UNYT

UNIVERSITY
OF NEW YORK
TIRANA

TRANSFORMATION OF THE INTERNATIONAL SYSTEM

COURSE READER

(2021)

DR. ADAM EHRLICH

UNIVERSITY OF NEW YORK TIRANA

Transformation of the International System

Semester:	March 5-20, 2022
Time/Place:	Friday-Sunday (see schedule for exact dates and times)
Professor:	Dr. Adam Ehrlich
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Office Hours:	By appointment
Turnitin ID/Pass:	33764384 / adam
Course Type:	Concentration Course (B)
Study Program:	MA in International Relations
Credits:	6 ECTS

Course Description

This course traces the origins and evolution of the concept of the "international system". It will look at the post-Cold War crisis of confidence regarding this international system and whether it can be reinvigorated as a model by incorporating into it concepts and methodologies of other disciplines, namely, international and world history. In turn, we will study the "world systems" theories put forward by historians and historical sociologists, both in their own right and as theories that can be of use to political scientists and IR theorists. As a case study for examining the different methodological approaches of historians and political scientists, we will look at the Cold War and how both disciplines are experiencing a "transformation" of outlook regarding this historical period.

Course Objectives:

Upon completion of this course, students should be able to:

- Place the IR theories they will learn in a historical context;
- Understand how the international system has changed over time;
- Contextualize the various historical stages of the international system;
- Develop an understanding of the historical method as an academic discipline;
- Analyze and interpret primary sources within a research scheme;
- Enhance their critical reading, writing and thinking skills.

Required Readings:

Transformation of the International System Course Reader, available at Ramovi Kanceleri (rr. Shkelqim Fusha, ph. # 69 48 07 688).

Course Requirements

Attendance & Punctuality: Attendance in UNYT classes is mandatory. I will keep track of your attendance and punctuality every hour of class. Absences will generally not be excused, since students are allowed to miss one day (four hours) without it affecting the attendance grade. In addition, students should refrain from arriving late without prior approval from the professor (or the hour will be counted as an absence). The grades for attendance will be as follows:

Hours Missed	Grade
0-4	**100%**
5	**95%**
6	**90%**
7	**85%**
8	**80%**
9	**70%**
10	**60%**
11	**50%**
12	**40%**
13	**30%**
14	**20%**
15	**10%**

Participation and Exams: There will three take-home essays (1500-1700 words each) based on readings discussed in class. In a Master's class like this one, participation is clearly important, and so will be evaluated with a relatively high percentage.

Office Hours: For Master's students, these should be arranged independently (although there are office hours posted on my office door).

Academic Dishonesty: UNYT does not tolerate academic dishonesty. Read the UNYT Student Honor Code for a more detailed description of plagiarism and cheating. Please be aware that assignments submitted via Turnitin should not receive a plagiarism mark of over around 10% or they may be marked down significantly.

Assessment Criteria

Attendance	**5%**
Essay I	**25%**
Essay II	**25%**
Essay III	**30%**
Participation	**15%**

Grading Scales

Letter	%	Generally Accepted Meaning
A	96-100	Excellent
A-	90-95	
B+	87-89	Good
B	83-86	
B-	80-82	
C+	77-79	Acceptable
C	73-76	
C-	70-72	
D+	67-69	Not Acceptable (not passing)
D	63-66	
D-	60-62	
F	59-1	Failing Grade
	0	No Submission

Course Schedule

SESSION	DATES	TOPICS, READINGS & EXAMS
I	**Saturday, March 5 (11.00-15.00)**	*The "International System" and Its Critics* Readings: **Kaufman, Mingst, Acharya, Buzan/ Little**
II	**Sunday, March 6 (11.00-15.00)**	*Bridges & Boundaries* Readings: **Elman, Levy, Jervis, Schroeder, Lawson, Lebow**
III	**Friday, March 18 (17.00-21.00)**	*Historical Sociology & "World System" Theory* Readings: **Griffiths, Wallerstein, Skocpol, Hobden, Nexon**
IV	**Saturday, March 19 (11.00-15.00)**	*The End of History – Or Its Rebirth?* Readings: **Kumar, Griffiths, Fukuyama, Huntington, Frank**
V	**Sunday, March 20 (11.00-15.00)**	*Coming Full Circle* Readings: **Denemark, Kaufman/Little/Wohlforth**

Full List of Readings

SESSION	Weekly readings consist of *abridged excerpts* from the following articles/books:
I	• Joyce Kaufman, *Introduction to International Relations: Theory and Practice,* 2013. • Karen Mingst, *Essentials of International Relations,* 1999. • Amitav Acharya and Barry Buzan, "Why is there no Non-Western International Relations Theory?" *International Relations of the Asia Pacific, 7* (3), 2007. • Barry Buzan and Richard Little, *International Systems in World History: Remaking the Study of International Relations*, 2000.
II	• Colin Elman and Miriam Elman, "Negotiating International History and Politics," in Colin Elman and Miriam Elman (eds.), *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations*, 2001. • Jack Levy, "Explaining Events and Developing Theories: History, Political Science, and the Analysis of International Relations," in Colin Elman and Miriam Elman (eds.), *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations*, 2001. • Robert Jervis, "International History and International Politics: Why Are They Studied Differently," in Colin Elman and Miriam Elman (eds.), *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations*, 2001. • Paul Schroeder, "International History: Why Historians Do It Differently than Political Scientists," in Colin Elman and Miriam Elman (eds.), *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations*, 2001.

	• George Lawson, "The Eternal Divide? History and International Relations," *European Journal of International Relations, 18* (2), 2012. • Richard Lebow, "Social Science and History: Ranchers vs. Farmers?" in Colin Elman and Miriam Elman (eds.), *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations*, 2001.
III	• M. Griffiths, "Immanuel Wallerstein," in *Fifty Key Thinkers in International Relations*, 2008. • "The Development of a World Economic System", in *Modern History Sourcebook*. • Theda Skocpol, "Wallerstein's World Capitalist System: A Theoretical and Historical Critique," *The American Journal of Sociology, 82* (5), 1977. • Stephen Hobden, "Theorizing the International System: Perspectives from Historical Sociology," *Review of International Studies, 25*, 1999. • Daniel Nexon, "Which Historical Sociology? A Response to Stephen Hobden's 'Theorizing the International System'", *Review of International Studies, 27*, 2001.
IV	• Krishan Kumar, "Philosophy of History at the End of the Cold War," in A. Tucker (ed.), *A Companion to the Philosophy of History and Historiography*, 2009. • M. Griffiths, "Francis Fukuyama," in *Fifty Key Thinkers in International Relations*, 2008. • Francis Fukuyama, "By Way of an Introduction," in *The End of History and the Last Man*, 1992. • Samuel Huntington, "The Clash of Civilizations?" *Foreign Affairs*, Summer, 1993. • André Gunder Frank, "A Structural Theory of the 5,000-Year World System," in *Theory and Methodology of World Development: The Writings of André Gunder Frank* (S. Chew and P. Lauderdale, eds.), 2010.
V	• Robert Denemark, "World-System History: From Traditional International Politics to the Study of Global Relations," *International Studies Review, 1* (2), Summer, 1999. • Stuart Kaufman, Richard Little and William Wohlforth, "Theoretical Insights from the Study of World History," in *The Balance of Power in World History*, 2007.

The "International System" & Its Critics

Introduction to International Relations: Theory and Practice

Joyce Kaufman

The Nation-State Level

With the broad theoretical frameworks outlined, we are now going to move through the various levels of analysis in order to focus on the major actors that can help us better understand the international system. In doing so, it is important to remember that *levels of analysis* is just a framework; it is not an inclusive guide to understanding all aspects of international relations or events in the international system. In fact, in going through this, one of the things that should become clear is where there are weaknesses or failings in this approach. As we go through the levels, it will also be important to think about how to apply your understanding of the level to current international events and which theory would be most appropriate to help describe and explain that event. Learning a theoretical approach is not helpful unless you can apply it, since that is the way in which you can determine how useful the theory really is.

We begin by focusing on the nation-state level, which is the primary actor in international relations. After defining the concept and putting it into historical perspective, we will move into an analysis of it, including understanding some of the major questions that have influenced the field of IR and that pertain to the nation-state. As we do this, it will be important to bear in mind the different theoretical approaches (i.e., realism, liberalism, constructivism, Marxism) to better understand how each can help explain aspects of the behavior of the nation-state within international relations.

Definition of the Nation-State

The current international system has evolved over time from one in which empires interacted based on trade and economics, to the emergence of the nation-state lonies to the truly globalized and interdependent world that we know today. We need to have an understanding of the *nation-state* as a concept: it is important to note that our focus is on the nation-state itself, and not on the individual leaders or the impact of the policy decisions on the people within the state. Given the central role of the concept *nation-state*, it is important to begin with a definition. When we look at a nation-state, we are looking at two separate yet interrelated concepts. *Nation* denotes a group of people with common history, background and values, all of whom accept the sanctity of the state. The *state*, in turn, represents the formal trappings of the political system, such as the government and defined borders, and it, in turn, accepts certain responsibilities for the people who live within those borders. Hence, a *nation-state* is an entity that we usually think of as a country, made up of groups of individuals who live within a defined border and under a single government. Even though there might be different groups of people with their own cultures and ideas, they form a single society that has certain values and beliefs in common.

Much of contemporary international relations theory is tied to the nation-state, known as a country, as the primary actor. Furthermore, there are assumptions made about the ways in which this unit behaves and reacts to other nation-states that can help explain major concepts such as why countries go to war, or how countries seek to influence the behavior of one another. Realism and structural realism explicitly address the nation-state as the critical actor in international relations. Liberalism also focuses on the nation-state as a primary actor, but it also looks within the state as well in order to get a more complete picture of the state's behavior. Constructivism focuses on the nation-state, but as an entity affected and constrained by the social and political structures within which it interacts. The critiques of these theories are often tied to flaws that are perceived as coming from the use of the nation-state as the primary unit of analysis.

In fact, one of the problems with the nation-state as a central concept of international relations is that there are often many nations or groups of people who live within a state and do not necessarily recognize the legitimacy of that single state. This suggests some of the weaknesses in focusing on the nation-state as the basis for international relations. The problem becomes more acute when we look at nonstate actors and stateless peoples. Despite some of these structural issues, understanding the nation-state and the central role it plays in international relations is critical to understanding IR theory.

History of the Nation-State

The approach to understanding the nation-state level and the basic concepts that are inherent in it (such as sovereignty) is derived from the 1648 Treaty (or Peace) of Westphalia, which outlines the concept of the *sovereign nation-state* and reminds all states of the importance of recognizing the sanctity of national borders. Since the time of that treaty, we have the emergence of the modern sovereign nation-state.

As we look back prior to 1648, we see a world that was made up not only of city-states but also empires. The Greek city-states were at the height of their power around 400 BCE. These city-states were characterized by relatively small populations with limited territory. Although they existed in close proximity, each was independent. Over time, Sparta and Athens emerged as the two major city-states, thereby creating a *bipolar system* in which power was roughly balanced between the two. Under the leadership of Athens, many of the Greek city-states united in what became known as the Delian League, an early idea of *collective security* that brought the Greek city-states together so that they could defend themselves from the Persian Empire, which had been trying to expand into Greek territory. Relations between Athens and Sparta deteriorated, ultimately leading to armed conflict. A truce was reached after six years, with each recognizing the power of the other and acknowledging domination over their respective spheres of influence. This truce was short-lived, however, and its failure led to the outbreak of the Second Peloponnesian War.

Why is this ancient history important? The creation of the Delian League, designed to protect against the perceived aggression of Persia, was one of the earliest documented examples of what was later known as *collective security*. What took place during the Peloponnesian War was also an example of realist politics and the balance of power. Following the period of the domination of the Greek city-states, we see the emergence of the age of empires. An *empire* (as opposed to a nation-state or a city-state) can be defined as an entity composed of separate units, all of which are under the domination of one single power that asserts political and economic supremacy over the others, which formally or informally accept this relationship. Thus, the separate units or groups have some independence, but they remain under the domination of a supra-entity. One of the major goals of an empire, like any system, was to ensure that it perpetuated itself and continued to expand its domain and therefore its wealth. Because of its size, often the ruler of the empire had to depend upon local officials to carry out his or her bidding. There were a number of empires throughout history, including those in Europe, such as the Holy Roman Empire and the Austro-Hungarian, and in Eurasia, such as the Persian and later the Ottoman. In Asia, the Chinese empire was in place from 221 BCE to 1911 (with some periods of disruption) and was characterized by centralized rule with allegiance paid to the emperor.

The end of the Roman Empire in approximately 5 CE led to what became known as the Middle Ages in Europe. During this time we see the growth of the Christian church, which melded political power and religion to solidify its empire. In Europe in the twelfth and thirteenth centuries, we also start seeing a flourishing of municipalities that functioned like the old Greek city-states. Venice, Florence, Paris, Oxford, and so on each became established centers of law and behavior, focused primarily on universities. Eventually this also led to a clash between secular rule and the church, and by the late Middle Ages, we start seeing the rise of what we now refer to as *nationalism*, specifically, commitment to a central identity or consciousness rather than loyalty to the ruler or state. We also see the emergence of strong monarchs who reigned over their domain, sometimes with the support of the church and sometimes in opposition to it, such as Henry VIII in England. This was also the start of the age of exploration and colonization, as states looked for ways to expand their wealth and fortunes by going outside the limited territory of Europe, leading to the early era of globalization. But as we also saw earlier, the growth of the city-states contributed to competition and eventually conflict between and among many of these states, especially regarding the role of religion and political power within the area that was known as the Holy Roman Empire. Eventually this led to the Thirty Years' War, which lasted from 1618 to 1648. The war devastated Europe, but the treaty that ended the conflict had a profound effect on the practice of international relations.

Treaty of Westphalia

The Thirty Years' War ended with the signing of the Treaty of Westphalia in 1648. This treaty established some of the basic principles that govern international relations today, as well as firmly establishing the nation-state as the primary actor in the international system with certain responsibilities and powers. The treaty established the European political system that we are familiar with. It ended the Holy Roman Empire and replaced it with a system of sovereign states. It made the monarch the primary political leader with authority over his people, supplanting the role of the church. Thus, as a result of this treaty, secular rule superseded the rule of the church. This in turn led to the notion that each national leader has the right to maintain his own military in order to protect himself and his territory. This also contributed to the growth of centralized control of the political system, since each monarch had a monopoly on the use of force for both domestic and external purposes. Thus, the individual state and the monarch or leader of the state became more powerful, with that power backed up by the use of force. In addition, the Treaty of Westphalia led to a redrawing of the map of Europe so that a core group of states became dominant, primarily Austria, Russia, Prussia, England, France, and the Low Countries (that would become Belgium and the Netherlands).

Along with the legacy of the modern nation-state, the Treaty of Westphalia also gave us some of the major concepts that govern the relationship between and among nation-states. Paramount among those is the concept of *sovereignty.* K. Holsti, in his classic text on international relations, notes that

> the principle [of sovereignty] underlies relations between all states today. . . . The principle of sovereignty is relatively simple: Within a specified territory, no external power . . . has the right to exercise legal jurisdiction or political authority. This establishes the exclusive domestic authority of a government. That authority is based on a monopoly over the *legitimate* use of force.

