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International Relations 2011 25: 499

DOI: 10.1177/0047117811411742

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International Relations
25(4) 499–511

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DOI: 10.1177/0047117811411742

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William C. Wohlforth

Dartmouth College

Abstract

I argue that realism in particular and IR more generally erred by assigning Kenneth Waltz's *Theory of International Relations* pride of place in revivifying realist thought. Had Robert Gilpin's *War and Change in World Politics* been given equal billing, international relations research would have unfolded quite differently over the past three decades. Scholars would not have been bewildered by change, bewitched by the balance of power, blind to numerous potentially powerful realist theories, and bothered by endless and unproductive zero-sum debates among representatives of competing paradigms. And had all those pathologies been absent, we would be far better prepared today for the intellectual and policy challenges of a world in which underlying power balances appear to be changing quickly, and the status quo inter-state order is ever more contested.

Keywords

Robert Gilpin, realism, international relations theory, Kenneth Waltz, balance of power, paradigms

When Robert Gilpin published *War and Change in World Politics* three decades ago, realism was poised for a major revival.¹ Given the centrality of this venerable scholarly tradition and the degree to which other schools of thought developed in response to it, how realism ended up revivifying and modernizing itself would have profound consequences for the discipline. Where would the scholarly field of international relations (IR) be today if Robert Gilpin had become the standard bearer for realism instead of Kenneth Waltz?² Or, more modestly, where would we be today if a distinctly 'Gilpinian' variant

Corresponding author:

William C. Wohlforth, Dartmouth College, 6108 Silsby Hall, Hanover, New Hampshire 03755, USA

Email: William.Wohlforth@dartmouth.edu

of realism had taken clear and distinct shape alongside Waltz's neorealism? Without questioning the brilliance of Kenneth Waltz's *Theory of International Politics*, in this essay I shall suggest the many ways in which our field would have been far, far better off had either of these counterfactuals actually occurred.³ I begin with sections on what happened and why it happened, and then make the main case: that as influential as Gilpin's work has been, had it been even more central to the key debates in the field, in all likelihood we would know a lot more about international politics today.

What happened

Robert Gilpin is one of the seminal thinkers in international relations scholarship of the past half century. Yet, according to citation counts and polls of practising scholars, his influence on the discipline, though large, remains well below that of his contemporaries Kenneth Waltz and Robert Keohane.⁴ The origins of this outcome can be traced to developments in IR research in the United States in the 1980s. In a nutshell, international relations 'paradigms' got defined in a manner that obscured Gilpin's contribution. In 1983, Keohane published an essay, 'Theory of World Politics: Structural Realism and Beyond', that treated Waltz's 1979 *Theory*, Gilpin's 1981 *War and Change*, and Snyder and Diesing's 1977 *Conflict Among Nations* as emblematic of an important new sophistication in realist theory.⁵ The following year, Robert Ashley published an essay 'The Poverty of Neorealism', critiquing the new realist scholarship and including Gilpin as a prominent exponent of it.⁶ At this stage, it seemed as if Waltz was but one of a coterie of scholars who were revivifying classical realism by developing a new, more social science-oriented neorealism.

Two years later, Keohane's edited volume *Neorealism and its Critics* appeared, quickly becoming one of the definitive works for graduate students of that era. Following and propelling an already established trend, Keohane's introduction to that volume portrayed Waltz, not Gilpin, as definitive of contemporary realism and as the preferred foil for the development of scholarship, including Keohane's own work. In addition to numerous critical essays, the volume presented the core theoretical chapters of Waltz's book. There could be little doubt: Waltz's theory *was* neorealism. In this short interval, Waltz's work thus came to trump all others as the definitive modern restatement of realism. And because realism plays such a large role in IR – if only as foil for others' work – whoever came to be seen as definitive of that approach and whoever came to be seen as offering the main alternatives to it would have an outsized influence on scholarship.

And so it happened. Waltz's take on realism became the defining foil for Keohane's institutionalism and then for Wendt's constructivism, and, less directly but still importantly for the English School, for the development of liberal theory, for democratic peace research, for both the formal modellers and the econometricians. Inevitably, the leading lights of those lines of research – most notably Alex Wendt, Bruce Russett and James Fearon – end up looming large in citation counts and scholarly poll results. And their prominence sustains the prominence of the scholars whose work shaped theirs, notably Waltz and Keohane.

