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# WE CAN NEVER STUDY MERELY ONE THING: REFLECTIONS ON SYSTEMS THINKING AND IR

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### WE CAN NEVER STUDY MERELY ONE THING: REFLECTIONS ON SYSTEMS THINKING AND IR

ABSTRACT: Robert Jervis's System Effects was published just as systems thinking began to decline among political scientists, who were adopting increasingly strict standards of causal identification, privileging experimental and large-N studies. Many politically consequential system effects are not amenable to research designs that meet these standards, yet they must nonetheless be studied if the most important questions of international politics are to be answered. For example, if nuclear weapons are considered in light of their effect on the international system as a whole, it becomes clear that they have obviated the need for a global balance of power by allowing states to counterbalance threats by acquiring nuclear weapons rather than investing in massive conventional balancing efforts. Similarly, systems thinking should inform our understanding of the impact of a 'unipolar power'' such as the United States, which has enjoyed an overwhelming preponderance of conventional military power since the fall of the Berlin Wall. A unipolar power is likely to become involved in frequent conflicts because it is not restrained by the presence of a peer competitor.

The aphoristic version of Robert Jervis's *System Effects: Complexity in Political and Social Life* (Princeton University Press, 1997) is that "we can never do merely one thing."<sup>1</sup> Any action in a system will have multiple, intertwining effects. If this is so, we can never study merely one thing either. What does this mean for scholars of international relations, who study one of the most complex social systems of all, global politics?

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Without attempting to settle such a broad question in such a short piece, I will use this article to explore several of its aspects. I start by laying out the core of Jervis's view on systems and their effects for international politics. Then, I trace the causes and consequences of the decline of systems thinking in the field of International Relations  $(IR)^2$ since the publication of Jervis's book and reflect on its future application. Finally, I exemplify the implications of Jervis's views by applying them to two particularly important issues in contemporary international politics: nuclear weapons and U.S. power preponderance.

#### What Is a System?

According to Jervis (1997, 6), "we are dealing with a system when (a) a set of units or elements is interconnected so that changes in some elements or their relations produce changes in other parts of the system, and (b) the entire system exhibits properties and behaviors that are different from those of the parts." A system's key properties are, therefore, interconnectedness and non-additivity. Together, these two features of a system manifest themselves through what Jervis calls "system effects," with consequences for both those acting within a system and those studying it. Specifically, system effects limit both the control that political decision makers exercise over the outcomes of their actions and the explanatory power of the theories that scholars develop to explain those outcomes. The bulk of Jervis's book is thus devoted to highlighting the ways in which system effects complicate the consequences of decision makers' actions—and scholars' attempts to establish their causes.

Overall, Jervis finds at least four types of system effects. Each of them is the focus of a particular line of IR research.

The first relates to how one actor's preferences and strategies affect those of others. In IR, this type of system effect has been the object of abundant scholarship, using both formal and informal game-theoretic tools to study dynamics such as the security dilemma (Jervis 1978), coercion and escalation (Schelling 1960 and 1966), and the causes of war (Fearon 1995; Powell 2006).

The second type of system effect analyzed by Jervis centers on the complications produced by the interaction of more than two players. This has been the object of two types of IR scholarship. Some scholars have used multiplayer game theory to study, for example, the dynamics of hegemonic stability (Snidal 1985) or international alliances (Morrow 1994). Other scholars have used computer simulations and complex adaptive systems to explore the evolution of the international system (Pepinsky 2005).

Third, Jervis discusses the constraints imposed on actors by the structure of the system in which they operate. This type of system effect is well known to those familiar with systemic theorizing in IR, as it is the focus of structural realism (Waltz 1979; Mearsheimer 2001; Glaser 2010), the most self-consciously systemic brand of IR scholarship.

Finally, Jervis looks at the reverse system effect: how actors affect the structure of the system they populate. This dynamic informs much social theory (Giddens 1984) and in IR it is the object of process theory, a subfield of recent constructivist scholarship (Wendt 1999; Jackson and Nexon 1999; Pouliot 2008).

#### Why Social Systems Are Hard to Predict

Although these effects are produced by any type of system, according to Jervis they are particularly important in social systems. Unlike the agents that populate natural systems, social actors are reflective and can therefore change their preferences and strategies as a result of interactions with other agents, changes in structural conditions, and even their knowledge of particular theories about the consequences of their behavior. The selfreflective character of the actors that populate the international system states, political leaders, international organizations, and so on—therefore limits the predictive power of both those acting within the system and of those observering it, complicating the status of IR theories.

Take, for instance, the case of domino theory, which Jervis (165-76, 266-69) discusses at length. Because of system effects, domino theory's central prediction—that an agent's display of weakness on one particular issue will create a reputation for weakness that will, in turn, lead other agents to push aggressively on other issues, leading to a string of negative outcomes following the initial concession—is difficult to test. If political leaders believe this prediction, then after making a concession they will take actions that boost the credibility of their other commitments. These actions will have the effect of preventing the dominos from falling. Jervis calls this the "domino theory paradox": political actors' belief in domino theory leads them to implement policies that ultimately falsify it.