Holsti then notes that "no state has the right to interfere in the domestic affairs of another state. This prohibitive injunction has been breached frequently, but it is assumed and observed most of the time by most states." Although, as Holsti notes, there have been frequent violations of this norm, on the whole it provides the basic framework for relations between and among nation-states (i.e., international relations).

The important point to remember is that the current international system grew from events that took place almost four hundred years ago. Although some specifics have changed as new countries were created and as different political systems, such as democracies, evolved to replace the monarchy that was then the norm, the basic structure and concepts governing the nation-state and its actions in the international system remain in place.

Essentials of International Relations

Karen Mingst

The Emergence of the Westphalian System

Most international relations theorists locate the origins of the contemporary states system in Europe in 1648, the year the Treaties of Westphalia ended the Thirty Years' War. These treaties marked the end of rule by religious authority in Europe and the emergence of secular authorities. With secular authority came the principle that has provided the foundation for international relations ever since then: the notion of the territorial integrity of states – legally equal and sovereign participants in an international system.

The formulation of sovereignty – a core concept in contemporary International relations – was one of the most important intellectual developments leading to the Westphalian revolution. Much of the development of the notion is found in the writings of the French philosopher Jean Bodin (1530-96). To Bodin, sovereignty is the "absolute and perpetual power vested in a commonwealth." It resides not in an individual but in a state; thus, it is perpetual. It is "the distinguishing mark of the sovereign that he cannot in any way be subject to the commands of another, for it is he who makes law for the subject, abrogates law already made, and amends obsolete law."

Although, ideally, sovereignty is absolute, in reality, according to Bodin, it is not without limits. Leaders are limited by the type of regime – "the constitutional laws of the realm" – be it a monarchy, an aristocracy, or a democracy. And lastly, leaders are limited by covenants, contracts with promises to the people within the commonwealth, and treaties with other states, though there is no supreme arbiter in relations among states. Thus, Bodin provided the conceptual glue of sovereignty that would emerge with the Westphalian agreement. The Thirty Years' War, which had begun as a religious dispute between Catholics and Protestants, ended due to mutual exhaustion and bankruptcy. The treaties that ended the conflict had three key impacts on the practice of international relations.

First, the Treaties of Westphalia embraced the notion of sovereignty. With one stroke, virtually all the small states in central Europe attained sovereignty. The Holy Roman Empire was dead. Monarchs – and not a supranational church – gained the authority to decide which version of Christianity was appropriate for their subjects. With the pope and the emperor stripped of this power, the notion of the territorial state came into focus and people increasingly accepted it as normal. The Treaties not only legitimized territoriality and the right of *states* – as the sovereign, territorially contiguous principalities increasingly came to be known – to choose their own religion, but the Treaties also established that states had the right to determine their own domestic policies, free from external pressure and with full jurisdiction in their own geographic space. The Treaties thus introduced the principle of noninterference in the affairs of other states.

Second, because the leaders of Europe's most powerful countries had seen the devastation wrought by mercenaries in war, after the Treaties of Westphalia, these countries sought to establish their own permanent national militaries. The growth of such forces led to increasingly centralized control, since the state had to collect taxes to pay for these militaries and leaders assumed absolute control over the troops. The state with a national army emerged as a powerful force – its sovereignty acknowledged and its secular base firmly established. And that state's power increased. Larger territorial units gained an advantage as armaments became more standardized and more lethal.

Third, the Treaties of Westphalia established a core group of states that dominated the world until the beginning of the nineteenth century: Austria, Russia, Prussia, England, France and the United Provinces (the area now comprising the Netherlands). Those in the west – England, France, and the United Provinces – underwent an economic revival under the aegis of liberal capitalism, whereas those in the east – Prussia and Russia – reverted to feudal practices. In the west, states improved their infrastructure to facilitate commerce, and great trading companies and banks emerged. In contrast, in the east, serfs remained on the land, and economic development was stifled. Yet in both regions, states led by a monarch with absolute power (called "absolutist" states) dominated, with Louis XIV ruling in France (1643-1715), Peter the Great in Russia (1682-1725), and Frederick II in Prussia (1740-86).

Why is There no Non-Western International Relations Theory?

Amitav Acharya and Barry Buzan

International Relations of the Asia Pacific, 7 (3), 2007

Our goal is to encourage non-Western IR thinkers to challenge the dominance of Western theory. We do this not out of contempt for the IR theory that has been developed there, but because we think Western IR theory is both too narrow in its sources and too dominant in its influence to be good for the health of the wider project to understand the social world in which we live. We hold that IR theory is an open domain into which it is not unreasonable to expect non-Westerners to make a contribution at least proportional to the degree that they are involved in its practice. There is, in addition, the powerful argument of Robert Cox (1986) that "theory is always *for* someone and *for* some purpose." IR theory likes to pose as neutral, but it is not difficult to read much of it in a Coxian light, especially those that offer not just a way of analysing, but also a vision of what the world looks like (realism, English School pluralists), or should look like (liberalism, Marxism, critical theory, English School solidarists). In the Coxian perspective, liberalism, especially economic liberalism, can be seen as speaking for capital; realism and the English School pluralists speak for the status quo great powers and the maintenance of their dominant role in the international system. Though they are presented as universal theories, all can also be seen as speaking for the West and in the interest of sustaining its power, prosperity and influence. Various strands of Marxism and critical theory, meanwhile, claim to speak for excluded or marginalized groups (workers, women, Third World countries).

From this Coxian perspective, Asian states have an interest in IR theory that speaks for them and their interests. Neither China nor Japan fit comfortably into realism or liberalism. China is trying to avoid being treated as a threat to the status quo as its power rises, and the moves to develop a Chinese school of IR are focused on this problem. Japan is seeking to avoid being a "normal" great power and its status as a "trading state" or "civilian power" is a direct contradiction of realist expectations. The Association of Southeast Asian Nations (ASEAN) defies the realist, liberal and English School logic that order is provided by the local great powers. South Korea and India perhaps fit more closely with realist models, yet neither seems certain about what sort of place it wants for itself in international society. To the extent that IR theory is constitutive of the reality that it addresses, Asian states have a major interest in being part of the game. If we are to improve IR theory as a whole, then Western theory needs to be challenged not just from within, but also from outside.

WHAT DO WE MEAN BY IR THEORY?

It is important at the outset to have some sense of what "theory" means in IR. The question is problematic because of the dichotomy between the hard positivist understanding of theory, which dominates in the U.S., and the softer reflectivist understandings of theory found more widely in Europe. Many Europeans use the term theory for anything that organizes a field systematically, structures questions and establishes a coherent and rigorous set of interrelated concepts and categories. The dominant American tradition, however, usually demands that theory be defined in positivist terms: that it defines terms in operational form, and then sets out and explains the relations between causes and effects. This type of theory should generate testable hypotheses of a causal nature. These differences are captured in Hollis and Smith's (1990) widely used distinction between *understanding* and *explanation*. In both of these forms, theory is about abstracting away from the facts of day-to-day events in an attempt to find patterns, and group events together into sets and classes of things. Theory is therefore about simplifying reality. It starts from the supposition that in some quite fundamental sense, each event is *not* unique, but can be clustered together with others that share some important similarities. Each power rivalry (or development trajectory, war or empire etc.) will have both some unique features and some that it shares with others of its type. For theorists, the goal is to find the most powerful explanations: those where a small number of factors can explain a large number of cases. Waltz (1979) aims for this type of parsimonious theory with his idea that anarchic structure makes the distribution of capabilities the key to understanding the main patterns of international relations for all of recorded history.

For the enquiry that we have in mind, we do not think it necessary to get engaged in the bottomless controversies about theory that emanate from debates about the philosophy of knowledge. We are happy to take a pluralist view of theory that embraces both the harder, positivist, rationalist, materialist and quantitative understandings on one end of the spectrum, and the more reflective, social, constructivist and postmodern on the other. In this pluralist spirit we also include normative theory, whose focus is not so much to explain or understand the social world, but to set out systematic ideas about how and why it can and should be improved. Although normative theory has a different purpose, it shares the underlying characteristic of theory that it abstracts from reality and seeks general principles applicable across a range of cases that share some common features. Privileging one type of theory over others would largely defeat the purpose of our enterprise, which is to make an initial probe to find "what is out there" in Asian thinking about IR.

Given the peculiarities of international relations as a subject, it is worth saying something about whether IR theory needs to be universal in scope (applying to the whole system) or can also be exceptionalist (applying to a subsystem). As noted above, the holy grail for theorists is the highest level of generalization about the largest number of events. That impulse points strongly towards universalist IR theories, like Waltz's, that claim to apply to the whole international system and to be timeless in their application (though even Waltz can be faulted here for keeping silent about the vast swaths of history in which "universal" empires held sway, overwhelming his logic of international anarchy).

Yet there is also plenty of room for exceptionalism. Perhaps the leading example is European studies, where the emergence of the EU has created a regional political structure that fits neither domestic nor international political models. It is too far removed from anarchy to be Westphalian, and too distant from hierarchy to count as either an empire or a domestic political space. This post-Westphalian experiment has a reasonable claim to be exceptional, and is theorized about in terms of "multi-level governance" and other such specifically tailored concepts. In principle, area studies should be a main location for subsystemic theorizing. In relation to Asia, elements of this are visible in the idea that East Asia may be dressed up in Westphalian costume, but is not performing a Westphalian play. Because of its Confucian culture, East Asian states are more likely to bandwagon with power rather than balance against it. This line of thinking assumes that what Fairbank labelled the "Chinese World Order" – a Sinocentric and hierarchical form of international relations – has survived within the cultures of East Asia despite the superficial remaking of the Asian subsystem into a Western-style set of sovereign states.

The problem with area studies is that although it might well be the right location for subsystemic, exceptionalist theorizing, area studies is generally dominated by disciplines that have a low interest in theorizing, effectively taking exceptionalism to be a reason *not* to theorize. Subsystemic theorizing in IR is generally underdeveloped. Area studies experts mostly are not interested in it, and most mainstream IR theories concentrate on the system level (realism on "great powers", liberalism on "universal values", the English School on "international society", and "globalization"). Even theorizing about regionalism is often done in universalist, comparative terms. Despite the effective dominance of system-level theorizing in IR, it is clear that if pushed to an extreme, the logic of exceptionalist claims would deny the possibility of universal IR theories – or indeed any universal social theory. If cultural differences are strong enough, then shared features at the system level will be too thin to support universal theories.

If, as discussed above, all theory is for someone and for some purpose, this effectively makes universal theory impossible other than as a disguise for the secular interests of those promoting it. E. H. Carr's (1946) warning that "the English-speaking peoples are past masters in the art of concealing their selfish national interests in the guise of the general good" captures this Coxian perspective nicely, given the Anglo-American domination of IR. At the systemic or subsystemic level, is it possible to aspire to detached science in attempting to understand and explain how the world works, or must all such attempts be seen as inevitably part of an ongoing political game to sustain the hegemonic view and those whose interests are served by that view?

WESTERN DOMINANCE OF IR THEORY

There are two, partly reciprocal, ways in which the Western dominance of IR theory manifests itself. The first is the origin of most mainstream IR theory in Western philosophy, political theory and history. The second is the Eurocentric framing of world history, which weaves through and around much of this theory. Since the fact of Western dominance is not controversial there is no need to demonstrate this in great detail. But a brief sketch of the main branches of IR theory in this light gives a sense of the nature and sources of Eurocentrism that might well prove useful in setting up comparisons with non-Western thinking about IR.

Classical Realism, with its focus on state sovereignty, military power and national interest, is rooted in the diplomatic and political practices of modern Europe up to 1945. It likes to claim an intellectual pedigree in classics of European political theory such as Hobbes, Machiavelli and Thucydides, and uses this to support its claim that power politics is rooted in human nature, and is therefore a permanent, universal feature of the human condition. This, in turn, supports a foreign policy prescription based on self-interest, self-reliance, suspicion, vigilance and prudence.

Neorealism differs mainly by placing the source of power politics in the survival needs of states embedded in anarchic international system structures. Both classical realism and neorealism project onto the rest of world history their basic Europe-derived story of international anarchy and balance of power politics as a permanent, universal structural condition. They support this move by citing examples from both Western history (classical Greece, Renaissance Italy, modern Europe) and samples of non-Western history that run parallel to the European story ("warring states" periods in India, China and the Mayan world). Because of its commitment to anarchic structure and balance of power politics, realism largely ignores the great swathes of history where empires such as the Roman, the Han, the Persian, the Inca and the Aztec held sway over their known worlds. Its main historical story is the modern one in which Western powers both fight amongst themselves and take over the rest of the world, though that said, realism unhesitatingly makes room for any state, Western or not, that qualifies as a great power. Japan thus climbs into the realist frame from the late nineteenth century, and China began to do so after the communists took power. Realism's current privileging of the Western powers is thus historically contingent, and not built into the theory. Realism has played a major role in defining the mainstream subject matter of IR in state-centric terms. In that sense, it has been an accomplice to Western hegemony by taking the political system that the West imposed on the rest of the world, and declaring it the norm for all of world history.

Strategic Studies is closely linked to realism, generally accepting the realist interpretation of how the world is, and focusing within that on the technical, tactical and strategic aspects of military power and its uses. Strategic Studies is rooted in the tradition of the Western way in warfare and its classics: Clausewitz (Napoleonic wars), Mahan (British naval practice and strategy) and a host of responses to developments in Western military technology. During the Cold War, Strategic Studies flourished in the pursuit of deterrence theory as a response to the co-development of nuclear weapons and long-range missiles. In this pursuit it was much influenced by rational-choice modes of analysis drawn from Western economic thinking. Since then, it has been much obsessed with the so-called "Revolution in Military Affairs". But here at least there was some non-Western input with Mao Zedong and Che Guevara acquiring status as writers on guerrilla war, and Sun Tzu on strategic thinking. Like realism, the tendency of Strategic Studies to privilege the West is contingent rather than built in.

Liberalism and *Neoliberalism* have clear roots in European political and economic theory (Hobson, Kant, Locke, Smith), and in the Western practice of political economy from the nineteenth century onwards. The central liberal principles of individualism and the market (and more hesitantly, democracy) all come out of Western thinking and practice, yet are presented as universal truths that are applicable to – and beneficial to – all human beings. The general policy prescription of liberalism is the need to homogenize along liberal lines economic and political practices and human rights across the planet. Whereas realism reflects a backward-looking assessment of the European experience (how things were and always will be), liberalism reflects a forward-looking one: how to improve on past practice and move humankind towards a more peaceful, prosperous and just future. Justification for this frankly imperial perspective is found in the great relative success of the West (in terms of power and prosperity and justice) compared with the rest of the world during the past two centuries.

Marxism is the main reaction against liberalism's response to the rise of an industrial economy in the West. Instead of using individualism and the market to unleash the power of capital into an ever more prosperous future, Marxism sees the liberal formula as profoundly unstable and leading inevitably to class war. Marxism is the opposite of liberalism in preferring collectivism to individualism and a command economy to a market one. It also shares some of realism's belief in the durability of conflict in the human condition. But like liberalism, Marxism rejects the past and looks forward to a better future, and also sees its own prescription as universally valid. And like all theories, it is Eurocentric and emerges out of the unique intellectual, social, and historical conditions of the West.