As for realism, it continued to develop in Waltz's shadow. Recognizing realism's diversity, by the early 1990s scholars had begun identifying sub-schools within realism,

such as 'defensive' and 'offensive' realism. These were seen not as independent intellectual developments but as outgrowths of Waltz's own reformulation of realist theory into neorealism. Taking his cue from Waltz, the most influential realist of the current generation, John Mearsheimer, developed offensive realism in a major text that purports to be *the* new reformulation of realist thought.⁷ The Morgenthau–Waltz–Mearsheimer succession became the standard narrative about American realism, with Gilpin recognized as a major figure, but one whose work does not quite seem to fit.

Why it happened

Why did Waltz's *Theory of International Politics* come to be seen as the definitive work of modern realism, rather than Gilpin's *War and Change*? The more one compares the two works, the more puzzling their respective roles in the field become. Both are deeply learned. Both are incontrovertibly realist. Both are grounded in classical and modern works. In hindsight, Gilpin's work was arguably far more relevant to a world only a few years away from the major geopolitical upheavals occasioned by the Cold War's end and the Soviet Union's collapse, but no one could have known that in the early to mid 1980s.

I see three contextual factors as most important. First was the apparent relevance of the books to the events of the day. At the time, neither 'war' nor 'change' seemed to resonate with what seemed like a stable Cold War stalemate. By contrast, *Theory of International Politics* stressed the enduring verities of international relations in general and the Cold War in particular. The book was a potent riposte to all those flighty analysts who saw major structural changes afoot, from the complex interdependence touted by Keohane and Nye to the re-emergence of multipolarity highlighted by Richard Rosecrance, Stanley Hoffman and others, and the North–South axis featured in so many works of the time. To such talk Waltz replied that the deep structure of world politics remained anarchic and bipolar, which meant that constrained rivalry and cooperation between the United States and the Soviet Union would remain the central issue. And so it seemed to pan out. Interdependence, the 'Group of 77', the North–South struggle, dependency theory, the rise of Europe and the new multipolarity – all were swept aside as the 'new Cold War' of the early 1980s and the re-emergence of détente towards the decade's end appeared to validate Waltz's core messages.⁸

Second and arguably more important in the context of academe was the fact that Waltz presented his arguments in a way that best fitted the particular conception of social science that was just becoming fashionable among American political scientists. This was the idea that the great scholarly traditions of IR such as Realism and Liberalism should be refashioned as internally coherent scientific research programmes comprising a hard core of assumptions and a related set of scope conditions and specific propositions. It was a vision of how to think about scientific progress adopted from the philosopher Imre Lakatos's portrayal of the history of physics. Keohane endorsed it in his 1983 essay, though with many thoughtful reservations about its real applicability to IR. Over time, however, other scholars – both favourably and unfavourably disposed to realism – came to adopt this vision in far more unqualified terms.⁹

Whatever the merits of this Lakatosian vision, Waltz's book seemed tailor-made for it while Gilpin's did not. The books' very titles are indicative. One is about the specific

problem of change, while the other purports to be a comprehensive 'theory of international politics'. Clearly, the latter is more likely to be seen as the reformulation of realism into a scientific research programme. Unfortunately, few bothered to notice that the titles are misleading. Waltz's book is not really a theory of international politics. It does not address in any explicit way most of the phenomena that are encompassed by that term. Rather, Waltz presented a theory that purported to help answer a few important but highly general questions about international politics: why the modern states system has persisted in the face of attempts by certain states at dominance; the recurrence of balances of power; why war among great powers recurred over centuries; and why states often find cooperation hard. In addition, the book forwarded one more specific theory: that great-power war would tend to be more frequent in multipolarity than bipolarity.

Gilpin's 'red book' is no less sweeping and addresses a set of questions no less central to both the realist tradition and IR more generally: how to explain change in international politics; why defined international orders rise and decline; the causes of great wars and long periods of peace; and the rise and decline of hegemonic great powers. As I shall argue below, *War and Change* actually yields more relevant, testable middle-range theories than *Theory*. The book had the advantage over Waltz's *Theory*, moreover, in its comprehensive historical sweep. While Waltz's empirical references were almost wholly confined to the post-seventeenth-century European international system and its global successor, Gilpin's analysis included pre- and non-European international systems stretching back to antiquity. And Gilpin's book was arguably more comprehensive than Waltz's in its explicit focus on the interaction between economics and politics.

But notwithstanding all these qualities that a scholar of a different temperament might have been tempted to tout as a comprehensive theory, Gilpin did not 'pretend to develop a general theory of international relations that will provide an overarching explanatory statement'. Rather he modestly claimed only 'to provide a framework for thinking about the problem of war and change in world politics'.¹⁰ This modesty was commendable, but it reduced the likelihood that the book would come to be seen as a definitive restatement of realism.

