’s book. This is akin to asking students to adopt certain stylized theoretical approaches. For instance, we should not be teaching our students about the workings of the UN and world diplomacy through caricatured activities (e.g., replicating en masse UN committees or simply concentrating on the General Assembly in model UN simulations). Rather we should be asking them to use their knowledge of what passes for debate in and around the world organization as a platform for thinking about new, alternative, and better ways of organizing relations among whatever collectives that we imagine are the primary political units of analysis. So—and to rather unfairly return to Drezner’s brave experiment—what Zombie IR should be teaching us is how to use global threats as the basis for constructing new world orders rather than as a medium for trotting out and regurgitating hackneyed theories and playing predetermined roles.
The Language We Speak
The problems that we face are not merely theoretical, methodological, or pedagogical. Our divisions, and with them our capacity and desire to scale the walls that divide us, are further reinforced by the very way we talk among ourselves. Each approach favors a language that reflects particular theoretical and methodological preferences, which needs to be learned for us to be able to understand, engage, and be heard. The dominant discourse within other silos is as foreign as romance languages are to Arabic or Chinese speakers. Few take the time to learn “foreign” languages or even dialects within disciplinary or field mother-tongues, which are prerequisites to building bridges across vast and growing divides, or to understand the cultures and practices to which they are organically connected. Carol Cohn recognized the incommensurability of the languages that we speak in IR almost three decades ago. Michael Barnett did likewise with his focus on UN-ese within humanitarian catastrophes a decade and a half ago. These are among the most poignant attacks on contemporary IR but we too seldom assign them to our students, let alone act on the important lessons that they convey.

In combination, our theoretical, methodological, pedagogical, and linguistic divides have brought us close to the edge of an abyss into which we can easily totter. We risk irrelevance and student dissatisfaction. We should not be mistaken in thinking that this threat is either overstated or distant. It is neither. As a community of scholars, we seldom try to understand the world in ways that identify common ground. We rarely teach students ways of exploring how to make the world a better place. We not only talk past one other, if we stopped to listen to ourselves, we would realize that we are occupying different stories in the Tower of Babel. In many ways, we have regressed, rather than progressed.

Returning to Grand Debate?
What passes for grand debate in IR has tended to be more about refining opposing approaches or theoretical traditions than engaging in genuine dialogue or advancing knowledge. As Steve Smith puts it,

Despite the discipline’s fondness for so-called great debates, there have been few; in the main, the differing positions have simply ignored one another. This does not mean that there have not been strong opposing positions within the discipline . . . there has indeed been a rivalry between competing theoretical frameworks; what there have not been are debates in the strong sense of the word (whereby contrasting positions indicate their superiority over rival positions through explicit debates).9

We are not harking back to the imagined days of a bygone golden age—they were far from that. We are simply pointing out that, no matter how badly
these debates were conducted (and we could and should have conducted them much better), we had a sense of ourselves; we had a common lexicon; we shared an appreciation for contested terrain; and we actively engaged. Yet as IR grew and our community increased dramatically and as we prioritized innovation, we moved away from the grand debates of realism versus idealism (with the occasional nod to some form of structuralism) and interpretivism versus behavioralism, the interparadigm debate,\textsuperscript{10} and the brief moment when a synthesis of sorts occurred in the form of the neo-neo debate.\textsuperscript{11} Subsequent turns in the field—postpositivist, cultural, and constructivist, among others\textsuperscript{12}—have tended to orient us along different and seldom converging paths, despite some notable attempts to do otherwise.\textsuperscript{13}

Such is our fragmentation that the regard in which we hold the field varies dramatically across our membership. For some the moniker “international relations” perfectly describes what it is that we do, but for others it represents all that is wrong with the way that we look at the world. We need look no further for ample evidence than the debates within departments that sit beneath the apparent semantics of naming graduate programs in “international relations,” “international politics,” “international studies,” “global politics,” “global studies,” and so on. This atomization and the resulting hostility toward and caricatures of the denizens of other silos means that we avoid doing precisely what IR should do; that is, generate knowledge about the world around us. Instead, we pursue incremental movements forward in subfields that continue to move farther away from other subfields and that speak even more quietly to the field’s core concerns.

We do, of course, need to do more than return to an era when debates merely solidified what we already knew about our epistemological positions; and we need to recognize that we have never been very good at having grand debates about issues that are fundamental to our field.