What, then, is the truth-status of domino theory? Its empirical falsification seems to depend on political leaders believing it to be true. If, on the contrary, its truth were not taken for granted, it might well be true.

Similarly, let's postulate for a moment that political leaders believe that the theory of offensive realism (Mearsheimer 2001) is correct. Drawing on historical evidence, this theory claims that political leaders consistently try to maximize the relative power of their states in an attempt to become a global hegemon. At the same time, because of system effects such as counterbalancing or the limitations to power projection created by oceans, offensive realists show that past political leaders have consistently been defeated in their hegemonic ambitions. Believing this, future political leaders might no longer attempt to turn their state into a global hegemon, thereby falsifying the theory at least in its prescriptive dimension.

How can we theorize in such circumstances?

In System Effects, Jervis aims at helping us develop the intellectual apparatus necessary to deal with such predicaments, with a particular emphasis on international politics. Philosophically, the purpose of his book is to combat our *anosognosia* (our lack of awareness about our ignorance) of system effects. Jervis reveals the system effects that shape and shove international politics, turning them from "unknown unknowns" into "known unknowns" to better inform our decision making and theorizing. The point is not to wish away the complexity of systems, rather to make actors and policy makers aware of it.

#### The Decline of Systems Thinking in IR

Systems thinking has been a perennial element of IR theory. Concepts such as relative power—reflecting the view that the expected consequences of one's actions must take into account the expected reactions of others—have been invoked at least since Thucydides used it to explain the origins of the Peloponnesian War. Likewise, the balance of power perhaps the oldest, most pervasive concept in IR theory (Little 2007) reflects a systems approach to thinking about international politics: Whatever one state does to disrupt the balance and gain a preponderance of power, other states will thwart that goal, re-establishing an equilibrium among them (Nexon 2009). Yet despite Jervis's encouragement to think in more sophisticated systems terms about international politics, systems thinking has been on the wane in IR during the fifteen years since the publication of his book.

To put this development in historical context—and painting with a very broad brush—between the mid-1960s and the late 1970s, IR was dominated by empiricist, inductive approaches (Russett 1969; Schmidt 2002). This type of scholarship aimed at establishing some ground truths by amassing facts about international politics that had until then eluded any systematic, objective treatment. Then Kenneth Waltz published his Theory of International Politics in 1979, explicitly trying to carve out a space for theory-systems theory, in fact. Waltz was so successful in this aim that the almost two decades between the publication of his book and Jervis's System Effects were a period during which IR was consumed with theoretical, often paradigmatic, disputes, with neorealism, neoliberalism, and later constructivism vying for primacy as the right lens through which to understand international politics (Keohane 1986; Hollis and Smith 1990; Monteiro and Ruby 2009). In sum, for twenty years after the publication of Waltz's book, grand systemic theorizing reigned supreme in IR.

Inevitably, this situation produced a backlash. Around the time *System Effects* came out, the pendulum started to swing back towards more empirical research and, at most, some mid-level theorizing. Since the late 1990s, systems theory, formal game-theoretic models, constructivist process theory, and computer simulations have all either declined or failed to live up to their initial promise. In my view, this decline is the result of mutually interacting intellectual and professional changes flowing from the rise of increasingly strict standards for identifying causal relationships, a trend that emerged throughout political science more generally.

The "question of causal identification" is the question of how we know that cause x has produced effect y. For positivists, who dominate the mainstream of U.S. political science, knowledge is at its most scientific when we can establish causation cleanly—i.e., when we can be certain that what we call the cause has indeed caused the effect, without the intervening action of other variables. The gold standard for producing this kind of knowledge is the experimental method. Our causal inferences are stronger when based on experiments in which the posited cause can be turned into a "treatment" and applied to subjects that are chosen randomly or "as–if" randomly from a representative

sample of the population, while the remaining subjects in the sample are presented with a control situation that is identical save for the absence of the treatment.

Such data can be generated by conducting laboratory (Webster and Sell 2007) or field experiments (Gerber and Green 2012), or by leveraging natural experiments (Dunning 2012). Second best to experimental data is large-N observational data, which may suffer from manifold biases but nonetheless (in the eyes of most social scientists) produces robust, statistically significant correlations on which we can pin our causal beliefs. At the bottom of the causal-identification totem pole are research designs that focus on relatively rare phenomena and rely on qualitative evidence to study them.

Alas, the types of questions that would benefit from systems thinking tend not to be susceptible to experimental or large-N quantitative answers, for two reasons.