Of course, the larger context doubtless interacted with the authors' own proclivities. Gilpin had no interest in redefining realism and becoming its standard bearer. He wanted to provide a better theoretical framework for understanding war and change. As he made perfectly clear in his 'blue book' *Political Economy of International Relations*,¹¹ he did not believe that realism or its competitors were, could or should be unified theories or internally coherent, scientific research programmes. They were, he held, part complex intellectual tradition and part political ideology. Given those beliefs, Gilpin would not have forwarded himself as the champion of a new realist 'theory of international politics' even if he had been inclined to do so. And, as those who know him will be quick to affirm, he was *not* inclined to do so. He was neither a self-promoter nor a nurturer and promoter of like-minded graduate students. Those he did train are living proof of his commitment to intellectual eclecticism and independent thinking.

The third and arguably most important factor is that Waltz's *Theory* provided a far more attractive and convenient foil for other scholars. Its status as the preferred foil for other lines of inquiry resulted from the possession of many attractive features *War and Change* lacked. *Theory* appeared 'parsimonious', having very few working parts. *War*

and Change was a congeries of propositions. *Theory* was more thoroughly 'structural', operating solely at the systemic level. *War and Change* contained multiple avenues for interaction between the domestic and systemic levels. *Theory* appeared to construe structure as material, while *War and Change* appeared to allow in important roles for ideas. A rigorous, structural and materialist theory was simply a much more attractive and modern foil than a messier, multivariate framework.

But *Theory* was not only attractive, it was also very, very convenient. It purported to be the last word on what structure defined in material terms could explain. It followed that anything *Theory* could not explain was, in some sense, important – if only as a lesson in the limits of structural theory. The result was a long list of publications claiming that Waltz's theory could not explain this case, that event or the other phenomenon. Had *Theory* never been written and invested with IR's aspirations for what a structural theory could be, it is hard to see what contribution all these books and articles made.

And the book was written in such a way as to make many fairly obvious things about international relations seem to be major theoretical puzzles in need of arcane scholarly explanations. The theory could be read as saying, for example, that domestic institutions do not matter in explaining large-scale patterns of war and peace. Any theoretical or empirical demonstration that the nature of domestic institutions 'matters' in accounting for these patterns could be touted as a major finding. Similarly, the theory could be read as saying that international institutions do not matter in international politics, which, as Keohane quickly pointed out, created a major puzzle out of the simple fact that states expend considerable time and effort in the creation of such institutions. And the theory could be read as saying that the nature of collectively held ideas or beliefs do not matter in explaining politics in anarchical settings, pitching another slow ball for the emerging constructivist school.

Gilpin's writings do not lend themselves to this sort of portrayal. They manage to say new and non-obvious things about international politics without seeming to deny the possibility of such a large number of easily observable facts of international life. Gilpin's works do not rule out a causal role for ideas, institutions and domestic politics, but rather stress their interaction with material power. No one reading them could derive the implication that a Soviet- or Japanese-led international order would operate like an American- or British-led one.

The root of these differences is not that Gilpin is rich, nuanced, subtle and somehow less scientific than Waltz. Rather, as Stephen G. Brooks pointed out in a seminal article, it is that even though they are both clearly realist works, the two books are built on very different foundational assumptions.¹² Waltz's theoretical edifice rests on the assumption that states are conditioned by the mere *possibility* of conflict, while Gilpin assumes – more in keeping with expected utility theory and most mainstream social science – that states make decisions based on the *probability* of conflict. Waltz's worst-case, possibilistic assumption was the key link between the condition of anarchy and all the strong and counter-intuitive implications about state behaviour he derived that attracted so much controversy in the 1980s and 1990s. In Gilpin's probabilistic world, however, states may well choose a wide variety of strategies depending on their assessment of the probability and severity of security threats. They may choose to pursue economic gain instead of security if the probability of conflict is low, or they may choose to pursue power and

prestige in the near term in order to be more secure in the long term. Thus, for Gilpin, states do not always ‘maximize security’ at all times and under all conditions, as Waltz held. Theories that ‘assume that one can speak of a hierarchy of state objectives ... misrepresent the behavior and decision-making of states’, he insists. Rather, ‘it is the mix and trade offs of objectives rather than their ordering that are critical to an understanding of foreign policy’.¹³

Brooks argued persuasively that Gilpin’s foundational assumptions were completely distinct from those that undergirded Waltz’s neorealism as well as classical realism, and that a number of other self-defined realists adopted an approach to explanation similar to Gilpin’s. In Brooks’ view, this warranted the designation of a distinct sub-school within realism that he called ‘postclassical realism’. Had Brooks’ analysis truly taken hold, then Gilpin’s work would have come to be seen as the foundation of another distinct, modern realist school of scholarship alongside neorealism. But this did not happen. Brooks, too, was late to the party. By the time his article appeared, sub-schools within realism had already come to be called offensive and defensive realism, which were seen as outgrowths of Waltz’s neorealism.¹⁴ And, though he began to be classified as an ‘offensive realist’, Gilpin clearly did not fit in either of these schools.