The *English School* has its roots in much of the same Western political theory as realism (Hobbes, Machiavelli) and liberalism (Kant), albeit with more prominence given to Grotius and the idea that states can and should form among themselves an international society. The main models for this are found in European history, both classical Greece and modern Europe, though some work has also been done to show the existence of international societies in premodern, non-Western contexts. The English School's main contribution to world history is to show how an international society formed in Europe expanded to take over the world. Through the success of its imperialism, Europe remade the world politically in its own image of sovereign territorial states, diplomacy and international law. Decolonization left behind a world in Europe's image, in some places made quite well, and in other places badly. The English School has been much preoccupied with the consequences of expanding a culturally coherent European international society to a global scale that lacks a strong common culture to underpin it. It has told well the stories of how China, Japan, the Ottoman Empire and some other non-Western countries encountered European international society.

Historical Sociology is perhaps on the borders of IR theory. It has links to Marx, Weber and other classical Western sociological thinkers. Although some parts of its literature have taken on broad world-historical themes, notably Wallerstein (1974), Mann (1986) and Hobson (2004), the main focus of this literature is on the making of the Westphalian state, and thus, like the English School, it puts European history on centre stage. Some elements of historical sociology, most notably Tilly (1990) cut close to realism in their linkage of the state and war.

Critical Theory has roots in the idea that the point is not just to understand the world but to change it. Unlike the other progressive IR theories, Marxism and liberalism, which offer quite concrete visions of the ideal future, critical theory offers a general commitment against exclusionism and in favour of emancipation. Like other progressive theories it is universalist, but unlike them (and more in common with historical sociology) it seeks to understand each situation in its own terms. In one sense critical theory is an offshoot of the Western tradition of normative theory and the practice of promoting preferred (Western) values.

Constructivism and *Postmodernism* both have roots in Western philosophy of knowledge and social theory, building particularly on the work of modern European social theorists such as Bourdieu and Foucault. They set themselves up as alternatives to the materialist, positivist epistemologies underpinning realism and liberalism, seeing the social world as needing to be approached in its own terms as an intersubjective realm of shared understandings. Within that, constructivism is mainly a methodological approach, not carrying any necessary normative content of its own. Postmodernism tends to be more radical, seeking out and challenging the endlessly unfolding relationship between knowledge and power, rejecting metanarratives and the Enlightenment project, and seeing "truth" as a temporary social construction limited in time and space. Both constructivists and postmodernists see themselves as universalist in application of methods, but as particularist in seeing social structures as being limited in time and space, and so difficult or impossible to compare across time and space.

This brief survey shows not just the striking variety of Western IR theory, but also the great extent to which, despite its frequent universalist pretensions, it is rooted in European history and Western traditions of social theory and practice. A few flecks of non-Western thinking or actors are allowed in at various points, but mainly pretend to be universal in order to validate universalist claims. At the very least this West-centrism suggests it is possible for non-Western societies to build understandings of IR based on their own histories and social theories, and even to project these in the form of universalist claims.

NON-WESTERN CONTRIBUTIONS

There are some non-Western contributions that fit broadly within our understanding of IR theory, though these almost never meet the criteria for hard theory. Instead, they are more likely to fit within softer conceptions, focusing on the ideas and beliefs from classical and contemporary periods. Broadly, one could identify four major types of work that could be considered as soft theory. What follows is a brief examination of each.

First, in parallel with Western international theory's focus on key figures such as Thucydides, Hobbes, Machiavelli, Kant, etc., there are Asian classical traditions and the thinking of classical religious, political and military figures: e.g. Sun Tzu, Confucius and Kautilya, on all of which some secondary "political theory" type literature exists. Attempts to derive causal theories out of these do exist, but have been rare. An important aspect, though not necessarily limitation, of this type of work is that there is not always a clear demarcation between the boundaries of what is domestic and what is "international" relations. More important, invoking the ideas and approaches of these classical writers is seldom devoid of political considerations. In the heydays of the "East Asian Miracle" in the 1980s and early 1990s, for example, Confucian thought and ideas about communitarianism were frequently cited as the basis of an "Asian Values" perspective, which was offered by elites in the region as an alternative to Western individualist liberal values. It was also presented as the alternative conceptualization of an East Asian international order, which could challenge the hegemonic ambition of the liberal mantra of "democratic peace". In India, Vedic ideas about strategy and politics have been invoked as the justification of India's acquisition of nuclear weapons. This is by no means unexceptional, however, since the development of international relations theory often reflects real-world developments, but the invoking of Confucian and Vedic justification for a particular approach to international relations came at a time of growing wealth of power of certain nations: there has been no corresponding invoking of classical ideas to explain crisis or decline of nations in Asia.

A second category of work that might be called soft IR theory in Asia relates to the thinking and foreign policy approaches of Asian leaders such as Jawaharlal Nehru, Mao, Aung San of Myanmar, Jose Rizal of the Philippines and Sukarno of Indonesia. Although a good deal of their thinking may be sourced to training in the West or training in Western texts at home, they also came up with ideas and approaches independent of Western intellectual traditions that were a response to prevailing and changing local and global circumstances. One concrete example would be the idea of non-alignment, developed by Nehru and fellow Asian and African leaders in the 1950s, which though adapted from concepts of neutralism in the West, was in many respects an independent concept. Nehru also promoted the idea of non-exclusionary regionalism, as opposed to military blocs based on the classic European balance of power model. Aung San's ideas offered something that could be regarded as a liberal internationalist vision of international relations, stressing interdependence and multilateralism. Like Nehru, but focusing on both the security and economic arena, he rejected regional blocs that practice discrimination, such as economic blocs. In the 1960s, Sukarno developed and propagated ideas about international order, such as OLDEFOS and NEFOS ("old established forces" and "new emerging forces"), which drew upon his nationalist background as well as his quest for international leadership. Another example would be Mao's three worlds' theory, and his ideas about war and strategy. There is some parallel here with the influence of statesmen and generals in Western thinking about IR, foreign policy and strategy: e.g. Clausewitz, Bismarck, Metternich, Wilson and Lenin, in the case of whom it is hard to separate the intellectual contribution from praxis, and where theory always served immediate policy goals.

Unlike the case of these Western practitioners, however, not many scholars specializing in IR theory have taken up the challenge of interpreting and developing the writings of Asian leaders from the perspective of IR theory. The case of Nehru is especially interesting and relevant, because Nehru was recognized both within India and in the world, as a thinker in his own right, rather than simply as a political strategist. Moreover, unlike other political leaders of the day, Nehru did engage Western realist intellectual writings. Nehru attacked Walter Lippmann's prescription that the post-war world order should be organized around a number of alliances each under a great power orbit. The fact that India could be the putative leader of a future South Asian "Hindu-Muslim" bloc that Lippmann proposed did not impress Nehru. Such ideas about power politics were seen by Nehru as a "continuation of old tradition" of European power politics. Aung San also rejected military alliances under great power orbit; any "union or commonwealth or bloc" that Myanmar may be invited to participate in must not be "conceived in the narrow spirit of the classic balance of power". In short, for Nehru, some of the "realist" solutions to the world's problems ignored new forces sweeping the world, including the physical and economic decline of Western colonial powers after World War II, as well as the upsurge of nationalism and demands for freedom in the former colonies.

Despite their widely different backgrounds and circumstances, the ideas and approaches of Asia's nationalists shared some important common elements. First, they did not see any necessary conflict between nationalism and internationalism. This might have been driven partly by a desire to mobilize international support for national liberation. This "open nationalism" of Asia was in some respect distinct from the exclusionary and territorial nationalism of Europe. Though a Myanmar patriot and a staunch nationalist, Aung San saw no necessary conflict between nationalism, regionalism and internationalism. He believed that regional cooperation could compensate for Myanmar's weaknesses in the defence and economic sphere. The most important aspect of this nascent internationalism of Asia was the advocacy of Asian unity and regionalism. Nehru was the most articulate early post-war advocate of Asian unity, which he saw as the inevitable restoration of cultural and commercial links across Asia that had been violently disrupted by colonialism.

It is noteworthy that many of these figures self-consciously distanced themselves from utopianism or "idealism". In critiquing nationalism, Rabindranath Tagore dreaded the epithet of "unpractical" that could be flung against him and Aung San proclaimed: "I am an internationalist, but an internationalist who does not allow himself to be swept off the firm Earth". Similarly, in criticizing Lippmann's vision of great power orbits balancing each other and regional defence pacts such as the Southeast Asia Treaty Organization (SEATO) and the Central Treaty Organization (CENTO), Nehru defended himself against the charge levelled against him of being a "starry-eyed" idealist. Nehru derided the "so-called realistic appreciation of the world situation", expressed by Turkey, which defended regional pacts on the ground that they represented a more realistic response to the threat posed by communism than Nehru's idea of cooperation and "engagement" with China and the Soviet Union. Far from being a pacifist, Nehru claimed membership by the newly independent nations in such pacts represented a new form of Western dominance at a time when colonialism was in its final death throes, and which could lead to Europe-like tensions and conflicts in Asia and Africa.

A third type of work is done by non-Westerners who have applied Western theory to local contexts and to assess their relevance. For example, Muthiah Alagappa suggests that "Asia is fertile ground to debate, test, and develop many of these [Western] concepts and competing theories, and to counteract the ethnocentric bias", but considering their work as part of non-Western IR theory may be problematic: should this work have the same claim to be an authentic contribution to non-Western IR theory compared to work that makes independent generalizations from the Asian experience that might have transregional or even universal applicability? Will this merely reinforce the dominance of Western theory by relegating area knowledge to little more than provider of "raw data" to Western theory?

An alternative pathway may be found in a fourth type of work that studies Asian events and experiences and develops concepts that can be used as tools of analysis of more general patterns in international relations. Some of the finest examples of this include Benedict Anderson's "imagined communities" and James Scott's "every day forms of resistance", which have inspired scholars of comparative politics as well as international relations. Anthropologist Edmund Leach's *Political Systems of Highland Burma* is an example from another discipline that is now used to underscore fluid notions of ethnic identity in Southeast Asia and beyond. What distinguishes this type of work is that the scholars are not turning Asia into a mere test bed of Western social science theory, rather, they are identifying processes from Asian (and other local) settings that could be used to explain events and phenomena in the outside world. A key challenge for IR theory in Asia is to explore how "local knowledge" can be turned into definitive frameworks for analyzing global processes. Such type of work – in which Western local patterns have been turned into IR theory concepts – is commonplace in the West. For this reason, the Concert of Europe has been the basis for the literature on "security regimes"; the European Union is the main springboard of the entire theory of neoliberal institutionalism and the classical European balance-of-power system informs a good deal of theorizing about power transitions, alliance dynamics and "causes of war" literature. Hence, if European and North Atlantic regional politics could be turned into international relations theory, why not Asian regional politics?

One candidate for an indigenous theory is postcolonialism. There is now a discernable IR variant in which Indian scholars have played a prominent role, for example, Homi Bhaba on subaltern studies and Arjun Appadurai on globalization. They are rebelling against "orientalism" and challenge Western dominance by pointing to its odious outcomes. Gayatri Spivak criticized Foucault for treating "Europe as a self-enclosed and self-generating entity, by neglecting the central role of imperialism in the very making of Europe". Postcolonialism also seeks to dismantle relativism and binary distinctions found in postmodern theory, such as the distinction between First World–Third World, and North–South. These are useful contributions in the search for a non-Western IR theory, but postcolonialism cannot be regarded as an authentic attempt to counter Western-centrism, because it is basically framed within cultural discourses originating from the West. It is also noteworthy that postcolonialism has not attracted wide adherence in Asia from scholars outside of South Asia, certainly not in China.

INTERNATIONAL SYSTEMS IN WORLD HISTORY

REMAKING THE STUDY OF INTERNATIONAL RELATIONS

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INTRODUCTION

This book seeks to remake the study of international relations by viewing international systems from a world historical perspective. It is aimed both at students working in the field of International Relations (hereafter identified as IR), and at others interested in examining the social sciences and history in macro or holistic terms. In a sense it is a textbook, and written as such: for example we include an extensive glossary of the key terms used in the text (pp. 440–2). But it is not the usual type of textbook that presents how a discipline currently sees itself and introduces its subject matter to beginners. We want to use the marriage of theory and history to change what IR understands as its subject matter, and how it sees its mainstream theories relating to each other. Moreover, it is not just designed for an IR audience; we hope to attract interest and comment from historical sociologists, archaeologists, world historians, and anyone else trying to understand humankind as a whole. The book is designed to be accessible to anyone who has mastered a standard undergraduate course on international relations and our hope is that everyone who reads this book will find themselves looking at international relations from a more holistic, more integrated, and more historically contextualized perspective than the one they had before.

Perhaps the biggest difference between this and other IR texts is that instead of tracing the history of the contemporary international system back 350 years to the Treaty of Westphalia in 1648, the date conventionally used in mainstream IR to mark the origins of this system, we examine the whole history of the multiple international systems that have formed over a period of more than five millennia. Our starting point is 3500 BC when the Sumerian city-states began to interact in the area between the Tigris and the Euphrates that now forms part of modern Iraq. From our perspective, it was these city-states that constituted the first known fully-fledged international system. But we also find it both fruitful and necessary to investigate the pre-international systems that evolved during several tens of thousands of years before the rise of city-states. These systems not only provided the precursors for international systems, but existed alongside them right down to the twentieth century. Pre-international systems provide the backdrop to an era of five millennia when a range of very different international systems came into existence around the world. These international systems have been largely ignored within mainstream IR, in part because of the assumption fostered, especially in neorealism, that they can be accounted for by established theory. It is presupposed that there is little purpose to be served by investigating them, because although they provide additional historical depth, they give no extra theoretical purchase on the understanding of international systems. We fundamentally disagree with this position.

From our perspective, existing frameworks in IR are seriously crippled by their failure to build on a long view of history. And because mainstream IR theories are derived almost exclusively from the model of the Westphalian international system established in the seventeenth century, they inadvertently, but effectively, isolate the whole discourse of IR from the wider debates about world history. IR theorists have largely failed to follow the English school injunction that history requires 'the elucidation of the unlikeness between past and present' (Butterfield 1949: 10). On the contrary, to the extent that IR theorists have turned their attention to world history, they have been mainly impressed by how similar previous international systems have been to our own.This position reflects a long-standing tradition of thought in Europe. Nearly 200 years ago, Heeren (1819) produced a Manual charting the history of the European 'states-system'. In outlining his theoretical framework, Heeren observed how similar the European states-system was to the Greek and Italian city-states, as well as the Diadochi Empires, formed after the collapse of Alexander the Great's empire. It has taken a historical sociologist to observe, albeit controversially, that the emergence of international relations is 'coeval with the origins of nation-states' and thus at least to imply that Europe provides the first, and only, example of an international system (Giddens 1985: 4). Like Giddens, we think that there is something very distinctive about the international system that formed in Europe after 1500. But we disagree strongly that the modern European system is the only relevant case, and we want to paint our picture of international systems on a much broader historical canvas. In doing so, we eschew the route followed by Heeren, as well as many contemporary IR theorists, who focus exclusively on the limited number of international systems in world history that appear, superficially, to resemble the contemporary international system. We are intrigued by Wight's (1979: 24) remark that the 'political kaleidoscope of the Greek and Hellenistic ages looks modern to our eyes, while the immense majesty of the Roman peace, and the Christian unity of the medieval world, seem remote and alien'. Do the Greek and Hellenistic ages really look modern? And is the Roman peace and medieval world as 'remote and alien' as Wight makes out? Just as important, can we characterize these very different political arenas as international systems?