First, the types of questions for which systems thinking is more important and fruitful, and indeed the questions for which systems thinking has been used in the study of international relations, tend to render experimental methodologies unfeasible, unethical, or impractical. For example, our ability to conduct experimental research on the origins of major wars, the sources of systemic change in international politics, the causes of nuclear proliferation, or the dynamics of nuclear escalation is quite limited. Furthermore, data on these questions is also limited, making large-N observational studies (deemed second best to an experimental research design) often also unfeasible. Our ability to study these questions using the two most reliable methods of causal identification is therefore restricted.<sup>3</sup>

Second, the nature of system effects undermines the advantages of stricter identification standards, such as the experimental method. The rules of the experimental method—according to which the only thing that can vary between control and treatment groups is the treatment are incompatible with Jervis's contention that, in a system, we cannot do merely one thing. If we want to capture system effects, then, the experimental setting is not our best friend. Of course, we can use experiments to attempt to isolate particular system effects. But in studying the interaction of multiple system effects, experimental methods, even when usable, would have little if any advantage over other, supposedly weaker, causal-identification strategies. By trying to change merely one thing (the treatment) at a time, scientists create an "unsystemic" environment from which system effects are artificially absent.

One could take this to mean that scientific methods cannot deal with the problems generated by system effects and that, consequently, we cannot have a science of IR—or a social science, for that matter, since societies and economies, not just international relations, are systems and therefore experience system effects.

I think this interpretation of the situation goes too far, because it is premised on an arbitrarily restrictive view of science for which we have no justification (Monteiro and Ruby 2009). Whether science (regardless of the methods it uses) can get at the way the world *really* works is not only an unsettled question, it is an unanswerable one. Our inability to ground science on a firm foundational footing—or to prove conclusively that such ground does not exist—should lead us to endorse a sort of foundational prudence, so we can continue to muddle through with our attempts to gain knowledge on different questions using the theoretical and methodological stances most adequate to each.

In addition, there is much to be learned about the workings of international politics in the area that lies between, on the one hand, the strictures of the experimental method and, on the other, the intractability of trying to capture all possible system effects produced by a particular policy or action. In this wide middle ground, we can continue with our attempts to illuminate how taking action x—despite the myriad unpredicted consequences it may have, as a result of system effects—is, on average, more likely than not to produce consequence y.

Jervis's work should therefore not be read as an invitation to abandon the scientific study of systems. Rather, it is an invitation to acknowledge the complications that system effects introduce in research on phenomena that take place in a systemic setting, and to adapt our methodological expectations accordingly.

Nevertheless, the rise in the standards for identification of causal power that we have witnessed in political science over the last two decades has led many IR scholars to abandon approaches that have a systems dimension to them. Instead, they have pursued more clear-cut research designs and have asked questions that are more amenable to being answered by such designs. As a result, many of the most important questions of international politics receive less scholarly attention than they did two decades ago, as do theoretical approaches such as systems theory. In turn, conducting research on systems questions reduces one's ability to publish in top peer-reviewed journals, gain professional promotion at top research universities, and, more generally, win scholarly recognition. Furthermore, faculty members want to train graduate students—and graduate students want to be trained—in the approaches that maximize their chances of producing research that is considered to be on the cutting edge. Increasingly, such research is defined in terms of its design rather than its question, in what is currently called "design-based research" (Dunning 2012). Thus, the shift in intellectual standards has been reinforced in exactly the manner that Jervis might have predicted for the "system" consisting of international-relations scholars. This has all but precluded political scientists from asking the types of questions about international affairs for which systems thinking can be more useful.

Now, I think most scholars—at least most positivists (and I think Jervis is a soft positivist)—would agree that we should use the highest standard of causal identification available for any given research question. But as a profession, IR faces a difficult choice: it can uphold the latest, most stringent standard of causal identification, using it as a research design yardstick which then restricts the domain of questions that members of the discipline can study; or it can relax identification standards to allow for research on issues that are not always amenable to being studied using the highest causal identification standards.

A more calibrated approach to the problem of causal identification would keep identification standards in a dialogue with the political importance of each particular question. If a research question is politically important—i.e., if it has the potential to affect the lives of numerous people in important ways—we should not abstain from studying it, regardless of whether it is amenable to the most demanding criteria of causal identification. Instead, we should study it by using the best research design to which it *is* amenable.<sup>4</sup>

The litmus test a particular causal identification standard has to pass in order to deserve broad application is whether we would live in a better world were we to "forget" all the research done using designs that are less reliable at causal identification.<sup>5</sup> A quick thought experiment may prove instructive on this point. Would we be better off without our vast array of scholarship on nuclear deterrence—a field built upon scant empirical data, because nuclear crises are relatively rare and nuclear exchanges have never taken place? Likewise, would we be better off without any knowledge of deterrence merely because the questions

associated with it—how to modulate escalation, how to bargain under the shadow of terror, how to convert arms to influence, etc.—are made so complex by the system effects at play in crisis dynamics that we cannot come up with experimental or large-N research designs to study them? I think not. What we need is a healthier dialogue between identification standards and complex but important political questions.