The costs: what we lost

In sum, Gilpin’s work did not fit smoothly into the conceptual categories that developed in the 1980s to classify and organize IR research. The result was a failure to exploit fully its many virtues, which arguably exacted large costs for realism and international relations scholarship more generally.

Bewildered by change

First and most obvious is Gilpin’s theoretical apparatus for explaining change. Neorealism fosters a comparative statics approach to explanation in which, for example, properties of a multipolar system are contrasted to those of a bipolar system. It fosters a bias towards expectations of stability – a bias on vivid display in the waning years of the Cold War. Gilpinian realism, by contrast, encourages scholars and policy makers to

think of any international system as temporary ... to look for underlying causes of change, which accumulate slowly but are realized in rare, concentrated bursts ... to be on the lookout for gaps between the capabilities of states and the demands placed upon them by their international roles.¹⁵

The costs of downplaying a potentially powerful realist theory of change are hard to measure, but it may have played a role in the field’s generally unimpressive record of grasping the significance of changes underway in international politics in the 1980s. *Theory of International Politics* promulgated a view of bipolarity as a stable structure to which the United States and the Soviet Union, as ‘sensible duopolists’, would tend to adjust.¹⁶ This led to the expectation that they would cooperate tacitly to sustain the division of Europe and the stability it supposedly provided. This was the intellectual origin

of John Mearsheimer's famous 'back to the future' article, which urged Moscow and Washington to maintain Europe's division in the interest of peace. Numerous analyses followed suit, urging Washington, Bonn, London, Paris and Moscow to prop up the bipolar order in Europe, regardless of the costs this imposed on the peoples of East-Central Europe, in the overall interest of preserving stability. In short, people schooled deeply in Waltz's theory were not primed to expect policies such as Washington's and Bonn's successful effort to cajole and bribe the Soviets out of Europe.¹⁷

Needless to say, it is easier to construct a circumstantial case for the analytical costs of neorealism's static structuralism than it is to make the positive case that a more prominent place for Gilpin's realism would have improved matters. After all, Gilpin's approach did lead to arguments that do not stand up well in hindsight, notably regarding Japan's 'challenge' to US hegemony. And *War and Change* never directly addressed the scenario that was about to unfold: the decline of a clearly weaker challenger to a given hegemonic order. In keeping with the mood of the time, the book treats the Soviet Union as the main and most dangerous rising challenge to the United States, which is portrayed as in serious relative decline.

Yet it is hard to read the book today and avoid the conclusion that, had readers at the time known that it was the Soviet Union, not the United States, that was in steep relative decline, their eyes would have been opened to the likelihood of peaceful change. The book does not, as some allege, claim that war is the only mechanism of change. Far from it. Following E. H. Carr, Gilpin discusses appeasement as a key strategy for adjusting to changed power realities, noting only that it has often been hard to achieve in the past. In his Epilogue, Gilpin goes on to profess 'cautious optimism' about peaceful change because a number of factors – especially the high expected costs (nuclear weapons) and lower expected value (decreased economic importance of territory) of war – raised the attractiveness of appeasement as opposed to war. There is very little in the book to support the notion that force would be an optimal response to Soviet decline within a US-dominated order, and a lot to support the expectation that appeasement and accommodation to new power realities would win the day.

In short, had Gilpinian realism been more prominent at the time, the field would have been better prepared intellectually for rapid power shifts and major geopolitical change. This is not to claim that we would have predicted the Cold War's end. Rather, the point is that a more prominent role for a realist framework for explaining change would have meant a larger role for debates about shifting power balances and their implications for the Cold War order. While hardly predictive, the scholarly conversation would arguably have been much more relevant to what was about to happen.