To develop a really effective world history of international systems it is necessary to rethink entirely how to approach the conceptualization of the international system. There are two problems with existing mainstream conceptualizations. First, because they are so closely modelled on the Westphalian international system, they are unable to capture the huge swaths of world history where international systems have taken a radically different form. Second, earlier international systems are only embraced by the Westphalian model because the conceptualization of what constitutes an international system is so narrowly

defined. Existing conceptualizations fail to expose important differences between the modern international system and its seemingly similar predecessors. A theoretical framework that can reveal how international systems have evolved across the entire spatial and temporal sweep of human history needs to be much more elaborate than anything that currently exists.

When one confronts the existing concepts of international systems with the task of providing a world historical narrative, it becomes apparent just how seriously underdeveloped they are. Setting up an enriched theoretical framework of the international system pays significant dividends. Although there are other theoretical frameworks available to examine world history, we argue that the framework unfolded in this book generates a version and a vision of world history that are more coherent and comprehensive than any of the rival world historical narratives that have emerged in recent years. We aim to prove that Westphalia-based IR theory is not only incapable of understanding premodern international systems, but also that its lack of historical perspective makes it unable to answer, or in many cases even address, the most important questions about the modern international system. And we argue that the historical narrowness of most IR thinking goes a long way towards explaining why IR debates have had so little impact on debates in the other social sciences and history. Our view is that IR has failed to occupy a proper role in the macro-debates of the social sciences and history, and indeed that most of the interest in world history that does exist in IR is the result of its successful colonization by the world systems school deriving from the work of Wallerstein. Our hope is to show how this disastrous underachievement might begin to be rectified. Our historical account thus propels us to challenge the most fundamental theoretical assumptions about international systems that are found in the contemporary study of IR.

The book is built on three basic premisses. The first is that none of the existing conceptualizations of the international system in IR can describe and analyse how international systems have emerged and evolved through the course of world history. The second is that the level of theoretical understanding in IR has been held back by a failure to examine international systems from a world historical perspective. The third premiss is that the international system constitutes the most effective unit for developing world history as well as for helping social scientists to advance a macro-analysis of social reality.

If these premisses hold water, then it is surprising that so few links have been forged between world historians and IR theorists. Until recently, mainstream IR theorists have shown virtually no interest in examining international relations from a world historical perspective, nor have world historians shown much inclination to use an international systems framework to analyse world history. So although our theoretical framework and our world historical account of international systems are designed to be of interest to the general reader, we also wish, more specifically, to address and encourage a closer collaboration between IR theorists and world historians.

One of the great strengths of world histories is that they identify when critical transformation points have occurred in the past. Our framework shows that there have been three significant turning points in the world history of international systems. These are associated with a conjunction of step-level changes in some of the key components that make up our theoretical framework (all the components will be reviewed briefly at the end of this introduction). The first transformation point occurred more than 40,000 years ago when hunter-gatherer bands first began to engage in a form of exchange that resulted in the long-distance movement of goods and ideas (Bar-Yosef 1998). These goods and ideas were transferred from one group to another over hundreds and sometimes thousands of miles. Large numbers of hunter-gatherer bands were in indirect contact with each other and we associate these long lines of indirect contact with the first formation of pre-international systems. Because these hunter-gatherer bands were responsible for global colonization, these systems eventually encompassed virtually every corner of the habitable world and their last vestiges can still be dimly discerned today.

A second turning point can be traced back to 5,500 years ago when the very first state-like units began to emerge and interact. The mutual interactions between sets of these units formed the basis of the first fully-fledged international systems. Over time, the major units embraced by these systems became increasingly diversified and they included agrarian empires, nomadic empires, chiefdoms, city-states, and city leagues. For the next 5,000 years, this diversity persisted and represented a key defining feature of the ancient and classical historical era. During this era, pre-international systems and international systems coexisted, but international systems expanded at the expense of pre-international ones. Nevertheless, even by the end of this epoch, pre-international systems still occupied large sections of the globe, and for the majority of this period they also continued to expand into increasingly inaccessible and previously uninhabited regions. When the international systems expanded, they sometimes came into contact with each other and, over time, these separate systems coalesced. But an important feature of this epoch was the way that the economic and cultural sectors of international systems expanded further than the political sectors, so that international systems frequently established economic and cultural links without making political or military contact. Despite evidence of systems coalescing, a large number of discrete international systems persisted throughout this epoch.

The third turning point identified by our framework took place as recently as 500 years ago and it is most closely associated with the emergence of a new kind of political actor, the modern sovereign state. These actors formed initially in Europe, but by the end of the twentieth century, this mode of political organization had extended across the entire globe. The units in pre-international systems

were effectively eliminated, and so too was the diversity of actors that had flourished during the previous epoch. The various international systems that had emerged and survived all around the globe over the previous 5,000 years coalesced remarkably swiftly to form a single international system that extended over the lands and seas of the planet. This extraordinarily rapid process was largely completed more than 150 years ago, giving birth to the fully global international system, which in turn gave birth to the self-conscious study of international relations. But history has not stopped. There are debates about whether the Westphalian state is giving way to a postmodern state, and about whether military-political relations are yielding pride of place to political-economic ones. These debates open the door to questions about whether the modern era is now drawing to a close.

On the basis of this short synopsis we can now say a little more about our three basic premisses. First, we need to justify our claim that existing frameworks are unable to accommodate a world history of international systems. The importance of the international system in IR thinking cannot be doubted. It represents one of the central concepts in the discipline; indeed it is so central that the term is often left undefined. Generally the international system is taken to be a shorthand way of referring to the nexus of actors and interactions that constitute the subject matter of international relations. It is this conception of the international system that promotes the view that IR constitutes an independent discipline.

At first sight, it seems self-evident that the international system represents an ideal vehicle for developing a world historical perspective. After all, in contemporary IR the international system constitutes a framework that makes it possible to understand how international relations cohere across time and space. Certainly it is taken for granted that the existing international system stretches across the contemporary globe and that its origins can be traced back for several centuries. So IR has habitually worked with the idea that the current international system has extended across space and persisted over time.

But a significant factor has worked against examining the international system from a world historical perspective. It is the deep-seated assumption in IR that the Westphalian system epitomizes the international system. The assumption necessarily gives a strong Eurocentric bias to the discipline. Europe, it is argued, gave birth to the modern sovereign state and so the international system defined in terms of sovereign states must be viewed as an exclusively European product. Any attempt to apply the concept to a previous era is deemed to be anachronistic. As mentioned above, a softer line accepts that it is not inappropriate to apply the term to earlier periods of history, and the Sumerian and Greek city-state systems are sometimes cited as examples of international systems. But even from this perspective, international systems only appear as relatively isolated episodes within the context of a much broader world history.

The image of the international system as an interstate system is now so deeply ingrained that the two concepts are treated as synonymous. A whole network of terminology has grown up to reinforce this usage. So multinational companies, for example, are identified as transnational rather than international actors. At the same time, the background assumption is that these transnational actors operate within the existing international (meaning interstate) system. We have become so inured to the terminology that it is disorienting for us to think of international systems except in interstate terms. So, although the international system has sometimes been identified in terms of all the transactions that take place across state boundaries, this conception still privileges states as the defining unit of the system.

If the idea of international systems is to be extended to world history with any chance of success, it is essential to break free from this association. We need, for example, to be able to identify empires as international systems. Conventionally, when we follow the history of the Roman Empire, what we observe is a city-state expanding into an established international system to form the Roman Empire—a large and complex form of state. From our perspective, however, the Roman Empire constitutes a phase in the longer story of a Mediterranean–Middle Eastern international system: a phase in which the system's political structure takes a hierarchical rather than an anarchical form. At a very minimum, this change of labels matters in metaphorical terms. Just as it made a difference during the Cold War whether one thought of the Soviet Union as a state or as an empire (and thus as a kind of submerged international system), so it makes a difference whether analysts have to think of the Roman Empire as a state or as an international system in hierarchical form.

But we want to go beyond metaphorical analysis. Viewing the Roman Empire and the Soviet Union as types of international system forces us to reassess what we mean by an international system and how an international system should be conceptualized. The focus on sovereign states is too limiting. The established conceptualization only permits an understanding of the Westphalian system, and even for that purpose it is flawed. The underdeveloped nature of the concept becomes more apparent when one looks more closely at either the origins of the Westphalian system or the question of whether the contemporary international system is undergoing a transformation. In both cases, the existing theoretical framework proves inadequate and there has been a search for new vocabulary and new analytical tools (Ruggie 1993).

But little progress has been made so far. There have even been attempts to understand the contemporary international system by comparing it to the medieval era. But the literature on neomedievalism sharply divides between those who associate the term with an emerging cosmopolitanism and others who link it to a coming anarchy. There can be no clearer indication that IR lacks the tools to think clearly about either the past or the future than these references to neomedievalism. The only obvious feature that binds the medieval and contemporary worlds

together is that neither can be accommodated within the Westphalian model. From our perspective, a more sophisticated framework is needed to see the similarities and differences between the three phases in the world history of international systems. Our world historical account suggests that although there is room for comparison, the future is going to look massively different from the historical epoch within which medieval Europe was located.

Our second premiss is that the development of theory in IR has been held back by its confinement to the Westphalian straitjacket. An IR-based world historical perspective challenges a range of theoretical assumptions that have become well established in IR. It therefore follows that the level of theoretical understanding in IR has been retarded by a failure to study international systems from a world historical perspective. For example, we dispute the widely held assumption, expressed with greatest clarity by the neorealists, that the shift from hierarchy to anarchy represents the most fundamental or deep structural political change that can be identified in world history. Our world history reveals a much more interesting picture. We can see, in the first instance, that anarchy and an absence of hierarchy persisted during the era of pre-international systems and again during the current 500-year epoch of the global international system. On the other hand, there were frequent and recurrent moves from anarchy to hierarchy in all of the international systems that formed during the 5,000-year period after international systems first came into existence. Our account of world history shows that it is change in the structure of the dominant units, not the move from hierarchy to anarchy, that represents the most fundamental, era-defining type of transformation in international systems. It can even be hypothesized within our framework that it is the nature of the dominant units that determines the propensity for, and (in)stability of, anarchic or hierarchic international system structures. The pre-international epoch was defined by interaction among hunter-gather bands, whereas the first international epoch was characterized by a whole host of structurally differentiated state-like units. Then, with the emergence of the modern sovereign state, all prior forms of units were effectively eliminated, marking the start of a second international epoch. It follows that we are also disputing the neorealist claim that anarchic systems are characterized by 'like units' that possess a common structure. Again, a comparison of the three epochs reveals the problem with the 'like unit' thesis. Although it holds for the first and third epochs, its universality is undermined by the intervening epoch that lasted for 5,000 years.

A further illustration of the problematic character of contemporary IR theory relates to the importance attached to polarity in IR theory in general and neorealism in particular. From a world historical perspective, the prolonged debate between advocates of the balance of power as opposed to hegemonic stability theory fails to take on board that it is a debate that only has relevance for the last 500 years of world history. The debate would have to be cast in very different terms if the previous two epochs were taken into account. As the details of our world history unfold, therefore, it becomes increasingly apparent just how many central IR assumptions are built on an understanding of the last 500 years and how many of them prove to be unfounded when a world historical perspective is adopted.

Our third premiss is that the international system constitutes the most effective unit for developing world history as well as for helping social scientists to advance a macro-analysis of social reality. World history, after falling out of fashion for some considerable time, has now undergone a significant revival, generating a renewed interest in the provision of overarching frameworks that can be used to trace the history of human beings across time and space. There is a desire to escape from the confinement imposed by histories written from within a specific time period or from a particular national or even continental perspective. Breaking loose from the confines imposed by these familiar and more parochial accounts of the past is not easy; and, as a consequence, world historians are engaged in a major debate about the kind of frameworks that can most usefully be employed to promote a world historical perspective. So far, IR has played little or no part in this debate. But if our assessment is correct, then the concept of the international system should be promoted as a framework for studying world history.

Sanderson (1995) claims that there are two leading approaches to world history. The first, now most closely associated with William McNeill (1991), but initially popularized earlier in the twentieth century by Spengler and Toynbee, uses the idea of a civilization as the central unit of analysis. Within the discipline of history, this is the dominant approach (Manning 1996: 777). The other, linked primarily to Immanuel Wallerstein (1974), focuses on world systems, and is rooted in historical sociology. Although they generate clear transformation points neither of these frameworks is as 'thick' as our conception of the international system. We accept that civilizations represent important units in world history. But there has been no attempt to identify the key processes and structures that define a civilization. Even more important, although McNeill recognizes the world historical importance of the mounted nomads who lived beyond the boundaries of the civilizations, he fails to embrace them systematically within his framework. Recently, McNeill (1998) has acknowledged the weaknesses in the civilizational framework, and has argued that a better framework is provided by what we call interaction capacity. What we show, however, is that although interaction capacity does play a crucial role in world history, its theoretical significance only becomes really apparent when examined in conjunction with the elements of process and structure.

The impact of Wallerstein's theory of world systems on world history and across the social sciences is indicated by the enormous literature it has inspired. He certainly does identify the basic economic and political structures and processes that define his world systems. Indeed, we would accept that he has developed a very powerful theoretical model. But we do not accept his argument that the military-political sector of a world system can be regarded as epiphenomenal and therefore ignored. From our perspective, the costs of eliminating this sector are very high. Another crucial problem with Wallerstein's framework is that he ignores the significance of interaction capacity. Moreover, his analysis only covers certain sections of the globe, leaving other sections as large 'black holes' (Adas 1998: 86). Finally, although Wallerstein does make reference to earlier periods, he is overwhelmingly concerned with the type of system he designates as a 'world economy' that emerged after AD 1500. It seems to us that both of the leading approaches in world history have produced theoretical frameworks that are relatively thin in comparison to the one that we formulate below.

~~Given how extensively Wallerstein's model has been adopted by analysts~~ looking at the premodern world, despite being designed specifically to examine the post-1500 era, the preoccupation of IR theorists with a Westphalian model of the international system is probably insufficient to explain why world historians and others have failed to draw on these models. The truth is that attempts in IR to theorize about the international system have failed to resonate with world historians. Beyond the confines of IR, the international system is not widely regarded as an inherently useful or illuminating concept. For most people, the idea of the international system is opaque, conveying little meaning. ~~Although theorists in IR have come up~~ with a series of ill-assorted metaphors, from billiard balls and cobwebs to octopuses and egg boxes, designed to illuminate the nature of the contemporary global system, these metaphors have signally failed to penetrate beyond the boundaries of the discipline. The international system remains a shadowy, unfamiliar concept which has neither become part of popular parlance nor entered the general vocabulary of other social sciences.