If IR is to move away from the abyss of irrelevance, we should begin discussions that involve everyone: about how the world is governed; about how we have ended up with the kind of governance arrangements that we have; and, most importantly, about what kind of global governance we ought to have (and how we should get there). Moreover, we should engage in this debate mindful that we must fully explain the value-added of what we bring to the table, and that we must express and defend our approaches in a common language that everyone—even those from other fields and disciplines—can understand.

To repeat, the reasons for overcoming our atomization are compelling—IR risks losing its capacity to understand the world around us, excite our students, and contribute relevant global public policy formulation. A turn toward a grand global governance debate offers a clear opportunity. Yet a fieldwide debate is important not just to refresh IR, but also to make it relevant. We have yet to get a grip on precisely how the world is organized, how power and authority are
exercised by a host of actors through a range of mechanisms, and how forms of governance have changed within and across historical epochs (an essential task that few have even realized that we need to undertake). And these are analytical prerequisites so that we can set about designing a better world order—an activity that has fallen out of fashion, but is urgently required.

Consider the following: We have yet to work out satisfactory explanations for the relations that exist among actors of various sorts. Principal-agent theory, for instance, gets us only so far in understanding relations between the executive branch in Washington, DC, or London and the governing bodies of the World Bank or UN Development Programme (UNDP), or between chains of command in relations among a range of such actors as state legislatures, international organizations, and subcontracted delivery agents. It does not tell us nearly enough about differences in relationships between numerous agents and numerous principals over time and across contexts. Thus, it explains one small slice of what all of us might agree is international relations.

Likewise, we know a great deal about the power of financial markets. But we do not know nearly enough about how precisely global financial decisionmaking affects daily life other than to say that global financial crises make the everyday lives of ordinary people more precarious (and none more so than in the Global South) and to show this by associating various phenomena. The turn toward an “IPE of the everyday” is a good start, but not yet wholly satisfactory. What are the transmission belts, the role of regulation and regulators, the relationship between global commodity markets and local spot markets, and between traders and speculators? Similarly, we claim to have clear indicators that suggest that such states as China, India, and Brazil are rising in power and prestige, but the means by which we measure this rise are pure guesstimate, argument, and proxy (e.g., crude income statistics and human development indices, and so on).

We are learning a lot about how international norms arise, the processes of their transmission, and their subsequent success, mutation, and demise. But do we actually know how much of a difference they make? Can we really say that the Responsibility to Protect (R2P) and the norm of global poverty reduction that underpins the Millennium Development Goals (MDGs) have made a difference beyond crude speculation about counterfactuals? We know a great deal about international negotiations and decisionmaking. Yet how often has such knowledge been the basis for outside-the-box thinking about the reform of international institutions rather than a platform for tinkering at the margins (with the debate about expansion of the UN Security Council being perhaps the best and most circular as well as most unlikely to occur example)?

Perhaps the starkest illustration of why we need a serious debate about how the world is organized and governed is because we have not been good at accounting for, or indeed spotting, change and continuity. The end of the
Cold War has been endlessly picked over, but a humbling bottomline is that no scholarly or policy community saw it coming. This momentous change escaped prediction, but so too did the unintended consequences of what might happen if UN involvement in complex emergencies were to cease suddenly—as in the wake of US withdrawal from Somalia in 1992. In so doing—and despite many warnings of what was about to ensue—we helped the policy world sit back while genocide raged in Rwanda.

Equally, we have been slow to understand the role that new actors have come to play in the global economy. The call for market deregulation and rolling back state intervention in the 1980s and 1990s opened up more space for private involvement in the governance of global affairs. Some of the more notorious actors have been subjected to sustained analysis by IR scholars—credit-rating agencies and private military companies being perhaps the two most obvious—but we still do not have a good handle on the role of markets, the power and influence of transnational corporations, or the costs of such nefarious actors as organized criminal networks. Even more basically, we have little appreciation of the significance of public-private partnerships in shaping the world in which we live; for instance, the precise role of private firms in setting standards for food and health. Crucially, we have not explained sufficiently how power and authority in the current world order changed from those that we took for granted when scholars such as Hans J. Morgenthau were interested in questions of global arrangement.