Another way to assess the cost imposed by failing to accord Gilpin's theory a more central role is to consider important questions it raises that have not been addressed. In *War and Change*, Gilpin argued that leading states 'will attempt to change the international system if the expected benefits exceed the expected costs'. Remarkably, however, the Cold War's and the Soviet Union's dissolution never prompted international relations scholars to debate whether the 'expected net gain' of system change might be positive for the United States. After all, the Soviet Union's demise ushered in a major power shift in the United States' favour, presumably increasing the net expected benefits of system change. It is hardly surprising that scholars set aside the question of revising the territorial

status quo – plausible arguments for the benefits of large-scale territorial conquest in the nuclear age are hard to imagine. But the territorial status quo is only a part of what Gilpin meant by ‘international system’. The other part comprises the rules, institutions and standards of legitimacy that frame daily interactions. Why no debate on changing that aspect of the system?

One answer is the hold that neorealism has on many scholars’ minds, coupled with the disinclination of non-realist scholars to investigate power-centric questions. Neorealist thinking led many observers to assume that unipolarity was but a ‘moment’, and so long-range projects of systemic activism did not appear germane. Even if unipolarity might last, most realists accepted Waltz’s core argument about the incipient potential of great-power counterbalancing to rein in any leading state seeking to alter the system’s rules. At the same time, in keeping with the theory’s premises, many neorealists downplayed the significance of revising rules, institutions and standards of legitimacy. And in the years since neorealism’s advent, other schools of thought were disinclined to explore the ways in which material power can shape rules, institutions and standards of legitimacy, tending instead to see them as distinct constraints on material power.

As a result, scholars seemed to be taken by surprise when the United States showed signs of a restless revisionism, first in the Clinton years and then, more dramatically, under George W. Bush. As Robert Jervis put it at the time, ‘The odd fact [is] that the United States, with all its power and stake in the system, is behaving more like a revolutionary state than one committed to preserving the arrangements that seem to have suited it so well.’¹⁸ It would be a caricature to suggest that Gilpin’s theory predicted the specific content of US policy – much less to suggest that anything in Gilpin’s writings would suggest that it was somehow optimal. The point is that post-Cold War US revisionism does not appear ‘odd’ in light of the theory, and that, had more scholars been thinking of US unipolarity in Gilpinian instead of Waltzian terms, the field would likely have been better prepared analytically to understand what was about to happen.

Bewitched by the balance of power

A second virtue of Gilpin’s work is less widely recognized but arguably far more important: the theoretical ideas developed in Gilpin’s red book have stood the test of time much better than those in *Theory*. There are many reasons for this, including the fact that Gilpin’s work is richer, containing a greater array of theoretical arguments and testable conjectures. But that just makes the larger point that realists made a wrong turn when they restricted the range of realist theories by hitching their wagon so securely to Waltz.

The best example concerns balance-of-power theory. Waltz’s work is built upon a rigorous rendering of this venerable theory, which he posited as *the* ‘realist’ explanation for the ‘recurrence of balance’ through international history, by which he meant the failure of repeated bids for European or global hegemony. Balance-of-power thinking had been a part of realism long before Waltz, but his 1979 book elevated its role almost to the point of making it synonymous with realism as a whole. The result was a realist obsession with this theory that continues to this day, as realists struggle to explain the absence of genuine counterbalancing against the United States.¹⁹ Scholars sceptical of realism

seized upon the repeated empirical failures of balance-of-power theory as evidence of realism's fatal 'degeneration'.²⁰

Scholars' preference for seeing neorealism as a comprehensive research programme obscured the fact that Gilpin presented a completely different theory to explain the same phenomenon as Waltz. Surveying a far broader sweep of history, Gilpin argued that the balance of power played a distinctly secondary role in limiting hegemonic expansion when compared to other countervailing forces such as natural barriers and the loss-of-strength gradient, economic and technological limits to optimal size, and domestic institutions.²¹ Unlike the 'apples vs oranges' criticisms of Waltz's balancing theory that accumulated over the 1990s, which tended to focus on short time spans or a few cases, this was a proposition directly contradicting Waltz, for it explained an similarly general empirical regularity over a very broad sweep of history. Over 20 years after the publication of Gilpin's book, scholars finally got around to testing these two theories. The result? In a nutshell, Gilpin was right. Balance-of-power dynamics played a role, but a secondary or even tertiary one compared to the factors Gilpin identified.²²

Blind to potentially powerful theories

In short, in assigning such a large role to Waltz's neorealism, realists took a wrong turn by allowing the rich realist tradition to become overly static and narrowing the range of realist theories to those highlighted by Waltz. Gilpin's theory of change and his conjecture about the historical forces limiting expansion are just two examples. Had Gilpinian realism been recognized as a distinct school, many more important and arguably more plausible conjectures about international politics would likely have been investigated more thoroughly.