#### The Future of Systems Thinking in IR

For the most part, IR scholars associate systems thinking with systemic theories and, in particular, with structural neorealism (Waltz 1979; Mearsheimer 2001; Glaser 2010). But this need not be the case. The different system effects highlighted by Jervis can be studied only by using different approaches, of which structural neorealism, like systemic theorizing more broadly, is only one important instance.

The first step to encourage further use of systems thinking in IR is to separate it from a particular style of work, systemic theorizing, that is only one of several ways of thinking about system effects. In other words, we need to separate *system* effects from *systemic* effects. System effects take place within any system, even one with just two actors and a very simple structure, e.g., two-player games such as the stag hunt, prisoners' dilemma, or chicken (Schelling 1966; Jervis 1978). Systemic or structural theories are designed to capture this type of system effect when it applies to the entire international system: how its structure imposes constraints on the states that populate it. But the entire international system is made up of numerous sub-systems, down to the smallest ones, all of which generate their own system effects. So there are plenty of applications of systems thinking beyond structural theory. In short, many system effects are not systemic.

A second step towards greater use of systems thinking in IR would be to emphasize the possibility of incorporating it without discarding all hope of establishing causal effects. At the limit, a systems approach in which nothing is taken for granted—in which no *ceteris paribus* analyses are acceptable, and all the types of system effects described by Jervis are at work all the time, with strong consequences—would all but negate causality. As Jervis (1997, 57–58) puts it, "as actions combine to constitute the environment in which the actors are situated and actors in turn change as the environment alters, the language of dependent and independent variables becomes problematic."

This need not be the case, however. Systems thinking can take many forms, depending upon the constraints we impose on what is endogenous and what is exogenous to the theory. In fact, at least with the conceptual tools we have available today, we will not get any purchase on system effects unless we ignore or fix the complications introduced by some of them in order to study others. For instance, it will be difficult to study the effects of indirect interactions among three states if we do not bracket the issue of how these will transform the environment in which they interact. Conversely, in order to study how actors transform the environment in which they exist, we will likely have to abstain, at least in part, from understanding other system effects. Even strong constructivists who focus on questions of "mutual constitution"—questions that cannot be settled by looking at causal relationships, because the two variables affect each other—agree on fixing or abstracting from some parts of the world in order to interpret others.

Put differently, we need to allow for the study of system effects to vary according to two dimensions: the scope of the system we analyze and the scope of the variables we endogenize. This creates the typology of systems thinking presented in Table 1.

Although typologies require strong simplifications, I want to use this one to make two points.

First, systems thinking can be incorporated in small doses by studying particular system effects without endogenizing everything. For instance, two-player game-theoretic models have significant purchase over many

		scope of system		
		small	large	
scope of endogenization	small	two-player game-theory	structural realism, multi-player game-theory	
	large	constructivism	structural constructivism, process theory, computer simulations	

Table 1. Typology of Systems Thinking in	IF	ł
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of the system effects that relate to how one actor's preferences and strategies affect those of others. The fact that it can do so without endogenizing many other processes (such as, for instance, the effect of actors on their environment) may very well be what makes it relatively easy for two-player game theory to generate testable empirical implications (although it does so less than it should). Similarly, structural IR theories focus on a particular type of system effect—systemic constraints—without allowing others, such as two-player dynamic interactions  $\hat{a}$  la game-theoretic models, to factor in.<sup>6</sup>

Second, the different types of systems work listed in the table are in a complementary relationship with each other, not a competitive one, because they are not trying to explain the same system effects. Thus, process theory studies the sequences through which agents such as states reshape the structure of international politics-for instance, by gradually introducing international law as a constraint on state action (Koskenniemi 2001). Studying that process requires focusing on the bottom-up actions of various states that, over a long period of time, ended up producing the structural constraint that we to refer as international law. This is compatible with using a top-down, structural approach to studying the system effects of this new structure on the behavior of states. Similarly, constructivist scholarship devotes a great deal of attention to the processes through which norms emerge, spread, and become ingrained (Finnemore and Sikkink 1998). This is compatible with the subsequent use of twoplayer game-theoretic models in which the willingness to abide by certain norms is incorporated into the preference functions of the actors.

Much of the future added value of systems thinking in IR may come from the intersection of these different types of work. To make the point with a broad example: Structural realists generally argue that relative power, measured by material factors, is an important variable in determining how states will behave and thus, ultimately, in establishing systemic peace and stability. In structural-realist work, ideology is (with the puzzling exception of nationalism) merely a tool used by statesmen to advance their goals of security or power maximization. Structural constructivist scholarship, on the other hand, focuses on the role of ideology and norms in shaping the ways states interact in a systemic culture that, in turn, determines their needs for different levels and types of material capabilities, depending on the level of enmity and threat in the system (Wendt 1999). But whence do these norms come? To answer that question, we must resort to process theory, which focuses on the production of normative content by agents and its spread through social systems. How, then, do material capabilities foster the spread of norms by determining the power of different actors that participate in the process of spreading them? Only by paying attention to the ideational processes that change the structure of the international system *in a particular material context* will we be able to get a handle on this question (Barkin 2010).