Because there is a growing interest across the social sciences in fostering macro-approaches to analysis, this general lack of interest in international systems remains surprising. Anthropologists, sociologists, and geographers, as well as archaeologists, are acknowledging that they have devoted too much time in the past to observing what goes on within social units, and too little time to investigating the relations between these units. There has been a search for frameworks that will allow them to get a handle on macro-analysis. The framework that has been most extensively resorted to is Wallerstein's world systems. Almost all the attention has been centred on his conception of a world economy, and despite his injunction that no world economies existed before AD 1500, the framework has been extended all the way back to the fourth millennium BC.

There is one area of literature, however, where reference to international systems has been made. As well as developing a wider spatial perspective on social systems, sociologists have also become increasingly interested in extending the temporal reach of their analysis and there has been an upsurge of interest in historical sociology. Here, contact has been made with international relations. Skocpol (1979), Tilly (1985), and Mann (1986) all draw on the idea that the anarchic character of the relations between states helps to account for the persistence of war. Although extremely illuminating in many ways, from the perspective of the international system, the work of these historical sociologists simply reinforces the long-established realist view in IR that the essential features of international politics are enduring and unchanging. None of them, not even Mann, who adopts a very long world historical perspective, makes any move to historicize the idea of the international system.

It is unreasonable to expect historical sociologists to carry out a task that falls squarely in the court of IR theory. To make progress, we believe that IR theorists have to join hands with all of those attempting to understand the macro, systems side of the human social world, whether they be historians, sociologists, political geographers, economists, or archaeologists. Theory and history may sometimes make strange bedfellows, but as the world systems school has demonstrated, the fruits of their union can be powerful and compelling in a way that neither of them can be when taken alone. From our perspective, the problem with IR theory is that it has treated the international system as standing outside history, and has then used history to reinforce this ahistorical assumption. Neorealists are particularly guilty of this fault. There is little point in turning to history with a framework that is incapable of exposing evidence of change. What we attempt to do in this book is to develop a theoretical framework that makes it possible to identify faultlines in history when the fundamental features of international systems have been transformed.

In *The Logic of Anarchy*, to which this book can be viewed as a sequel, we argue that despite the persistent criticism that has been levelled at neorealism, we nevertheless saw this approach as a useful 'foundation on which to construct a more solid and wider ranging Structural Realism' (Buzan et al. 1993). We employ a similar strategy in this book. We have deliberately favoured methodological pluralism over methodological monism. Many elements of both structure and realism are still prominent, but there is much else besides. The range and diversity of our borrowing also forbids calling the result a theory, for it contains no single line of cause and effect. Our position is that phenomena as massive and complex as international systems cannot be understood by any single method. The first task for international systems theorists *must* therefore be to show how existing theories stand in relation to each other. To say that they are simply different, or opposed, or mutually exclusive is not adequate. The job is to differentiate them in such a way as to expose their complementarities, to make clear how static and

Chapter 1

SYSTEMS, HISTORY, THEORY, AND THE STUDY OF INTERNATIONAL RELATIONS

One central aim of this book is to provide a new way of thinking about international systems. Although there has been extensive analysis of the international system in mainstream IR, the concept remains deeply contested. Indeed, it is possible to argue that the most important methodological and theoretical debates that took place in IR during the second half of the twentieth century were all centred on attempts to identify the most appropriate way to conceptualize and then analyse the international system.

These debates do tell us a good deal about why it is so difficult to conceptualize the international system. But they do not tell us how to develop an account of international systems from a world historical perspective. Mainstream conceptualizations of international systems in IR remain 'thin' and unidimensional, unable to assist in the task of telling the full story that we think needs to be told. Although parsimony in theorizing is a virtue, we argue that without a 'thicker' form of theorizing, a complex phenomenon like international systems simply cannot be adequately understood.

In the first section of this chapter we spell out some prevailing characteristics of the discipline that have prevented IR scholars from developing a 'thick' conception of the international system. Then in section 2 we examine the historiography of the discipline to see why these factors have been promoted and suggest that some of the inadequacies in the concept of the international system employed in mainstream IR can be attributed, at least in part, to the Americanization of IR that went on during the course of the twentieth century. The English school has developed along different lines, which can potentially give rise to a richer and more historicized conception of the international system. Finally, we show how world historical approaches transcend the history/theory divide that has had the effect of holding back the task of theorizing in IR.

THE UNDERDEVELOPED CONCEPTION OF THE INTERNATIONAL SYSTEM

Despite more than a century of intensive discussion about the nature of the international system, it is difficult to deny how underdeveloped the concept continues to be. Even the more sophisticated accounts of the international system fail to address some of the most elementary questions. Waltz (1979: 91), for example, talks about the international political system in terms of independent units coacting, but he does not specify how much interaction, or what type, is necessary for a system to exist. Will *any* interaction suffice, or must we identify a boundary (or perhaps boundaries) defined by levels, types, and frequencies of interaction? On one side of this boundary will be international systems, and on the other will be sets of lightly interacting parts not yet defined as international systems. Neorealism suggests (without specifically addressing the question) that an international system does not come into being until quite high levels of (strategic) interaction exist. On this basis Waltz's position is contradictory, because an international system does not necessarily, or even probably, form from the first point at which units begin to co-act.

This line of thinking points towards some interesting questions. Exactly what are the criteria for specifying that an international system exists? Is it useful or necessary to conceive of different types of international system—strategic, economic, cultural—in order to register the significance of different types of interaction? How far back in time can we apply the idea of international system? What does the history of the international system look like, and are there patterns in its development? When can we say that a fully global international system came into being? Existing research does not suggest obvious or uncontroversial answers to any of these questions, and before proceeding to suggest some possibilities it is worth considering why this is so. Why is it that such extremely basic questions about what is arguably the core concept in the discipline remain not only unaddressed, but almost unasked in IR? At least five complementary lines of explanation suggest themselves: presentism, ahistoricism, Eurocentrism, anarchophilia, and state-centrism.

PRESENTISM

The discipline of IR has been mainly focused on contemporary history and current policy issues. The fast-moving nature of the subject, and the pressing demand for expertise on current events, encourage a forward- rather than a backward-looking perspective. Consequently rather few specialists within the discipline have had either a broad historical knowledge or an interest in acquiring it.

Occasionally, authors will raid further back and further afield, but these forays are usually guided more by the search for particular parallels with the modern European experience than by any interest in capturing the character of the international system in history overall (Holsti 1967: 2; Watson 1992; Wight 1977). Following Burke (1993: p. xi), we refer here to this perspective as presentism, or chronocentrism (Powelson 1994), which suggests that the dictum about using the past to understand the present is reversed. As a consequence, the few historical times and places that resemble the international anarchy of modern Europe get a disproportionate amount of attention, most notably classical Greece, Renaissance Italy, the 'warring states' period in China during several hundred years of the first millennium BC, and, to a lesser extent, 'warring state' periods in South Asia. Because these attempts to break away from presentism impose the present on the past, they reinforce the problem of ahistoricism in the analysis of the international system.

AHISTORICISM

Ahistoricism does not imply that the past is of no concern to social scientists, but rather that they should be searching for general laws that apply to the past as well as the present. Such a goal is dictated by the desire to emulate the invariant laws of natural science that hold across time and space. Social scientists of a positivist predisposition, anxious to emulate the natural sciences, also seek to identify laws that are immune to historical variation.

In most areas of social science there has been a persistent debate about the relative merits of ahistoricism and historicism. In anthropology and archaeology, there has been a profound disagreement between formalists who subscribe to an ahistorical position and substantivists who subscribe to a historicist position. The former insist that concepts like trade and profit are universal and can be applied in any time and place. The latter insist that these concepts are meaningless when applied to tribal societies where it is not possible to identify a distinct economic system. In such a setting, they insist, the practices that define the existence of an economic system have no concrete reality. Identifying the exchange of goods in such a setting as trade misunderstands the nature of the transaction.

Debates of this kind have rarely taken place in IR where until recently it was widely accepted that the 'texture' of international politics did not change over time, because 'patterns recur, and events repeat themselves endlessly' (Waltz 1979: 66). Twentieth-century realists have assumed that the balance of power provides the basis for a transhistorical theory that accounts just as well for behaviour in the Greek city-states as it did for relations between the Soviet Union and the United States. In the last quarter of the twentieth century the ahistoricism of realism has come under increasing criticism. The easy assumption that we can compare the conflict between Athens and Sparta with the conflict between the United States and the Soviet Union rests, it is argued, on a 'gigantic optical illusion' (Rosenberg 1994: 90). The comparison requires the analyst to distort beyond recognition the underlying social structures that form the Greek city-states. Similar criticisms have been levelled at attempts to apply realist thinking to the feudal era

EUROCENTRISM

Eurocentrism has bedevilled every aspect of the social sciences, and it is hardly surprising that it has had an impact on IR. At first sight, it might appear that there is nothing untoward about the familiar Eurocentric account of how the contemporary international system emerged. It seems to be almost self-evidently true that Europeans created the first global international system by bringing all parts of humankind into regular economic and strategic contact with each other. They occupied whole continents and stamped upon them a system of territorial boundaries, trading economies, and colonial administrations. The few places that they did not reduce to colonial status (Japan, Siam, Persia, Turkey, China) were forced to adapt to European models in order to preserve themselves. But as with ahistoricism, this story can only be told in this way by ignoring or distorting great swaths of the past. In particular, as writers like Hodgson (1993) have demonstrated, Eurocentric accounts invariably ignore the Afro-Eurasian system that existed long before the Europeans began to extend across the globe.

Rather than tracing the origins of Europe, we argue that it is this much wider history that constitutes the real antecedent of the contemporary global international system. Indeed, one can only explore the origins and significance of the idea of international system, and fully understand what is happening to it now, by comprehending its non-European dimension. Such comprehension requires more than merely selecting the handful of times and locations from the ancient and classical era during which anarchic structures similar to modern Europe's briefly held sway. It means addressing the whole sweep of ancient and classical history in terms of international system, and asking just what kind of system(s), if any, existed before the Europeans subordinated everything to their own anarchic model. Only by following this course can one bring the historical record to bear on the question of what are the necessary and sufficient conditions for an international system to come into being.

Eurocentrism is closely related to the idea of Orientalism. According to Said (1995; Sardar et al. 1993), the conception of European culture and identity gained in strength from the eighteenth century onwards by being contrasted with the Orient. Europe was seen to be outward looking, dynamic, and progressive whereas the Orient was depicted as inward looking, stagnant, and decadent. Marx, for example, contrasted the dynamism of capitalism with the static Asiatic mode of production. By the same token, the vibrant European system of states was not

seen to have any counterpart in the Orient. Said insists that Occidental students of the Orient promoted an image of the East that helped the Europeans to see themselves as inherently superior to the rest of the world. The Orientalist thesis has probably been exaggerated (MacKenzie 1995), but there is no doubt that IR has been studied from a very Eurocentric perspective with a concomitant failure to come to terms with how non-European 'others' understood international relations or organized their world.

ANARCHOPHILIA

The fourth reason why basic questions about the core concept in the discipline remain not only unaddressed but almost unasked is anarchophilia, which is very much a consequence of ahistoric and Eurocentric perceptions. This normative assumption is strongest in neorealism. Classical realists have often expressed more mixed feelings about the virtues of anarchy, and liberals have tended to see it as the main cause of war and disorder.

Adam Watson (1992, 1997) has opened an attack on anarchophilia, arguing that much of the international history of the last 5,000 years has not been anarchic, but has ranged across a spectrum with anarchy at one end, empire at the other, and hegemony, suzerainty, and dominion in between. Moreover, he argues that both anarchy and empire are extreme conditions, the natural instabilities of which tend to push the norm into the middle ranges of the spectrum. It is not easy to break free from the grip of anarchophilia because we are preconditioned to think of the international system in anarchic terms. Other disciplines are not so constrained. Historians like Gallagher and Robinson (1953), for example, find it appropriate to depict the links between Britain and Latin America in the first part of the nineteenth century in terms of an informal empire. As a consequence, it might be easier to tell the story of the British Empire, or the Soviet empire, for that matter, if they are identified as regional international systems rather than as states in the international system. Watson's framework raises the possibility that even the most abstract and successful theoretical development in the discipline has been profoundly, and probably unwittingly, shaped by an undue reliance on the peculiarities of the European and contemporary world experience.

STATE-CENTRISM

Although almost inseparable from anarchophilia, state-centrism (or politicophilia) is a distinct reason for the underdeveloped conceptualization of the international system. There has, of course, been extensive attention paid to the economic, social, and environmental dimensions of international relations within the discipline. But attempts to conceptualize the international system have focused overwhelmingly on the military-political dimension. Perhaps even more important, politics has been linked almost indissolubly with the state. This is perhaps not that surprising after the Second World War, an era when the idea of the political system became little more than a synonym for the state (Easton 1953). A previous generation of pluralists in both Britain and the United States had endeavoured to dispense with the idea of the state when analysing politics (Little 1991, 1996). At the beginning of the twentieth century, pluralists began to argue that links between financial centres around the world were now closer than cities within the state had been in the past (Angell, 1912: p. viii). The state was characterized as a 'metaphysical spook' by the pluralists and during the inter-war era there were tentative attempts to analyse international relations from a non-state pluralist perspective (Fox 1975; Wilde 1991). It seemed possible in the 1960s and 1970s that the pluralist perspective would be resurrected and that a multi-centric and multi-layered image of the international system might be developed (Burton 1968; Keohane and Nye 1973). But the putative pluralists very rapidly drew back and refocused their attention on the state (Keohane and Nye 1977; Keohane 1984). Although Rosenau (1990) advocated the need to combine the pluralist and realist images of the international system, others have failed to follow this route and the most sophisticated attempts to conceptualize the international system have been restricted to the state-centric perspective.

THE ALTERNATIVE

Within mainstream IR the concept of international system has almost invariably been depicted in one-dimensional terms. The resulting assessment is necessarily partial and there is generally no acknowledgement that a more comprehensive approach to the task of conceptualizing such a complex phenomenon is required. The underdeveloped concept of the international system has acted as a Procrustean straitjacket on the discipline. We hope to transcend the weaknesses discussed in this section by developing a very open-ended approach to international system which does not prejudge the nature of the dominant units in the system, privilege one sector of activity over another (for example, politics over economics), or give precedence to one mode of explanation over another (for example, structure over process). To achieve these objectives we need to draw extensively on both history and theory. But first, it is worth exploring briefly how this one-dimensional conception of the international system has come to prevail in IR.

THE HISTORIOGRAPHY OF INTERNATIONAL RELATIONS

During the last decade of the twentieth century, in the new post-Cold War era, IR underwent a large number of re-evaluations by scholars who were disturbed by the discipline's failure to forecast this ostensibly significant event. The putative weaknesses in the contemporary study of IR were frequently attributed to the dominance of realism in the era after the Second World War.

Can the failure to develop an adequate conceptualization of the international system also be laid at the same door? A plausible case can certainly be made that realism exemplifies all of the features identified in the previous section. Realists are preoccupied with applying their 'timeless' understanding of international politics to the exigencies of the contemporary international system. The focus on the competition between states as an inherent feature of the anarchic international system is the hallmark of realism. So ahistoricism does seem to be linked to state-centrism and presentism in realism. But realists are also drawn to anarchophilia and Eurocentrism: statesmen in the eighteenth and nineteenth centuries are viewed as having perfected statecraft and thereby stabilized the anarchic structure of the European international system. All five features discussed above have unequivocally been attributed to the work of the classical realists who dominated IR after the Second World War; and they are also present in the work of the neorealists who came to the fore in the 1980s.