Consider, for example, the notion of 'imperial overstretch'. Historian Paul Kennedy coined this term to describe the fate of past leading states whose 'global interests and obligations' became 'far too large for the country to be able to defend them all simultaneously'.²³ Even before the wars in Iraq and Afghanistan degenerated into prolonged, draining commitments, the Bush administration's pre-emptive war doctrine led many analysts to warn that the United States too might be in danger of suffering from imperial overstretch.²⁴ But what do we actually know about it? Does it apply to a country like the contemporary United States, whose fiscal travails are more obviously the result of domestic social spending and insufficient tax revenue than external pressures?²⁵

Scholars lack good answers to these questions, in part because scholarship on imperial overstretch is dwarfed, in both volume and prominence, by the voluminous output on balancing. So much so that scholars often conflate the two phenomena: in the historical cases highlighted by Kennedy and others leading states suffered from imperial overstretch mainly because they faced a counterbalancing constraint that demanded more resources than they were able to extract domestically. In the post-Cold War world, however, the United States has not faced this kind of balance-of-power constraint. This raises a key question of whether there are limits to the US polity's capacity to generate power in the absence of the threat posed by a geopolitical peer rival.

Unbeknownst to many scholars, the outlines of precisely such a theory can be found in *War and Change*, which develops a set of propositions concerning what Kennedy

would later call imperial overstretch that are conceptually distinct from balance-of-power theory.²⁶ Drawing on major literatures in economics and economic history, Gilpin identified key *internal* factors that appear to cause the share of national income devoted to 'protection' (that is, national security and protection of property rights) and consumption (private and public consumption of goods and services) to increase as a society ages. In addition, he analysed two important (non-balance-of-power-related) external factors: the rising costs of political dominance and the loss of technological leadership, both of which, he contended, would tend to increase the longer a hegemonic power was at the top of the international heap.

The set of empirical and logical propositions that Gilpin advanced in 1981 is the closest thing we have to a comprehensive realist theory of imperial overstretch. After publication, it sat, uneasily, alongside leadership long-cycle scholarship (e.g., Modelski, Thompson, Rasler and Thompson) and world system theory (e.g., Wallerstein).²⁷ While those rise-and-decline schools continued to develop and test propositions relevant to imperial overstretch, realists remained focused on balancing. Given realism's centrality to the discipline, propositions about imperial overstretch did not become as central to scholarly debates as they arguably should have done.

While Gilpin's framework is in need of a lot more development and empirical research, it appears far more relevant to the United States' contemporary 'imperial' dilemmas than any version of balance-of-power theory. If realists had not put all their apples in Waltz's balance-of-power cart, we might today be armed with a productive research agenda on imperial overstretch in a world without great-power counterbalancing. And, had that occurred, realists might have had much more compelling theory and empirical results on which to base their counsel of prudence and restraint for the United States after the Cold War's end.

Bothered by zero-sum competition

The bottom line is that many of Gilpin's important power-centric theories remained unexplored because they did not fit in the neorealist conception of realist theory and non-realist scholars considered all power-centric arguments the province of realism.²⁸ Competition among the schools of thought in IR is a general phenomenon whose baleful side effects cannot all be blamed on realism. Nevertheless, neorealism is arguably far more prone to zero-sum interactions with other theoretical traditions than Gilpinian realism. Brooks identified one reason: it is very hard for neorealists to relax the strict possibilistic assumption that drives the theory, and the result tends to be all-or-nothing debates with scholars working in a probabilistic world. The basic assumptions about decision-making in Gilpin's writings do not have this feature, and so are less prone to zero-sum interactions of this kind.

But that is not the whole story. The way Gilpin developed the core theoretical arguments and basic concepts out of which he built his work appear far more compatible with wider intellectual trends in IR and social science than the counterpart concepts and arguments featured in Waltz's *Theory*. As noted, that theory gained prominence in part by downplaying or ruling out a causal role for variables central to other research traditions, notably international institutions, domestic politics and ideas. Gilpin's work is not

positioned in this zero-sum way. Far from ruling out a role for institutions, *War and Change* offers a powerful explanation for the creation and waning of institutional orders. Instead of positing institutions as somehow antithetical to power politics, he explained how centrally power and institutions interact. Similarly, Gilpin's writings deal explicitly with the ways ideas and domestic institutions affect both the rise and decline of hegemonic powers and the nature of the international orders they foster. As noted, these features might have made Gilpin a less attractive foil than Waltz, but they add up to a commonsensical complementary between research interests and candidate explanations usually associated with competing scholarly traditions.