In sum, in order fully to incorporate the power of system effects in theories of international politics, we need to embrace a political epistemology that captures the multiple factors that go into the process through which each agent forms its political views, and this, in turn, requires that we incorporate the interaction of material and ideational factors.

Let me now flesh out these ideas in two areas where system effects play an important role: nuclear weapons and U.S. power preponderance.

#### Nuclear Weapons and Systems Thinking

The introduction of nuclear weapons was significant enough to deserve the epithet of a "nuclear revolution" (Jervis 1989). Still, most academic work on nuclear weapons treats them as a unit-level variable. Seen from this perspective, they have consequences primarily for those states that possess them, and for those engaged in disputes with nuclear states.

Jervis's book invites us to think about nuclear weapons as a systemic variable, however. In other words, we should reflect on the role of nuclear weapons as a technology that transforms the structure of the international system in ways that go beyond the direct effects of nuclear possession by particular states.

In what follows, I focus on the effects of the nuclear revolution for balance-of-power theory. Specifically, I explore the impact of the nuclear revolution on the relationship between balancing, which is a state strategy, and the systemic balance of power, which is a systemic outcome. I contend that the nuclear revolution makes it possible for states successfully to achieve the goal of balancing (i.e., guaranteeing their survival) without producing a systemic balance of power.

Balance-of-power theory is usually presented as having a natural-lawlike quality: states' need to ensure their own survival against a background of systemic anarchy inevitably leads to a particular outcome—a systemic balance of power (Nexon 2009). As is often the case with the "naturalization" of social laws, the contingency inherent in the relationship between balancing mechanisms and systemic balances of power is hidden by this understanding of power balances as inevitable. Jervis's work on system effects allows us to question the taken-for-granted nature of the relationship between balancing and systemic balances of power in the shadow of the nuclear revolution.

The core logic of any balance-of-power theory is relatively straightforward. States care first and foremost about their own survival. An unmatched concentration of power in one state threatens the survival of the others. In order to improve their odds of survival, the others will therefore balance against concentrated power. Threats to survival are minimized only by amassing at least as much power as is possessed by any other state. Balancing efforts will therefore lead to the emergence of a systemic balance of power.

I do not wish to question the notion that survival is the first goal of states. But in the nuclear age, the remaining steps in the balance-of-power logic do not necessarily follow, because now an unmatched concentration of power in one state does not necessarily threaten the survival of others. Therefore, states' attempts to improve their odds of survival by balancing against concentrated power do not necessarily lead them to amass as much power as any other state; that much power may not be necessary to minimize threats to state survival. Thus, balancing efforts—even successful ones—may not produce a systemic balance of power.

An unmatched concentration of power in one state threatens the survival of others only if survival depends on a balance of power. This is the case in a conventionally armed world, where a state needs to possess roughly matching forces to deter a competitor's attack. Conventional inferiority vis-à-vis another state leads to military vulnerability and the inability to deter the adversary, ultimately undermining state survival.

In contrast, deterrence between powers with survivable nuclear arsenals is based on each state being unable to avoid suffering horrendous costs at the hands of the other in the case of an all-out conflict. Since this ability does not depend on a balance of conventional power, the possibility that a small nuclear arsenal can inflict a devastating retaliatory strike may deter any state—even one significantly more powerful in nuclear or conventional terms—from threatening the survival of the less powerful state. Therefore, states that acquire a nuclear arsenal have virtually guaranteed their survival even though they may possess negligible conventional capabilities. In a nuclear world, the connection between threat minimization and a systemic balance of power is severed. This possibility requires us to revise the view that a given state is able to guarantee its survival, and therefore stop its balancing efforts, only once it has amassed as much power as any other state (Monteiro 2009; Deudney 2011).

In a nuclear world, in fact, the pursuit of an overall (i.e., nuclear and conventional) balance of power between nuclear states is futile at best and dangerous at worst for a state's survival. Since two nuclear states are unlikely to go to war with each other—and, in any case, such a war would threaten the survival of both, regardless of the conventional balance of power between them—conventional balancing efforts between them for the purpose of assuring state survival would be relatively futile. Conventional balancing efforts could even be dangerous because an attempt to acquire conventional forces capable of being used against other nuclear states could be perceived by the latter as indicative of aggressive intentions, triggering preventive action (Debs and Monteiro 2012).

Furthermore, as Jervis (1997, 122) points out, nuclear weapons, by guaranteeing state survival, make alliances less important. It is senseless for several states to pool their nuclear or conventional resources to counteract a state that could destroy them anyway. This line of reasoning highlights the theoretical contradiction underlying mechanistic understandings of the formation of systemic balances of power.