Realism, it is often argued, was introduced into American IR by Europeans, like Hans J. Morgenthau, who came to the United States to escape the dangers that beset Europe in the 1930s. These key intellectual figures moved the basic tenets of realism to the centre of the academic stage in the United States (Guzzini 1998). Realist thinking made perfect sense during the ensuing Cold War; or certainly more sense than the approach of the inter-war idealists who had put their faith in international law and international organizations being able to override the long-standing imperatives of power politics. Problems with the state-centricism of realism, and its preoccupation with power and conflict, were brought to the surface briefly in the 1970s. But when relations between the Soviet Union and the United States deteriorated once more at the end of the 1970s, after the failure of détente, neorealists were able to move back to centre stage, where they stayed until the end of the Cold War. The neorealists distilled the essence of realist thought and then laced it with a large dose of scientific positivism. The scientific veneer, critics argued, blinded many to the deep-seated flaws of realism, and it also helped to deepen and widen the influence of the disabling factors discussed in the previous section.

SYSTEMS AND THEORY IN AMERICAN IR

The recognition that American IR represents a long-standing sub-field in Political Science has some important consequences. One is that it helps to account for the high level of uncertainty that exists about the status of IR as an academic discipline in the United States. In a major survey conducted in the middle of the twentieth century (Manning 1954), Harold Sprout, an important figure in the development of the subject, raised doubts about its 'inherent pedagogic virtue'.

Twenty years later, William Fox (1970: 29) could still identify a sense of 'inferiority' amongst IR scholars when their discipline was compared with other social sciences. By the end of the century, references were still being made, inside and outside the discipline to the 'feebleness' and 'triviality' of theorizing in IR (Ryan 1998; Walker 1988). On the face of it, such concerns are surprising. After all, during the second half of the twentieth century the subject substantially came into its own. It had developed a wide range of well-regarded textbooks, and formed its own professional associations. Journals devoted to this area of knowledge steadily expanded in numbers. Nevertheless, as we enter a new century, with the study of international relations being taught and researched in a growing number of countries across the globe, the American discipline of IR still seems to lack the status accorded to Economics, Anthropology, Sociology, and Political Science.

The sub-field status of IR also accounts for the anomalous role played by the idea of international system in American IR. The term came into increasing prominence after the Second World War when the idea of 'system' was seen in Political Science to play an important role in defining the disciplinary boundaries of all the social sciences. David Easton, for example, insisted that it was essential to be able to locate Political Science on the 'general map of social science' and that 'the idea of a political *system* proves to be an appropriate and indeed unavoidable starting point' (1953: 96). Other social sciences such as Economics, Sociology, and Anthropology were demarcated in similar fashion.

System thinking, however, was not only associated with the establishment of disciplinary boundaries, it was also closely linked to the 'behavioural' or 'positivist' turn which sought to ensure that the methodological rigour and technique of the natural sciences prevailed in the social ones. The attraction to general system theory was part of that process. David Easton's work on the political system epitomized this 'scientific' orientation. Easton wanted to use the idea of system to identify the recurrent relationships that open up a route to the formulation of theory about how the 'real world' political system behaves. The emphasis on systems thinking in American IR during the second half of the twentieth century is also associated with this desire to develop the discipline along scientific lines. The theorists most closely associated with the goal of turning IR into a science, such as Kaplan (1957), Singer (1961), and Waltz (1979), all focused on the idea of the international system and all identified the existence of systems by reference to recurrent patterns of behaviour. The desire to establish IR as a scientific discipline undoubtedly encouraged an ahistorical approach to analysis.

The promotion of a theoretical and scientific understanding of international relations was also associated with an attempt to break free from History. The behaviouralists believed that historians present the past as a unique series of events. It follows that their narrative accounts of the past cannot usefully be employed to develop a theoretical or scientific understanding of international relations (McClelland 1958; Morgenthau 1970: 67). After the Second World War, the

prevalence of this view throughout the social sciences led Meehan (1968: 109) to identify the emergence of a 'generation of social scientists with little knowledge and even less interest in history'. In IR, McClelland (1958), a widely acknowledged IR theorist, advocated a break from what he saw as the 'dead hand' of the historian that was holding back the development of the discipline. Towards the end of the twentieth century, although the importance of historical understanding was starting to be more widely appreciated throughout the social sciences, Richardson (1988: 316) could still observe 'the artificial separation between the disciplines of international history and international relations'. Ahistoricism and presentism have persisted as defining features of American IR.

There is little doubt that the status of American IR as an independent discipline was fostered by the onset of the Cold War and the emergence of the United States as a superpower. Hoffmann (1977) argues that it was at this juncture that IR emerged as an 'American Social Science'. An informal division of labour began to be fostered with Political Science examining what went on within the state and IR exploring what went on without (Easton 1981). This division reinforced the inside/outside image of the state that pragmatism and pluralism had begun to question during the first half of the century. Their ideas were completely lost sight of in American Political Science and it was nearly a quarter of a century before American IR began to point to the very complex transnational links forming among domestic actors operating within different states. These links established a much deeper conception of the international system, one that extended down into the hierarchical political structure of the state. They also crossed the boundaries between states and questioned the idea that the international system was essentially political in character. If the underlying premiss of general systems theory is accepted, that everything is linked to everything else, then it is not difficult to arrive at the point where the international system can be seen, in principle, to embrace everything that is going on in the world. The idea starts to encapsulate the breadth and depth of all human activity, pushing IR to become the study of humankind as a whole.

The changing metaphors used to depict the international system during this period neatly capture this transformation. For twenty years after the end of the Second World War, IR theorists were quite happy to think of the constitution of the international system in terms of states as billiard balls bouncing off each other (Wolfers 1962: 19). This metaphor is closely associated with the realist approach. But by the mid-1960s, a very different image of the international system was being advanced, depicting international relations in terms of cobwebs being constantly spun across the globe on many different levels (Burton 1968, 1972). This metaphor supported a pluralist approach. For the last three decades of the twentieth century there was a persistent debate between realists who insisted that the state continued to represent the dominant actor in an international system conceptualized in essentially political terms, and pluralists who argued that growing transnational ties, economic and social, as well as political, were eliminating the significance of the state and transforming the nature of the international system.

As the pluralists required the idea of the international system to embrace ever larger amounts of activity, there was a serious danger of the concept imploding. Certainly the original objective of locating the international system at the centre of the attempt to establish IR as a coherent and independent discipline within the social sciences was being undermined, though at the same time opening the possibility (so far not successfully pursued) for IR to become a kind of meta-discipline colonizing the macro-side of the other social sciences. Retaining a state-centric view of the international system helped to preserve the idea of IR as an independent discipline. Adopting a more pluralist approach to the international system not only undermined the coherence of IR, but also encouraged the boundaries of other disciplines to fray at the edges. All social sciences have endeavoured to maintain their independence by presupposing that the systems they deal with are effectively closed.

If this account is correct then it is much too simple to suggest that we can look to the reputed dominance of realism in American IR to account for the underdeveloped conceptualization of the international system. Realism was counterpointed by forms of pluralism that potentially offered a much richer view. Further light can be shed on the issue by looking at the role of system thinking in the English school.

SYSTEMS, HISTORY, AND THE ENGLISH SCHOOL

Schmidt's historiography of the origins of American IR has no British counterpart. But there is a growing interest in the historiography of the English school (Dunne 1998). The school, founded in the late 1950s, was made up of historians, IR theorists, and practitioners and this mix undoubtedly helps to account for their distinctive approach to international relations. Thinking in terms of systems was very important to the English school, but the approach diverged sharply from the one adopted in American IR. In the first instance, it might seem that the difference is just one of terminology, with the English school referring to a 'states-system' rather than the more familiar and recently established idea of an international system. In the United States, however, 'states-system' cannot be used without confusingly overlapping with discussions of US domestic politics. We have now become so accustomed to thinking of the international system as a system of states that the term 'states-system' has an archaic ring to it. But it is this resonance with the past that drew the English school to the concept. The term can be traced back to the end of the seventeenth century, when the German jurist, Pufendorf, defined a 'states-system' as 'several states that are so connected as to constitute one body but whose members retain sovereignty' (cited in Wight 1977: 21). The English school were drawn to the term because it was used in European diplomacy. For a states-system to exist, statesmen have to acknowledge its existence. ~~Members of the English school were also impressed with the use made of the~~ concept by the Göttingen school of historians at the start of the nineteenth century. Some of the key ideas developed by the English school are foreshadowed, for example, in the historical manual first produced by Heeren (1857), after the Napoleonic Wars.

Heeren acknowledged that there had been several states-systems in the past but insisted that the European states-system was the most sophisticated. Although he acknowledged that a primitive form of the balance of power existed in the Greek and Italian city-states-systems, he stressed that none of the earlier states-systems had achieved the maturity of the European states-system. The English school adopted exactly the same kind of argument, suggesting that the complexity of any states-system was determined by the complexity of the ideas and institutions developed by the states in the system. They argued that the balance of power, diplomacy, international law, and sovereignty were all ideas that had only really flourished in the European states-system.

In contrast to mainstream American IR, the English school were simply not constrained by the notion that the study of history and the development of theory are incompatible. They recognized that many historians do observe patterns in history and assume that it is possible to understand why these patterns occur. Butterfield, in particular, was very aware that historians are prone to work on the assumption that there are some large patterns at work in history. He was particularly concerned about what he called *The Whig Interpretation*, by which he meant the tendency of historians to interpret the past in terms of the present. Fukuyama's (1992, 1998) 'end of history' thesis can be viewed in this light. By contrast, Butterfield (1949: 10) believed that the historian is required to reveal how unlike the past is to the present. The English school eschewed ahistoricism and assumed that there were significant differences in the patterns of behaviour that developed in different states-systems. They wanted to be able to show, for example, that behaviour in the Greek city-states system and the Italian city-states system was different from behaviour in the European states-system because the Greeks and the Italians had failed to develop the idea of a balance of power (Wight 1977; Butterfield 1966). For members of the English school, understanding of international systems can most effectively be advanced on the basis of historical and comparative method.

It is, however, the English school view of theory rather than the emphasis on history that seems to pull the English school apart from the American IR approach to systems. The English school presuppose that to understand the patterns of behaviour that emerge in a system, it is necessary to understand the cultural ideas that underpin the actions of the states that are operating in the system. By contrast, in American IR, there is a deeply entrenched belief amongst systems thinkers that to comprehend the behaviour of international systems it is necessary to go beyond the understanding of the international actors. Singer believes that the social scientist can identify systemic patterns of which the international actors may be unaware. By the same token, Waltz insists that a balance of power emerges as an unintended consequence of state action and such a pattern will form in an anarchic arena whether or not states are aware of the idea. We examine this position in more detail in Chapter 2. But it is worth noting here that Bull, in particular, was sensitive to the pitfalls of pushing a historicist line of argument too far. He suggested that 'we are not sufficiently flexible in our idea of what a "system" is. Morton Kaplan regards an international system as a "system of action". You can take any area and look for the pattern of the relations between the states in that area. It will form some sort of "system". There is no need to posit even any consciousness of system amongst the states involved' (Dunne 1998: 125). What is most interesting about this assessment is that the English school was aware of the divergent American position and, as we will see in the next chapter, endeavoured to take it into account.

The English school avoids Eurocentrism, ahistoricism, presentism, and, in the more recent work of Watson (1992) discussed above, anarchophilia and state-centrism. It is unsurprising, therefore, that we draw heavily on their ideas in our framework. But although the English school help to show the way, there has been a failure to pull their eclectic ideas together in a systematic fashion. Moreover, although their systemic approach produces a thicker conception of the international system than anything found in mainstream American IR, it is still not nearly thick enough to reveal how the international system has evolved across world history. The work of world historians throws an indirect light on the history of international systems, but since they have not used the concept their accounts of it are at best partial and inferred.

The idea that historians simply string together unique events into a storyline underestimates the complexity of how narratives work. It also fails to accommodate the diversity of approaches that can be observed among historians. They vary, first, in terms of the length of time that they focus on, and on this basis can be divided into 'lumpers' and 'splitters' (Hexter 1979). The 'splitters' are interested in developing ever more detailed pictures of increasingly narrow slices of time, and the 'lumpers' want to provide pictures of ever wider chunks of time. At the extreme end of this spectrum lie the world historians who wish to create a picture of the entire world from the beginning of time to the present day. As we will discuss in Chapter 3, some historians are deeply sceptical of such an approach. But McNeill (1986: 71) notes approvingly that 'World history was once taken for granted as the only sensible basis for understanding the past'. Rashid al-din Tabib,

a court historian in Tabriz, for example, produced a world history at the start of the fourteenth century that embraced all of Eurasia (Rice 1976).

World historians generally presuppose that they do not proceed differently from any other type of historian. But perhaps the best-known approach to 'lumping', the French *annales* school, most closely associated with the work of Braudel, have argued more self-consciously that history is not simply concerned with surface *events* that are subject to rapid change over time, but must also take account of *processes* that must be observed over longer periods of time, and of *structures* that only become apparent over the *longue durée*. Similar ideas are implicit in the work of McNeill, Toynbee, and others who have told world history as the story of civilizations. The French school's ideas have had a very extensive impact. Certainly it is no longer controversial to argue that the divide between history and social science looks 'increasingly quaint, contrived and unnecessary' (Abrams 1982: 1).

But we can also find among this wave of world historians a self-conscious attempt to distance themselves from the work of those social scientists who have tried to break down the barriers separating Social Science and History. Fernandez-Armesto (1996: 7), who has provided an account of the last thousand years, insists that his book is a work of creative art that examines the activities of knights

But before concluding this chapter, we need to draw attention to the work of the sociologist Immanuel Wallerstein, who has anticipated some of the moves that we wish to make. In the 1970s, Wallerstein began to stress that the great weakness of the social sciences was the fact that they all operated on the basis of closed systems. He was opposed to disciplinary boundaries separating Political Science, Economics, and Sociology, to the boundary that divided History and Social Science, and to the impermeable boundary that shut political, economic, and social systems off from a wider world. He stressed the importance of using the idea of a 'world system' as the basic unit of analysis in the social sciences to break down these boundaries. What we find so striking is that although IR should have been well placed to make Wallerstein's points for him, the discipline signally failed to do so. Indeed, Wallerstein did not even mention IR. More disturbing for IR, Wallerstein's influence quickly became pervasive. In less than two decades, theorists across the social sciences, including IR, all became thoroughly familiar with his ideas and the many criticisms levelled against them. Just as important, the concepts associated with world systems are now regularly drawn upon within these disciplines. There is nothing like the same familiarity with any IR theorists of the international system. This concept quite simply has failed to resonate beyond what turn out to be the very circumscribed boundaries drawn around the study of international relations. It is difficult to avoid the conclusion that despite strenuous efforts, the discipline has not managed to establish a secure position for itself within the pantheon of the social sciences. Rather than integrating the other social sciences, it stands in some danger of being outflanked, or even reabsorbed, by them.