If we have been blind to, or at the very least distracted from, changes in the nature of power and authority within and across the world in the post–World War II era, we have been even less sensitive to the drivers of change and their significance as we have moved from one world order to another. We use the language of Pax Britannica and Pax Americana, yet seldom offer a compelling account not only of what changed but why and how that change took place. Organizing principles are a key, but not the only part of explaining this puzzle. What Craig Murphy calls the “Inter-Imperial World” of the nineteenth century—in which European empires competed with one another over land, resources, power, and influence—was not organized simply by a compact among France, Austria, Prussia, Russia, and the United Kingdom to secure coexistence. The Concert of Europe explained how the interimperial world was governed only to the extent that it managed to mitigate, but not eradicate, war among the European empires. There is no explanation about the principles by which the rest of the world was organized—the ideas of racial segregation, les missions civilatrices as proxies for subjugation, the tutelage of “races yet unable to stand on their own two feet” as justifications for colonization, and the use of terra nullius and other cartographic inscriptions as reasons for conquest and acquisition.
Clearly, somewhere in the grand ideas that informed the interimperial system of *global governance*—we have been using this term only since the 1990s, but the reality of governance for the world existed long before—were both good and reprehensible interests. We have yet to grasp, however, the exact forms of organization that became concrete on the basis of these principles; the sources of power and forms of authority that they established, generated, and entrenched; and the consequences that they produced.

We are just as far, however, from understanding the forms of governing the world that result from the new ordering principles of US hegemony. These principles—self-determination and economic liberalization to name the two most prominent—also blended progress and self-interest. *Self-determination* was a genuine means of enabling colonized peoples to govern themselves, but also a way of opening colonial systems formerly more closed to US business; *economic liberalization* was both a commitment to a free flow of goods across the globe and a selectively applied policy that fell short of opening up key sectors to external competition (of which agriculture is perhaps the most significant). These principles gave rise to forms of formal and informal organization that have shaped the world around us, and they have changed and mutated over time.

A key missing element is the how and why of moving from the interimperial world to Pax Americana. The Quaker economist Kenneth Boulding often quipped with great wisdom, “We are where we are because we got there.” Yet to truly comprehend how we have arrived at contemporary forms of order and organization—as many are continuations of, or responses to, prior incarnations—we need to understand how global governance *avant le mot* was manifested and altered over time as well as how competing forms coexisted. This lacuna is not only important when considering the competing forms of global governance that the Cold War facilitated, or the harrowing counterfactuals that a Nazi system would have generated, but also how much older forms of world order coexisted over the centuries. Only then can we understand how and why we got to where we are. And only then can we go to where we want to go by getting there.

Thus, how the world is organized is a perquisite foundation for where we should and could be going. A plan for a better world is conspicuous by its absence. Instead of a grand debate about how to make the world fairer and more habitable, at best we formulate policies to muddle through and at worst justify ingenious ways to argue that dramatic change is unfeasible and beyond the pale. We do ourselves, the field, and the planet no service by removing from our job descriptions the need to put forward or actively debate alternative visions of more desirable future world orders.

Consider for a moment another academic field that allows us to embroider these points further. Economics is the discipline that encouraged many of us in IR to be more scientific about the way that we do our work. Yet what
we call “mainstream” or “orthodox” economics is as normative as many of the heterodox approaches that challenge it. It has clear ideas about what forms of organization and governance should prevail, how scarce resources should be allocated, and what kinds of policy ought to be put in place to bring about their realization—albeit they are presented as “fact” and “law.” We are not suggesting that we mimic the values and imagined futures held by mainstream economists, but rather we are recommending that IR make more room for clearer visions of what the world should look like, how it should be ordered, and how we should get there. This space should be a, if not the, staple of IR. And to be clear, we are not arguing here for a single vision of the future. Rather, in letting the proverbial hundred flowers blossom, we as a scholarly community can at least debate how the world ought to be organized rather than passively accept how it is.

The comparison with economics is worth pursuing further. Our lack of appreciation for what drives change in the way that our world is organized renders IR as a broad intellectual undertaking ahistorical. “History” might be something that we introduce to students in the opening lectures of an introductory IR class, but we tend to carefully circumscribe it: either treating history as an empirical treasure trove wherein we can find examples that fit or can be made to fit the way that we choose to explain the world, or else concentrating so narrowly on concepts or particular issues that the lessons from studying historical developments are obscured. IR, properly framed, has a great deal to offer in seeking answers to such broad social science questions as to why some countries are rich and others are poor. While economic historians, for instance, approach this question by looking for the drivers of growth, a concern with how the world is governed and how this governance has changed over time could help those of us in IR to better understand the forms of organization that have contributed to the prosperity enjoyed by some states and the poverty suffered by the vast majority of others.