As Jervis (1997, 132) writes, for a systemic balance of power to emerge, "war must be a viable tool of statecraft." The reasoning goes like this: States care about their survival; therefore, they must be able credibly to threaten potential adversaries with high costs in case of a major war; and the only way of doing so is to balance until they possess as much as or more power than any such potentially predatory state. Nuclear weapons, however, raise the costs of war to the point at which it no longer is a viable tool of statecraft among the most powerful states in the system. In fact, nuclear weapons make all-out great-power war unwinnable (Jervis 1989). This means that a major war in a nuclear world endangers a state's existence, jeopardizing the initial premise of the argument that led to balancing in the first place: states' preeminent interest in their own survival. Consequently, any threat of major war issued by a nuclear state against another nuclear state threatens the survival of the state that issues it. In sum, nuclear weapons make the ultima ratio of international politics-the ability to wage major war rather than allow an adversary to

threaten a state's existence—absurd. It would be, in Bismarck's words, suicide for fear of death.

Conversely, Jervis has argued, invulnerable nuclear arsenals make it very difficult to prevent *any* war between great powers from escalating into a total nuclear war. As he explains, all-out war is the result of "a dynamic process in which both sides get more and more deeply involved, more and more expectant, more and more concerned not to be a slow second in case the war starts" (Jervis 1989, 19). For a nuclear war to remain limited, and therefore winnable, one of the belligerents must be willing to give up while retaining the capability to inflict devastating damage on the other side (Craig 2003, 30). To the extent that defeat would risk state survival, this violates the first premise of balanceof-power theory: that states care first and foremost about their own survival.

Nuclear weapons therefore give paramount practical import to the distinction between theories of balancing and balance-of-power theories. As Daniel Nexon has pointed out, the extant literature often conflates the two, despite their lack of logical coherence. But theories that explain the conditions behind a strategy of balancing and the ways of implementing it (i.e., theories of balancing) are logically independent from theories that explain the formation of balance of power at the systemic level (i.e., balance-of-power theories). In Nexon's (2009, 340) words, "even theories that posit the ubiquity of balancing strategies need not imply that these strategies aggregate into systemic power balances." Balancing may be a common state strategy, yet the international system as a whole might be persistently out of balance. Nuclear weapons bring this disjunction into bold relief.

This leads us to the next topic to which I want to apply systems thinking: U.S. power preponderance.

#### Unipolarity and Systems Thinking

Since the collapse of the Soviet Union in 1989–91, the United States has enjoyed a position unique among that of modern states: it possesses far more military power-projection capabilities than any of its peers. As we saw in the previous section, nuclear weapons make this situation potentially durable. In a nuclear world, to use Jervis's (1997, 275) terminology, an international system with a preponderant power can reach a condition of quasi-homeostasis. In this section, I use systems thinking to highlight four other key features of the current era of U.S. power preponderance.

First, in order to explain why the overwhelming military power at Washington's disposal is "allowed" by other states, we must combine the effect of nuclear weapons on the need for balancing strategies, mentioned above, with two other factors.

The first is the historical trajectory through which power preponderance emerged. The United States happened upon its current position as a result of the Soviet collapse. Granted, Washington devoted massive resources to building up a first-rate military throughout the Cold War. But it did so in order to compete with another great power, the Soviet Union, which threatened the stability—some would say the viability—of the Western way of life. But the movement from being one of two great powers to becoming the uncontested foremost power in global military affairs was not the result of an intentional process. The United States did not gradually become *primus inter pares*. Rather, it acquired that position by the sudden disintegration of its only competitor.

In systems-thinking terms, this highlights the role of hysteresis, the fact that the "status of a system at a particular point . . . depends not only on the state of particular variables, but also on how that state was reached" (Jervis 1997, 38). Had the current U.S. power preponderance been the result of an intentional effort on the part of the United States to obtain it, it is likely that other states, suspicious of U.S. motives, would have stymied it early on through counterbalancing. Because aiming explicitly at global power preponderance signals aggressive intent, even nuclear states would have been likely to balance against a state keen on attaining it. In sum, the radical transformation of the international system produced by the Soviet collapse made possible the relatively stable nature of the post-Cold War system.

Second, system effects are useful to understand how U.S. power preponderance has a nuanced effect on war-producing mechanisms. On the one hand, power preponderance dampens the odds of competition and conflict among major powers (Wohlforth 1999). On the other hand, however, recent scholarship has highlighted the ways in which power preponderance also fosters conflict (Monteiro 2011/12).

The basic intuition behind this last argument is based on the type of indirect effects emphasized by systems thinking. When the world is organized into two or more blocs, each headed by a great power, weaker states can typically find an ally to boost their odds of survival in the face of a threat. So, for instance, if the United States or one of its allies threatened a weaker country during the Cold War, that country would be likely to seek and obtain Soviet support and sponsorship. In a world with a preponderant power, however, a state that feels threatened by it has no potential great-power sponsor.