BRIDGES
&
BOUNDARIES

Bridges and Boundaries

Historians, Political Scientists, and the Study of International Relations

Editors
Colin Elman and
Miriam Fendius Elman

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Introduction

Negotiating International History and Politics

Colin Elman and Miriam Fendius Elman

For this book, we asked distinguished historians and political scientists to take stock of the differences and similarities between their disciplines, to reflect on how disciplinary training influences the study of international events, and to discuss the feasibility of cross-fertilization. The resulting volume explores how scholars from the fields of history and political science can learn from one another, while recognizing some of the nontrivial obstacles that divide them.

The contributors are drawn from particular subsets of their disciplines, and the views expressed are accordingly bounded. The conversation is between what might loosely be described as methodologically traditionalist diplomatic and military historians, and international relations theorists who do qualitative case studies.[1] It would have been possible to arrange a different cross-border dialogue: a broadly conceived volume of social scientists debating humanists, for example, or one that pitted mainstream historians against formal modelers or quantitative political scientists. Although these would have been useful exercises, creating interdisciplinary dialogue is easier with groups that at least bear

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1. "Loosely described" because all such labels are at best approximations, and because individual scholars' work rightfully resists such simple categorizations. For example, some of the political scientists contributing to the volume have utilized quantitative approaches in their work.

some initial similarities because the likelihood for fruitful dialogue is at its highest. We believe that we have assembled two such groups here.[2]

First, the authors share an interest in a common subject matter—the state, politics, and war. This may be of particular interest to the historians, since they are likely to find few opportunities for such a dialogue within their own ranks. Their discipline now puts a premium on subjects outside the traditional purview of diplomatic and military history. Historians have moved away from analyzing statecraft, government interaction, and great political events toward the study of historically voiceless groups.[3] This decline in the study of politics and war is reflected in the diminished attention given to the topics in professional and scholarly journals, in their virtual exclusion from annual meetings of the American Historical Association, and in their falling share of university faculty appointments and curricula.[4]

Second, the political scientists and historians represented here share a number of methodological leanings. As Stephen H. Haber, David M.

2. See Margaret G. Hermann, "One Field, Many Perspectives: Building the Foundations for Dialogue (1998 ISA Presidential Address)," *International Studies Quarterly*, Vol. 42, No. 4 (December 1998), pp. 614–615. These are not, of course, the only two such groups that could be identified, although it should be noted that affinities between the disciplines have often been temporally displaced. For example, today's international historians might feel more affinity for the political science of the immediate post–World War II period, where multicausal analysis and the role of intentions and statesmanship figured more prominently than it does in much of the research in the international relations subfield written over the past three decades. Similarly, political scientists who study international relations today would have found more in common with historians of the nineteenth century—or the cliometricians and new economic historians of the twentieth—many of whom felt that it was not enough for historians to reconstruct the past according to original sources; historians also had to discover general laws of human behavior and development, and they had to emphasize causal explanation rather than understanding, interpretation, or moral reflection.

3. To be sure, international history never captured a monopoly position in twentieth-century historiography. The notion that "history is past politics and politics is present history" was dominant in the nineteenth century, when historical research in the United States (and also in Europe) focused on the state papers and official documents that historian Leopold von Ranke and others considered quintessential to the writing of objective historical accounts. But by the early twentieth century, with democratization and the emergence of a mass society, there was already strong support for histories that transcended politics—a "democratization of history"—by an inclusion of broader segments of the public and an extension of perspective from leading public figures to social and economic conditions. The challenge to traditional historiography—from the French *Annales* school of historians, to the new U.S. social and economic history, to Marxist class analysis—has continued unabated.

4. For example, over the past two decades, the flagship journal of the U.S. historical profession, the *American Historical Review*, has not published a single research article

Kennedy, and Stephen D. Krasner put it, "Historians who study diplomatic history and political scientists who study international politics, despite some genuine differences, have always been engaged in a similar enterprise. Both have always been committed to a positivist methodology in which claims have had to be supported by empirical data."[5] This commitment is an orthodox and dominant position in political science, but is arguably becoming less so in history. Most of the historians in this book would be unsympathetic to postmodern perspectives that tilt the balance away from an objectively knowable past toward consideration of the historian's (and reader's) particular moral, political, or ideological beliefs. While recognizing that facts and documents do not speak for themselves and that observations are theory-laden, most of the participants would agree that claims should be evaluated against empirical evidence, and that historians do not write works of fiction, where stories of the past merely reflect—and serve to perpetuate—the underlying ideologies of their authors. John Lynn puts it well:

> Whereas historians in the past were prone to borrowing theoretical underpinnings from political science and sociology, today they are more likely to import much from anthropology and literary studies. Concepts generated by literary and linguistic scholars seem particularly embarrassing in the study of history because they undermine the value of evidence and conclude that documents cannot actually tell us about reality but only about the author of the document. This "linguistic turn" may be fine when approaching a novel or a poem, but it is usually malarkey when applied to the war archives.[6]

Third, just as both groups share a common commitment to attempting an objective analysis of the past, so too do they both suffer from pressures that seek to reduce their respective disciplines to communities in which qualitative research cannot find a home. In history, the popularity of postmodernist approaches has thrown traditional methods into disrepute. Diplomatic and military historians, who appeal to evidence

focused on the conduct of the Hundred Years' War , the Thirty Years' War, the Wars of Louis XIV, the War of American Independence, the Revolutionary and Napoleonic Wars, World War I, or World War II. See John A. Lynn, "The Embattled Future of Academic Military History," *The Journal of Military History*, Vol. 61 (October 1977), p. 780.

5. Stephen Haber, David M. Kennedy, and Stephen D. Krasner, "Brothers Under the Skin: Diplomatic History and International Relations," *International Security*, Vol. 22, No. 1 (Summer 1997), p. 34.

6. Lynn, "The Embattled Future of Academic Military History," p. 779. For another critique of the postmodern attack on objectivity, see Arthur Schlesinger, Jr., "History as Therapy: A Dangerous Idea," *New York Times*, May 3, 1996.

and share a commitment to uncovering an objectively knowable past, find themselves squeezed out of the discipline, and see themselves as one of the few remaining outposts of pre-postmodern approaches. Many traditional historians—diplomatic and military historians included—are so alienated and dissatisfied with their situation that they have formed a new professional association (the Historical Society), thereby further disassociating themselves from the discipline's mainstream.[7] For international historians the growing fear is that, among historians working in the United States at least, they are fast becoming a "dying breed," with international history dominated by political scientists who "do not do this history as historians can and should do it."[8]

In the international relations subfield of political science, by contrast, positivist approaches continue to dominate the discipline, and postmodernists are seen as minority dissidents. But international relations theorists who employ qualitative case study methods are subject to equally debilitating pressures: a large and increasing majority of effort, attention, and journal space is being given to formal modeling and quantitative research. In the flagship journal of the American Political Science Association, the *American Political Science Review,* for example, virtually none

7. For overviews of nineteenth- and twentieth-century trends in the study of history, see Robert William Fogel and G.R. Elton, *Which Road to the Past? Two Views of History* (New Haven, Conn.: Yale University Press, 1983); Georg G. Iggers, *Historiography in the Twentieth Century* (Hanover, N.H.: Wesleyan University Press, 1997); R.F. Atkinson, *Knowledge and Explanation in History: An Introduction to the Philosophy of History* (Ithaca: Cornell University Press, 1978), pp. 14–17; Michael Kammen, "The Historian's Vocation and the State of the Discipline in the United States," in Michael Kammen, ed., *The Past Before Us: Contemporary Historical Writing in the United States* (Ithaca: Cornell University Press, 1980), pp. 19–46. For extended discussions of the decline of diplomatic and military history in the profession, particularly in U.S. academe, see Gordon A. Craig, "The Historian and the Study of International Relations," *American Historical Review,* Vol. 88, No. 1 (February 1983), p. 2; Haber, Kennedy, and Krasner, "Brothers Under the Skin," pp. 34–43; Lynn, "The Embattled Future of Academic Military History," pp. 777–789; Charles S. Maier, "Marking Time: The Historiography of International Relations," in *The Past Before Us,* pp. 355–387; Stephen Haber, "Explaining the Methods Gap: History and the Social Sciences," paper presented at the interdisciplinary conference on Diplomatic History and International Relations Theory: Respecting Differences and Crossing Boundaries, Arizona State University, January 15–18, 1998.

8. Paul Schroeder, "The AHA and the Historical Society" reprinted in H-DIPLO@H-NET.MSU.EDU, September 30, 1998. For more on the new Historical Society see Courtney Leatherman, "Saying Their Field Is in 'Disarray', Historians Set Up a New Society," *Chronicle of Higher Education,* May 8, 1998, p. A12; Courtney Leatherman, "The Historical Society's Own Motives Are Topic No. 1 At Its First National Convention," *Chronicle of Higher Education,* June 11, 1999, p. A16; and the debate between James Banner and Marc Trachtenberg reprinted in H-DIPLO@H-NET.MSU.EDU, September 26, 1998.

of the few articles on international relations employ qualitative methods. Qualitative international relations theorists make do with fewer funding and publishing opportunities, and are increasingly pressured to defend their status as social scientists.[9]

Viewed collectively, the authors of this book share an interest in a common subject matter, a commitment to utilizing empirical research to uncover a knowable past, and a sense that they are becoming less welcome in their own disciplines. Accordingly, addressing what international historians and political scientists who employ qualitative methods to study international relations can learn from each other may be even more important now than it has been in the past.[10] Despite their commonalities, however, there are important disagreements among the contributing political scientists and historians—both with regard to how they view their own enterprise, as well as that of the other discipline.

International History and Politics: Boundaries

With the foregoing caveats in mind, in this section we discuss ways in which history and political science diverge. These include: different views of the purpose of theory and historical evidence; different understandings of causation; a contrasting emphasis on purposive and intentional behavior versus unintended outcomes; a related disparity in the degree to

9. We are not suggesting that qualitative methodologies are inherently superior to either quantitative or formal approaches; all three approaches have much to offer, but an undue emphasis on some to the exclusion of others is harmful. For similar appeals for methodological pluralism, see Lisa L. Martin, "The Contributions of Rational Choice: A Defense of Pluralism," *International Security,* Vol. 24, No. 2 (Fall 1999), pp. 80–83; Stephen M. Walt, "Rigor or Rigor Mortis? Rational Choice and Security Studies," *International Security,* Volume 23, Number 4 (Spring 1999), pp. 7, 47–48; and Stephen M. Walt, "A Model Disagreement," *International Security,* Vol. 24, No. 2 (Fall 1999), pp. 115, 128–130.

10. On this point, see Jack S. Levy, "Too Important to Leave to the Other: History and Political Science in the Study of International Relations," *International Security,* Vol. 22, No. 1 (Summer 1997), p. 23.

which attempts are made to attach judgments to political behavior; and a dissimilar sense of aesthetics. As we note in the next section, however, these dichotomous boundaries are increasingly challenged, and they are weakening in some important respects.

THE PURPOSE OF THEORY AND HISTORICAL EVIDENCE

While historians and political scientists apply theory to the study of international events, they use theory differently. As Richard Ned Lebow observes in his chapter, "Historians and social scientists lay claim to the same terrain with very different purposes in mind. Historians study the past as a valuable exercise in its own right. . . . Social scientists regard the past as data that might help them develop and test theories of human behavior."[17] Political scientists who study international relations from a qualitative perspective defend their research as significant and worthwhile not by demonstrating that they have helped to understand historical periods and events, but because they advance some particular research program, improve upon some existing theory, or generate new competing theories that challenge the received wisdom.[18]

Consider, by way of example, *Analytic Narratives*, a recent book by a group of distinguished political scientists. Although the chapters in the book convey an attention to historical detail and are explicitly geared to using the "narrative form, which is more commonly employed in history," the authors have a larger theoretical goal than merely explaining the "compelling" cases at hand: to further social inquiry based on the rational choice approach and game-theoretic models.[19] Randall L. Schweller's chapter in this volume provides an additional example. Schweller is explicit about why he is interested in studying the interwar period and World War II: not primarily for its own sake (i.e., not "to offer a new explanation of the war") but because of the impact his findings will have on classical realist and neorealist approaches to the study of international

17. See also Jack Levy's chapter. Note too Edward Ingram's "corset versus armor" metaphor for distinguishing the different ways theories are used in the two disciplines; John Lewis Gaddis's distinction between using theory to encompass narrative (political scientist) and embedding theory within narrative (historian); and Robert Jervis's and Ingram's claim that explanation and description are deeply intertwined in the historian's work, but sharply distinguished by political scientists.

18. King, Keohane, and Verba, for example, argue that good research designs in the social sciences should make an explicit contribution to the existing literature. See *Designing Social Inquiry*, pp. 15–17.

19. Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry R. Weingast, *Analytic Narratives* (Princeton: Princeton University Press, 1998).

relations. As William R. Thompson points out, and as Schweller's chapter illustrates, political scientists are rarely driven by empirical puzzles that emerge from observed events—why did the Cold War end peacefully? why did Gorbachev withdraw from Eastern Europe?—the bread and butter of the historian's enterprise. Instead, political scientists' work is puzzle- or theory-driven. Political scientists derive their puzzles from observed behavior that appears inconsistent with what existing theories would lead us to expect: how does the end of the Cold War, and Gorbachev's withdrawal from Eastern Europe, affect neorealist theories of international relations? Thus, according to Thompson, "political scientists are supposed to begin with theories and then move toward cases that can help them assess the relative utility of the theories that exist."[20]

THE NOMOTHETIC/IDIOGRAPHIC DISTINCTION. Linked to the political scientist's interest in theory-driven research is a consensus among the chapter authors that, for the most part, political scientists prefer generalizations that apply to different contexts across time and space, while historians eschew generalizations for deeper explanations and understandings of particular events—what Levy refers to as the nomothetic/ideographic distinction, or what has also been labeled narrative versus analytic explanation.[21] Instead of subordinating theory to the search for greater historical understanding of particular events and periods, political scientists are oriented to general problems and "prefer explanations of event-categories."[22] Trained in the positivist tradition,

20. The increasing respect for single case studies exhibited by recent proponents of qualitative methods, while further blurring the lines between history and political science in practice, has not changed the distinction between research driven by theoretical puzzles and empirical puzzles. For political scientists, single case studies are only valuable insofar as they have large theoretical implications. See Ronald Rogowski, "The Role of Theory and Anomaly in Social-Scientific Inference," *American Political Science Review*, Vol. 89, No. 2 (June 1995), pp. 467–470; and Timothy J. McKeown, "Case Studies and the Statistical Worldview: Review of King, Keohane and Verba's *Designing Social Inquiry*," *International Organization*, Vol. 53, No. 1 (Winter 1999), pp. 161–190. See also the chapter by Andrew Bennett and Alexander George in this volume.

21. See Dale H. Porter, *The Emergence of the Past: A Theory of Historical Explanation* (Chicago: University of Chicago Press, 1981); and Atkinson, *Knowledge and Explanation in History*, pp. 14, 113. As Levy notes, the nomothetic/idiographic distinction better describes contemporary differences in approach between historians and political scientists. However, it is inappropriate when considering the Hempelian tradition among historians and philosophers of history, popular in the 1940s and 1950s, which held that historians, like social scientists, explain events by subsuming them under laws governing the occurrence of like events.