Other core theories within Gilpin's larger framework are similarly conceived in a way that works better with ongoing developments in the field. The theory of war at the core of *War and Change*, for example, will sound familiar to contemporary students. Gilpin argued that war arose from a clash of preferences over the status quo order and contradictory assessments of the distribution of capabilities that could only be resolved by fighting. This rests on the same basic foundations as what has become known as the bargaining theory of war, since formalized by the likes of James Fearon.²⁹ Had Gilpin's work been more clearly accepted at the centre of modern realism, the complementarity between much pre-existing realist scholarship and the supposedly novel bargaining theory would have been apparent much earlier.

The list goes on. Gilpin's conception of prestige-seeking has since found powerful confirmation in both political science and psychology; his conceptualization of the concept of power, stressing the interaction between its material and psychological dimensions, is far more compatible with the rest of political science and other social sciences than neorealism's apparent materialism; and his approach to human decision-making that stresses expected utility and the resolution of trade-offs rather than a fixed hierarchy of preferences has found far greater applicability across the social sciences.

Conclusion

Had IR not been tricked by Lakatos into thinking that grand traditions of scholarship like realism and liberalism had to be translated into single theoretical 'research programmes', there is every likelihood that Gilpin's seminal treatment of war and change would have been recognized as being every bit as definitive a restatement of realist theory as Waltz's treatment of balance-of-power theory. And had that occurred, international relations research would have unfolded quite differently over the past three decades. Scholars would not have been bewildered by change, bewitched by the balance of power, blind to numerous potentially powerful realist theories, and bothered by endless and unproductive zero-sum debates among representatives of competing paradigms. And had all those pathologies been absent, we would be far better prepared today for the intellectual and policy challenges of a world in which underlying power balances appear to be changing quickly, and the status quo inter-state order is ever more contested.

Acknowledgments

My thanks to Wolfgang Danspeckgruber, Director of the Liechtenstein Institute on Self-Determination at Princeton University, for organizing the conference in honour of Robert Gilpin at

which this paper was first presented. For helpful critical comments on the most recent draft, I am grateful to the three anonymous reviewers at *International Relations*.

Notes

- 1 Robert Gilpin, *War and Change in World Politics* (Cambridge: Cambridge University Press, 1981).
- 2 In this essay, I use 'IR' to denote the academic discipline and 'international relations' to denote its subject.
- 3 Kenneth N. Waltz, *Theory of International Politics* (Reading, MA: Addison-Wesley, 1979).
- 4 In a survey of some 1400 IR scholars in the US and Canada, Gilpin was ranked 10th (in 2004) and 12th (in 2006) among scholars 'who have had the greatest impact on the field of international relations over the past 20 years'. In both years, Keohane, Waltz and Wendt were numbers 1, 2 and 3, respectively. See Daniel Maliniak, Amy Oakes, Susan Peterson and Michael Tierney, *The View from the Ivory Tower: TRIP Survey of International Relations Faculty in the United States and Canada*, available at: www.wm.edu/irtheoryandpractice/trip/surveyreport06-07.pdf. Cumulative citations in all political science journals on JSTOR for Waltz's *Theory* total 773, while Gilpin's *War and Change* garners 326; citations in the top five US journals (*American Political Science Review*, *International Organization*, *International Security*, *International Studies Quarterly* and *World Politics*) sum to *Theory*: 486, *War and Change*: 199.
- 5 Robert O. Keohane, 'Theory of World Politics: Structural Realism and Beyond', in Ada Finifter (ed.), *Political Science: The State of the Discipline* (Washington, DC: American Political Science Association, 1983).
- 6 Rickard K. Ashley, 'The Poverty of Neorealism', *International Organization* 38 (Spring, 1984): 225–86.
- 7 John J. Mearsheimer, *The Tragedy of Great Power Politics* (New York: Norton, 2001).
- 8 Note, however, that the Arab oil embargo and other developments in the international political economy helped propel that subfield within IR, in which Gilpin's work played a key role.
- 9 See, e.g., Bruce Bueno de Mesquita, 'Toward a Scientific Understanding of International Conflict: A Personal View', *International Studies Quarterly* 29(2), 1985, pp. 121–36; John A. Vasquez, *The Power of Power Politics: From Neorealism to Neotraditionalism* (Cambridge: Cambridge University Press, 1998); and Jeffrey W., Legro and Andrew Moravcsik. 'Is Anybody Still a Realist?', *International Security*, 21(2), 1999, pp. 5–55.
- 10 Gilpin, *War and Change*, p. 2.
- 11 Robert Gilpin, *Political Economy of International Relations* (Princeton, NJ: Princeton University Press, 1987).
- 12 Stephen G. Brooks, 'Dueling Realisms', *International Organization*, 51(3), 1997, pp. 445–77.
- 13 Gilpin, *War and Change*, pp. 19, 22.
- 14 'Security Seeking under Anarchy: Defensive Realism Revisited', *International Security*, 25(3), 2000–1, pp. 128–61.
- 15 William C. Wohlforth, 'Realism and the End of the Cold War', *International Security*, 19(3), 1994–5), p. 14.
- 16 Waltz, *Theory of International Politics*, p. 203.
- 17 Mary Elise Sarotte, 'Perpetuating U.S. Preeminence: The 1990 Deals to "Bribe the Soviets Out" and Move NATO In', *International Security*, 35(1), 2010, pp. 110–137.
- 18 Robert Jervis, 'The Remaking of a Unipolar World', *Washington Quarterly*, 29(3), 2006), p. 7.