This imbalance, by lowering the costs of war between a preponderant power such as the United States and a relatively weak state, has two concurrent effects. First, it boosts the bargaining leverage enjoyed by the United States in disputes with weak states unless its demands are so problematic (either because of their magnitude or the possibility that they will be recurrent) that the weak state has an incentive to resist them, risking war (Sechser 2010). Second, it weakens the credibility of U.S. negative assurances, undermining the coercive capability of U.S. pronouncements. Coercion requires both a credible threat of punishment being meted out in case the target ignores the coercive demands, and a credible assurance of punishment being withheld in case the target complies with them. A situation of power preponderance such as the one the United States enjoys today boosts the former but weakens the latter, making coercive attempts more likely to break down in war (Monteiro 2009). In sum, the indirect effects of its power preponderance help explain why the United States has been at war for fourteen out of the twenty three years since the Soviet Union abdicated its great-power status in 1989.

Third, system effects can help us understand why, despite conditions under which nuclear proliferation was expected to be rampant, we have seen very few attempts to acquire nuclear weapons since the end of the Cold War. Writing about the consequences of widespread nuclear proliferation, Jervis (2009, 213) worried that unipolarity may "have within it the seeds if not of its own destruction, then at least of its modification."As we saw in the previous section, nuclear weapons virtually guarantee the survival of any state—even a conventionally weak state faced with a global hegemon such as the United States. This should result in nuclear proliferation, as more states opt for acquiring the ultimate deterrent in order to ensure their survival. Still, only a handful of states have maintained nuclear programs with military goals since the end of the Cold War. Two have abandoned their nuclear ambitions: Iraq in 1995 and Libya in 2003. Two more are suspected of having ongoing nuclear programs: Iran and Syria. Perhaps more importantly, only one state managed to acquire nuclear weapons since the collapse of the Soviet Union: North Korea in 2006.

What explains this limited rate of proliferation, with only one new nuclear power in the past two decades, is the indirect effect of U.S. power preponderance. States that would value nuclear weapons know that they may be the target of a preventive strike by the United States aimed at avoiding proliferation. The lower the cost of an effective preventive strike, the more likely it is to happen. Therefore, U.S. power preponderance, by lowering the costs of counter-proliferation preventive strikes and making them potentially more effective, reduces the odds that a state willing to develop nuclear weapons will indeed get the opportunity to do so. Only by studying the interaction between the preferences (and strategies) of states considering nuclear acquisition with those of the United States can we explain the low rate of proliferation after the end of the Cold War (Monteiro and Debs 2012). In this instance, military power preponderance was self-sustaining.

Finally, system effects are crucial in evaluating the value of global power preponderance. What benefits does a state extract from it? What can it do that it could not were there to be a peer competitor?

It is possible that—at least in the security realm—power preponderance does not come with greater influence over international outcomes (Glaser 2011). As we saw in the previous section, in a nuclear world, power preponderance does not add much in terms of ensuring state survival. If states can ensure their survival short of establishing a systemic balance of power, then, *a fortiori*, they do not need to enjoy a preponderance of power to secure it. Likewise, power preponderance does not add much to a state's ability to project a security umbrella over its allies. In other words, preponderance is not necessary to provide credible extended deterrence guarantees to other states. After all, the United States was able to deter a Soviet invasion of Western Europe throughout the Cold War.

The only circumstances in which a preponderance of power may present a benefit in this respect would be in the case of a potential competitor assuming an extremely aggressive, risk-seeking posture (Glaser 2011, 141). Such a posture, however, would be self-defeating in a nuclear world, and is therefore highly unlikely. Absent extremely revisionist goals that are unlikely on the part of a rising power in a nuclear world, then, power preponderance is not necessary to ensure the ability of the United States to reassure its allies. To be sure, power preponderance does present the advantage of allowing the unipole to command the commons—the high seas, airspace, and outer space (Posen 2003). But the value of controlling the commons depends on the strategy implemented by the state that possesses it. Specifically, command of the commons only presents significant benefits for a strategy aiming at further increasing the state's preponderance of power. Such a strategy, however, is likely to signal aggressive intentions, triggering balancing dynamics that render it selfdefeating. In sum, power preponderance seems to be "much overrated" (Glaser 2011, 136).

In this essay, I have drawn inspiration from Jervis's *System Effects* to reflect on the value of systems thinking for some of the most important questions absorbing IR scholars and policymakers today. I conclude by summarizing what I think are the broader implications of his work for the conduct of IR scholarship and laying out three *futuribles*—three possible futures for systems thinking in IR.

Jervis's *System Effects* precludes simple theories or predictions; any mono-causal theory of anything social is bound to be of limited predictive power. We might be inclined to infer that social science— at least modern positivist social science, with its evolving standards of causal identification and its claim to describe and even predict the world—is nothing but a chimera. This would be too strong a reaction, however, and it would certainly not live up to the hopes Jervis himself expresses for his work.