For instance, Murphy’s International Organization and Industrial Change illuminates the role played by international organizations as modes of governance in advancing forms of economic accumulation. His work with JoAnne Yates on what we have called “creeping global governance” shows how minute, functionalist developments that standardize economic behavior and social norms lock in place systems of command and control that give rise to particular economic outcomes and social goods (some beneficial, some not). Murphy and Yates are notable exceptions in a field that has consistently failed to account for change, particularly in systems of governance. Even this ray of light, however, is obscured and only enables us to peer back into the relatively recent past and not into epochs of global governance gone by. If global governance is a legitimate analytical tool, it has to explain change not just today or in the post–Cold War era, but in other times and under other circumstances.
It thus is clear that IR must have something to offer if we turn our attention back to big questions—at least if we engage in exploring global governance. The data clearly show that those countries that were the richest in 1820 are almost invariably those that have prospered the most since (more often than not, dramatically), not just in crude measures like per capita income but also in health and other social indicators. We also know that it is from this period that the forms of global organization that we recognize today as international institutions were nascent and in the ascendancy, taking off with gusto from the 1850s—though we should be careful not to confuse “international institutions” with “global governance” as they are merely one specific, historically contingent element. European and later US power was consolidated through complex forms of command and control, and the opportunities enabled by these systems partially explain the dramatic and growing prosperity of those countries that were already doing well at the outset of the nineteenth century. Yet almost no one has sought to demonstrate how global governance and order have contributed to the accumulation of wealth for and improved morbidity of some and the relative and absolute impoverishment of others.

We should be extending such questions backward as well as forward. By so doing, we might realize how earlier forms of organization contributed to the success and demise of particular regimes and civilizations, which in turn could contribute to our knowledge about how best, and best not, to govern the contemporary world. The examination of how some orders mutated—such as the morphing of the Roman Empire into the Holy Roman Empire or, via a slightly “bumpy” route, the Holy See and Papacy today—could shed light on the forms of governance and principles of organization that generated greater propensities to peace, war, growth, or atrophy. Rome and ancient China are useful examples here in terms of both the perceived differences between the empires and the similarities in their internal and external forms of governance. Moreover, there are interesting questions of transitions between, mergers of, and interrelationships among imperial systems that help us better understand the governance of the ancient and not so ancient worlds that are part of the alloy that has helped forge our own world order.

Equally, the mechanisms by which empires were governed—the forms of administration, organization, and arrangement—could tell us much about elements of contemporary political contestation as well as about the forms of order that we almost had and should seek to avoid. The British use of islands as prisons, as places of forced exile, and as spaces to be emptied for colonial need is an example of a form of imperial governance that continues to have reverberations. The fascist economic tributary systems attempted by Nazi Germany and Imperial Japan during the 1930s were unequal forms of interstate organization that stand as examples of precisely the kind of global governance that we should avoid. As British social historian Ralph Fox put it in
the 1930s, “the existence of the Empire exercises a decisive influence on the life, and very often also the death, of every British-born man or woman.” Clearly, it also exercised a dramatic influence on the lives of colonial subjects. And investigating how this and other forms of organization—the worthy as well as the to be avoided—affect and have affected ordinary lives is a key motivation for better understanding the kind of global governance that we have and have had.

Making Global Governance Fit for Purpose in the Twenty-first Century

There is a structural problem with implementing our recommendation to begin an IR fieldwide debate about how the world is organized; why we have the forms of governance that we currently have; what forms of organization and order existed in other epochs worldwide; and how we ought to construct better planetary (or otherwise) systems of command and control. That underlying problem is the feeble intellectual traction of global governance itself. If it is to be made fit for purpose in this century, the term needs serious attention.

At its simplest, global governance should encourage us to ask questions about how the world is organized, how power and authority are exercised, and how adjustments (incremental, wholesale, or otherwise) can be made to make the world a better place. To date, however, the term has been so intimately wedded to a specific and contemporary moment—the post–Cold War era—that its analytical purchase is feebler than it should be. That moment saw the world as increasingly pluralistic, which encouraged analysts of all stripes to exploit the energy and resources of the growing number of nonstate actors to help solve problems that were (and continue to be) global in scale. Yet some used “global governance” almost interchangeably with “international organization.” Some worried that it was a proxy for world government—which, as Robert Cox so aptly points out, is just one conceivable form among many. Some were disparaging and too narrowly wedded to the notion that governance is the action of government and, thus, global governance must be what states as principals and their agents—intergovernmental organizations, especially the United Nations—do in the absence of a central authority. It is no wonder that Finkelstein pointed to the kitchen sink in the first volume of this journal.