- 19 See, e.g., Robert Pape, 'Soft Balancing Against the United States', *International Security*, 30(1), 2005, pp. 7–45; and Christopher Layne, 'The Unipolar Illusion Revisited: The Coming End of the United States' Unipolar Moment', *International Security*, 31(2), 2006, pp. 7–41.
- 20 See, e.g., John Vasquez, 'The Realist Paradigm and Degenerative versus Progressive Research Programs', *American Political Science Review*, 91(4), 1997, pp. 899–912.
- 21 Gilpin, *War and Change*, pp. 146–54.
- 22 See W. C. Wohlforth et al., 'The Balance of Power in World History', *European Journal of International Relations*, 13(6), 2007, pp. 44–60; and Richard Little, Stuart Kaufman and W. C. Wohlforth (eds), *The Balance of Power in World History* (CITY: Palgrave Macmillan, 2007).
- 23 Paul Kennedy, 'The (Relative) Decline of America', *The Atlantic*, August 1987. Though he did not use the term, he provided a more fulsome definition in his magisterial *Rise and Fall of the Great Powers* (New York: Random House, 1987), p. 533:

it has been a common dilemma facing previous even as their relative economic strength is ebbing, the growing foreign challenges to their position have compelled them to allocate more and more of their resources into the military sector, which in turn squeezes out productive investment and, over time, leads to the downward spiral of slower growth, heavier taxes, deepening domestic splits over spending priorities, and a weakening capacity to bear the burdens of defense.

- 24 See, e.g., G. John Ikenberry, 'America's Imperial Ambition', *Foreign Affairs*, September–October 2002, pp. 000–00; and Jack L. Snyder, 'Imperial Temptations', *The National Interest*, Spring 2003, p. 3.
- 25 As Lawrence Kotlikoff and Niall Ferguson note, 'The key point – and here the resemblance with Kennedy's earlier argument ends – is that this overstretch has almost nothing to do with the United States' overseas military commitments. It is the result of America's chronically unbalanced *domestic* finances.' See 'Going Critical: American Power and the Consequences of Fiscal Overstretch', NYU, Stern School of Business Working Paper, May 2003, p. 2.
- 26 See Gilpin, *War and Change*, chapter 4.
- 27 See, e.g., Karen Rasler and William R. Thompson. *The Great Powers and Global Struggle, 1490–1990* (Lexington: University Press of Kentucky, 1994); George Modelski, *Long Cycles in World Politics* (London: Macmillan, 1988); and Immanuel Wallerstein, *The Modern World System*, 3 vols. (New York, Academic Press, 1974–89).
- 28 To be sure, some blame for this can be laid at Gilpin's door. For example, *War and Change* failed to acknowledge and explore the commonalities and differences between Gilpin's theory of hegemonic war and the variant developed by A. F. K. Organski in *World Politics* (New York: Knopf, 1958), and subsequently developed as power transition theory.
- 29 James D. Fearon, 'Rationalist Explanations for War', *International Organization*, 49(3), 1995, pp. 379–414; and Dan Reiter, 'Exploring the Bargaining Model of War', *Perspectives on Politics*, 1(1), 2003, pp. 27–44.

William C. Wohlforth is the Daniel Webster Professor at Dartmouth College, where he teaches in the Department of Government. Recent publications include 'No One Loves a Realist Explanation: The Cold War's End Revisited' in *International Politics* July–September 2011, and *International Relations Theory and the Consequences of Unipolarity* (Cambridge 2012), co-edited, with G. J. Ikenberry and M. M. Mastanduno.