Instead, we should proceed by implementing two principles. First, we should not abstain from studying politically important phenomena just because of the complications introduced by system effects, even if the latter preclude the use of the highest standards of causal identification. Second, we should tone down our claims about the predictive ability of any of our theories about phenomena that take place in a systemic context—as all the phenomena studied by IR do. Ideally, our theories will be able to show that when cause x is present, effect y is more likely. But when outcome y takes place within a system, system effects mean that many other interconnected, non-additive factors may condition it. We should therefore be cautious when extrapolating from one particular theory—focused on one particular dimension of a system—prescriptions

about how outcomes are produced, as if they were produced in a vacuum. Instead, we should strive to articulate how other dimensions of the problem may condition our claims. The truth, or something thereabout, should emerge from our combined understanding of these different aspects of the issue at stake.

The role of IR is not to develop a field theory that accounts for every dimension of every problem in international relations. Rather, its role is to develop different theories, different languages, each dealing with different aspects of each problem in international relations. Along the lines of Richard Rorty's (1999) aspirations for philosophy, the goal of social science need not be to have fewer and fewer theories, with the regulative ideal of a single theory, a single language, with which we try to explain everything. Instead, our goal should be to produce ever more theories, more languages, to capture ever more dimensions of each politically important phenomenon. This "let a thousand flowers bloom" approach would be of great value for policy makers as well. If we take Jervis's view of complexity in social and political life seriously, the goal of describing any situation in international politics using one only theory is wrongheaded. Decision makers will be better informed about their context and better able to predict the outcome of their actions if they are supplied with multiple theories, each describing a particular aspect of the situational choice they face.

Since the fiftieth anniversary of the Cuban Missile Crisis is upon us as I write, let me use it to illustrate what I mean. Were we to send one representative of contemporary IR scholars in a time capsule to meet with President Kennedy in October 1962, we would not want her to prime the president in any single IR theory. No single theory would convey to the president the sum total of our usable knowledge in situations such as the one he faced. Instead, we would instruct our representative to brief JFK on a multiplicity of theories: deterrence theory, the spiral model, and nuclear-escalation dynamics; the psychological factors at play in crises, and other sources of misperceptions; the role played by audience costs, reputational concerns, the balance of power, etc. All of these were in play during the crisis, and none of them would in itself suffice to deal with the problem President Kennedy faced.

In the light of this modest aspiration, I foresee three possible scenarios for systems thinking in IR.

The first, more pessimistic possible future is one in which the trend towards higher causal identification standards acquires hegemonic proportions in political science, excluding systems thinking and any research on the complex system effects that Jervis did so much to emphasize. As a result, IR scholars interested in causal complexity and a systems approach would be pushed out of political science and, in all likelihood, into policy schools. This transformation would impoverish IR and our understanding of international politics. The study of system effects and complexity in international political life is an enterprise to which social theory is essential, and therefore it belongs squarely within social science. Any policy-oriented work should be seen as an implication, not the core, of our study of systems.

The second possible future is one in which system effects remain a niche specialty in IR, with most of the field tackling narrower questions that are amenable to the highest standards of causal identification but are usually less politically important. Since this outcome is the one that follows from the current situation of IR, I think it is the most likely to obtain. It would, however, be a future in which complex interactions and the role of emergent properties singled out in Jervis's *System Effects* would be scrutinized only by a minority of those studying IR, slowing down progress in our understanding of complexity in international politics.

The third, more optimistic scenario is one in which questions about system effects are again the object of a great deal of scholarship, and the complexity of international politics is fully embraced as a legitimate object of scientific study. For this future to materialize, two things would have to happen: political scientists in general would have to acknowledge that important political questions are plagued by system effects but must nonetheless continue to be studied with the best research designs to which they are amenable. And those interested in system effects more specifically would have to redouble their efforts to extract empirical implications, formulate testable hypotheses, and ultimately test their work using the available data.

Only from this concerted movement would we be able to maximize the potential Jervis saw for tackling complexity in social and political life.

#### NOTES

- 1. The expression is Jervis's, who borrows it from Hardin 1963, 80.
- Throughout this essay, I adhere to the convention of using "IR" to refer to the discipline of International Relations and "international relations" to index its substantive domain of study.

- 3. My argument here is *not* that questions for which systems thinking is more useful tend to be questions on which we have limited available data. These two points are separate. That in international politics these two problems often go hand in hand does not imply any causal relation between the two statements.
- 4. Shapiro (2005) reaches a similar conclusion, calling for "problem-driven" rather than "method-driven" research.
- 5. For the sake of argument, I bracket the question of whether any type of research design can be guaranteed to do better in terms of causal identification. As I argue elsewhere (Monteiro and Ruby 2009), there are good reasons to be skeptical about any such foundational claims.
- Glaser 2010 is a partial exception to this and, in this sense, truer to its systemsthinking credentials, as is its concomitant loss of parsimony and predictive power.

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