The genius of coining the term global governance lay in James Rosenau and Ernst Czempiel’s original 1992 formulation as “governance” without “government”—the idea that myriad forms of authority and formal as well as informal processes could—singularly, or in tandem or concert—exercise governance (i.e., shaping and, to varying degrees, steering aspects of global life) without necessarily being connected to formal government. Moreover, they pointed out that these forms of authority, and the mechanisms through which
they operated as well as their guiding principles and overarching ideas and ideologies, existed at all levels of world politics. They interacted with one another in specific ways in given contexts, and they often produced contradictory outcomes and countervailing tendencies. However, the bottom line was clear: the sum was a discernible form of governance that was better ordered than we might have expected.

It is working out the total sum as well as what constitutes it, along with the contradictions and countervailing forces that exist therein, that are the real purposes of an inquiry into global governance. Yet how many of us assign to our students Rosenau and Czempiel’s original book, or indeed Rosenau’s landmark statement about command and control in the first issue of this journal? We suspected—and confirmed by a brief survey of available course syllabi on international organization and global governance from universities across the world—very few. Why? Because these texts are difficult, and their meaning is obscured by a struggle to understand just what was going on in the post–Cold War moment.

Chief among a host of thorny problems resulting from the widespread confusion about the term’s meaning is our failure to recognize that the global governance of the 1990s and early 2000s is different from the kind of global governance that existed in the nineteenth century, the first millennium CE, or indeed today. We should recognize that global governance, if it makes sense at all, is not merely a descriptor for a post–Cold War pluralistic moment but rather is a legitimate set of questions about how the world is governed, ordered, and organized in every historical period.

Historical change is perhaps the best point of departure for a wider debate to overcome the contemporary and seemingly ever growing fragmentation and atomization of IR. Put differently, if we apply the same kinds of questions that led to understanding global governance as a pluralization of world politics at the end of the last century, we should also be able to determine what kinds of systems of world order existed before the current one, and how power and authority were exercised therein. We should have insights about the ultimate drivers of change and their impact. Our argument is that a deeper investigation of contemporary global governance can potentially capture accurately how power is exercised across the globe, how a multiplicity of actors relate to one another generally as well as on specific issues, how to make better sense of global complexity, and how to account for alterations in the way that the world is and has been organized (or governed) over time—both within and between historical periods.

However, wrenching global governance from the contemporary moment and applying it historically is half the battle. This move backward would have limited salience if it also was not a valuable approach to understanding tomorrow. The future-oriented value lies in treating global governance as a set of questions that enable us to work out how the world was, is, and could
be governed as well as how changes in grand and not so grand patterns of
governance occurred, are occurring, and ought to occur. In short, if we have
nothing to say about how the world was, is, and should and could be gov-
erned, we should wonder whether what we are actually doing qualifies as
international relations.

**Saving IR?**
We have an advantage in trying to save IR from tumbling into an abyss that
other disciplines do not necessarily have. We have a captive audience and we
will be employed because what we do is associated with the big events of
today—terrorism, torture, recession, rising powers, poverty, proliferation,
atrocities, climate change, famine, pandemics, and the list goes on—and our
students continue to see international careers as attractive. Yet we should not
underestimate the problems that will arise if we do not stem and reverse the
field’s ever growing atomization, begin to talk more with one another, and
address serious questions about improving the way that the world is gov-
erned—all part of what global governance should push us to explore. It is a
pursuit, a set of questions, a form of inquiry. Ultimately, we are interested in
different manifestations of global governance and how they transition,
mutate, change, and develop.

If global governance does not come to the rescue of IR, we active par-
ticipants in the field may retain the dignity of the condemned walking to the
gallows in George Orwell’s retelling of “a hanging.” We may step aside and
temporarily avoid treading in a puddle of rainwater in our way. The outcome
will nonetheless be the same.

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**Notes**
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23. Murphy, *International Organization and Industrial Change*.