22. Bruce Bueno de Mesquita, "Theory and the Advancement of Knowledge About War: A Reply," *Review of International Studies*, Vol. 10, No. 1 (January 1984), p. 67.

they explain individual events by identifying "if, then" generalizations, or covering laws, that match the pattern of the particular event. That is, an event is explained as an instance of a certain type of event, which is shown to accompany, and follow regularly from, specified initial conditions.[23] By contrast, historian Richard Hofstadter argues that "in our own time the scientific ideal no longer has quite the same plausibility for historians as it did for their predecessors in the Darwinian age, or as it now has for their colleagues in the social sciences. Most historians continue to feel that they deal with events which, though in some sense comparable, are essentially unique."[24]

Historian Clayton Roberts and political scientist Joseph Lepgold aptly observe the difference. According to Roberts: "Historians study the battle of the Marne, not battles in general; they seek the causes of the Enlightenment, not of enlightenments in general; they study the rise of Hitler, not of dictators in general. Things in general they leave to the sociologists." Says Lepgold: "A good IR [international relations] theory . . . should identify and explain patterns that transcend particular issues, actors, and historical eras. . . . Theory offers little explanatory power if it must be reinvented each time issues, actors, and eras change."[25] Similarly, John Lynn notes in his chapter that "there is some truth to the old stereotype that historians are embedded in their case studies and usually reluctant to generalize. The defining characteristic of historians may not be their dedication to the past in general, but their immersion in a *particular* past."[26]

23. Jack Snyder, "Science and Sovietology: Bridging the Methods Gap in Soviet Foreign Policy Studies," *World Politics* (1988), pp. 171–172. See also Bruce Bueno de Mesquita, "The Benefits of a Social Scientific Approach to Studying International Affairs," in Ngaire Woods, ed., *Explaining International Relations Since 1945* (Oxford: Oxford University Press, 1996), pp. 52–53.

24. Richard Hofstadter, "History and the Social Sciences," in Fritz Stern, ed., *The Varieties of History: From Voltaire to the Present* (New York: Meridian, 1957), p. 367.

25. Clayton Roberts, *The Logic of Historical Explanation* (University Park, Penn.: Pennsylvania State University Press, 1996), p. 8; Joseph Lepgold, "Is Anyone Listening? International Relations Theory and the Problem of Relevance," *Political Studies Quarterly*, Vol. 113, No. 1 (Spring 1998), p. 47.

26. Some chapter authors insist that historians *do* make generalizations (see, for example, Lynn, Pelz, Schroeder, and Gaddis), but are careful to limit these claims to specific times and places. Whereas the political scientist's scope conditions are usually analytic in nature, the historian's scope conditions are temporal and geographical. For the argument that generalizations are essential to conveying the meaning of a historical episode, and showing that it was consequential, see Raymond Martin, "Objectivity and Meaning in Historical Studies: Toward a Post-Analytic View," *History and Theory*,

Contrary to political scientists who generate causal generalizations, historians prefer to view historical cases in their entirety, and employ a holistic approach to explain complex events—effects "grow" out of "causes" by processes that cannot be broken down into identical, comparable, or discrete units.[27] Complex events such as a war, a revolution, and the rise of a parliament, are too rich in detail and too different from other members of its class to be subsumed under any covering law. Not only is the historian interested, for instance, in World War II for its own sake and not as a typical example of a great power war, but she insists that there is no such thing as a *typical* great power war. Consequently, many historians proceed, in John Gaddis's words, to "reconstruct complex events" by tracing the sequence of events that brought them about, and by showing that the connections between those events and other certain previous events "stand in an inner relationship," constitute a single process, and belong together.[28] According to Dale H. Porter, for historians

> the explanation is not intended to demonstrate an invariant relationship between typical causes and typical effects, but to show that *this* particular set of events appears most intelligible if one looks at it *this* way. . . . As a pattern of events emerges, the meaning of any element in the pattern may change from what was expected at an earlier stage, and cannot be determined fully until the whole pattern has developed. Understanding events in this way requires a shift from *prediction* to *retrodiction* or reasoning from present to past . . . one has to look at the event according to its future consequences—what it meant for subsequent events. The historian does this because he senses that the significance of an event depends upon hindsight: it is really determined by what happened later.[29]

Rather than being a mere chronological sequencing of events, the historian's narrative explains an event by tracing its intrinsic relation to other

Vol. 32 (February 1993), pp. 44–49. According to Martin, historians make the case that we should care about the defeat of the Spanish Armada or the witch hunts of the sixteenth and seventeenth centuries by moving away "from the local and the particular and toward a larger perspective." Either the historian shows how the episode fanned out to effect aspects of subsequent history, or she shows how the event was an instance of a broader social pattern. See also Porter, *The Emergence of the Past*, pp. 35–36, 44–47.

27. Porter, *The Emergence of the Past*, p. 42.

28. Roberts, *The Logic of Historical Explanation*, pp. 16–17. Philosophers of history and historians themselves have termed this historical method in a variety of ways: narrative explanation, genetic explanation, synoptic judgment, sequential explanation, and colligation.

29. Porter, *The Emergence of the Past*, pp. 10, 14, 86–87.

events that both precede and follow it, and by seeing the same event from difference perspectives—something participants in the event could not have done. As Edward Ingram argues, "Historians live by an idiosyncratic version of clock time in which the clock may travel in both directions. . . . World War I does cause the Crimean War, for historians if not for Napoleon III and Viscount Palmerston."[30]

~~To be sure, we should not overstate this distinction. Many political scientists who study international relations share international historians' focus on particular, important events, and feel uncomfortable generating universally applicable law-like generalizations. They prefer contingent generalizations relevant to specific times and places. Similarly, like political scientists, many international historians appreciate that international politics can exhibit considerable continuity over time, and that within these stable periods international events can be usefully compared in generating generalizable theories.[31] Nevertheless, the nomothetic/idiographic distinction is an important one.~~

WHAT IS A CASE? Political scientists explain particular situations by referring to other situations of the same type. For the political scientist, historical events are discrete and clustered—they are treated as similar to the degree that they contain similar components. As Charles C. Ragin observes, "implicit in most social scientific notions of case analysis is the idea that the objects of investigation are similar enough and separate enough to permit treating them as comparable instances of the same general phenomenon."[32]

By contrast, historians are less likely to view complex historical events and processes as comparable, and resist detaching them from their "temporal moorings." For the most part, historians are unwilling to explain individual cases less fully in order to construct a grand theory that will explain the basic parameters of many cases with only a few causal factors. They also tend to view historical events as too complex to be easily classified with other events. As William C. Wohlforth puts it, "Historians' practice is a rejection of the very idea that events can be neatly coded as cases to test theories." Individual events are not simply instances, but the main theme. For example, an explanation of Louis

30. Ingram, "The Wonderland of the Political Scientist," p. 57.

31. On these points, see the chapters by William C. Wohlforth and William R. Thompson in this volume.

32. Ragin, "Introduction: Cases of 'What Is a Case?'" in Charles C. Ragin and Howard S. Becker, eds., *What Is a Case? Exploring the Foundation of Social Inquiry* (Cambridge: Cambridge University Press, 1992), p. 1.

Napoleon's decision to go to war with Prussia couched as "under such and such circumstances monarchs are likely to go to war" would be untenable. Even if such a statement correctly recorded that seven out of ten monarchs declared war under similar conditions, it would not explain why *Louis Napoleon* went to war.[33] In short, historians are wary of comparisons over long periods and generally reject attempts to project theories onto the future. While theories of international relations may be adequate for particular cases and periods, as time passes the assumptions on which they are based will cease to be true, and the theories will need to be revised. In the words of military historian Geoffrey Parker, "four years, let alone four centuries, can be a very long time where warfare is concerned."[34]

An example from this book nicely illustrates this difference. Historian Gerhard L. Weinberg (Chapter 5) insists that World War II cannot be compared to previous international conflicts because the German state and its war aims were unique. For Weinberg, World War II is in a class by itself. Political scientist Randall L. Schweller (Chapter 6) views Nazi Germany as a case for a generalizable theory. Instead of viewing Germany's policies as distinctive, Germany's desire for war is consistent with the foreign policy that we would expect from a revisionist state under conditions of tripolarity. For Schweller, Germany's foreign policy represents a case from a class of events—behavior encouraged by the constraints and opportunities of a tripolar international system.

Our point here is not only that Weinberg and Schweller explain the events differently, but that disciplinary norms guide the scholars in opposite directions. Political science guild rules, which reward theories that transcend time and place, encourage scholars *not* to differentiate between the world wars and compel them to treat Nazi Germany's foreign policy as an illustrative example of some larger theory of international behavior. Operating under a different set of guild rules, historians are better able to make these distinctions when the facts point in this direction. Thus, Schweller's theory-driven study forces him to "black box" Nazi Germany's domestic politics. To build a generalizable theory with Nazi Germany and World War II as illustrative cases, Germany must be treated as just another great power, and its foreign policies must be comparable to those of other states. The Holocaust as atypical genocide makes it difficult to use Germany as a case from a class of events; Schweller's

33. Fogel and Elton, *Which Road to the Past?* p. 43. See also Atkinson, *Knowledge and Explanation in History*, p. 35.

34. Personal correspondence, June 23, 1998.

theoretical explanation must be constructed without reference to it. By contrast, because Weinberg's guild rules allow him to consider Nazi Germany's policies as unique historical events, he is able to explain World War II by linking Germany's domestic politics with its international behavior. For Weinberg, a history of World War II that omitted the mass extermination of the Jews and the killing of other undesirables cannot be written.

~~rians use events for theory-building, they include the reservation that new facts may compel a change of the theory. As Wohlforth suggests, because they are resigned to the provisional nature of their readings of history, historians pay more attention to fresh historical evidence discovered with the opening of archives than do political scientists, who are less likely to view anomalous evidence as having a critical bearing on their theories.[41]~~

CAUSES AND CAUSATION

According to Gaddis, both political scientists and historians make causal claims, but do so differently: "Historians believe in contingent, not categorical causation." Similarly, Paul W. Schroeder suggests that political scientists and historians "conceive of and deal with causes in human affairs" in different ways. Nonetheless, both sets of scholars look for the same things from an explanation—clearly assigned causes resting on evidence subject to tests and verification.[42] For political scientists, causation involves establishing a concrete and testable relationship among variables, including identifying the necessary and sufficient conditions that regularly produce a particular result. Accordingly, political scientists try to rule out, and prefer to assign weights and rank to, variables rather than resign themselves to multiple sufficient causation. They usually avoid overdetermined explanations and rule out weaker causal factors in

42. By contrast, some historians argue that historical explanation involves understanding and interpretation rather than causation. According to this view, historical events and developments are not explained by assigning specific causes for them—instead the historian attempts to explain them through a process of empathetic understanding. For a critique of this view, see Fritz K. Ringer, "Causal Analysis in Historical Reasoning," *History and Theory*, Vol. 28 (1989), pp. 154–172; and Paul W. Schroeder, "History and International Relations Theory: Not Use or Abuse, but Fit or Misfit," *International Security*, Vol. 22, No. 1 (Summer 1997), p. 67.

favor of the one cause with the greatest explanatory power, rejecting "shopping list" explanations. For example, although realists concede that domestic political pressures affected the timing and rate of British, U.S., and French rearmament against Nazi Germany and also influenced the specific strategies that these states pursued, they insist that the international-structural level of analysis provides a more parsimonious—and hence better—explanation for the inefficient balancing displayed. That is, since multipolar international distributions of power decrease the likelihood of effective balancing against rising challengers, this reading of the case should suffice.[43]

Political scientists also generally assume that causal explanation requires comparison across cases. As Andrew Bennett and Alexander George and Jack Levy observe in their chapters, the received wisdom is that causality cannot be identified within the context of a single observation. Political scientist James Lee Ray explains: "no self-respecting 'scientist' is really interested, at least while wearing his or her 'scientist's' hat, in *a* case. Single events cannot be 'explained' . . . no 'explanation' is worthy of the name unless it alludes to a pattern into which the event in question fits."[44] Political scientists devote attention to choosing appropriate cases that will afford severe—or decisive—theoretical tests. Historical cases are chosen for study if they create hard tests for preferred theories, thereby establishing greater confidence in them.[45]

Many historians find these premises suspect. Although most of the historians represented in this volume would agree that historical arguments presuppose causal statements, they would likely find the political scientist's definition of causality disturbing.

Several of the chapter authors in this book suggest that most historians attempt to establish causation by process tracing; that is, by relating events to other events in a sequence.[46] Accordingly, historians are likely to be uncomfortable with attempts to explain complex events by subsum-

43. See Stephen M. Walt, "Alliance, Threats, and U.S. Grand Strategy: A Response to Kaufman and Labs," *Security Studies*, Vol. 1, No. 3 (Spring 1992), pp. 448–482; and Joao Resende-Santos, "System and Agent: Comments on Labs and Kaufman," *Security Studies*, Vol. 1, No. 4 (Summer 1992), pp. 697–702.

44. James Lee Ray, *Democracy and International Conflict: An Evaluation of the Democratic Peace Proposition* (Columbia: University of South Carolina Press, 1995), pp. 133, 136.

45. On this point, see the chapter by George and Bennett in this volume. In addition to selecting crucial cases, political scientists are also often instructed to choose "nonevents," thereby allowing variation on the dependent variable. To understand great power wars, for example, cases of great power war as well as crises that did not result in war would need to be compared.

46. See, for example, the chapters by Lebow, Levy, Bennett and George, and Gaddis.

ing them under laws, and attempts to distinguish between independent and dependent variables. They would agree that some causes are more important than others, but they would resist the understanding of causation that is implicit in most international relations theory.[47] As Gaddis puts it: "Historians do not . . . accept the doctrine of immaculate causation, which seems to be implied in the idea that one can identify, without reference to all that has preceded it, such a thing as an independent variable." Since in historical explanations each part of the sequence is dependent on its predecessor, and since early events and actions are considered to have important consequences for later developments, no one causal factor can be "lifted out" of the explanation and deemed independent.[48]

Not only does this belief in historical contingency belie the notion of independent variables, but it also rejects as "irresponsible" attempts to isolate single causes for complex events.[49] For historians, causes are often things that intervene in a process that has already begun, and effects are alterations in an expected trend or chain of events that would not have occurred had the cause been absent. That is, important causes are those that depart most from the normal course of events.[50] Moreover, as Robert Jervis notes in his contribution to this volume, and as the debate between Lebow and Gaddis on the latter's treatment of the origins of the Cold War makes clear, historians are less troubled than political scientists by multiple sufficient causation or overdetermination; that is, alternative possible sequences of events such that the same outcome can be produced by different causes.

For historians, multiple sufficient causation is less worrisome partly

47. See the chapters by Schweller, Gaddis, Schroeder, Ingram, and Pelz in this volume. For an overview of how historians decide which causes are the most important, see Roberts, *The Logic of Historical Explanation*, chap. 5. For the argument that historians establish causation by comparing cases, usually aspects of the prior history of the actor whose fate is being explained, see Martin, "Objectivity and Meaning in Historical Studies," p. 41, and Raymond Martin, "Causes, Conditions, and Casual Importance," *History and Theory*, Vol. 21 (1982), pp. 53–74.

48. Identifying necessary or sufficient conditions might also be difficult for historians because of their tendency to view outcomes as arising through different causal paths (i.e., equifinality). For an extended discussion of equifinality, see the chapter by Bennett and George in this volume.

49. The political scientist's preference for monocausality and simplicity should not be equated with a greater concern for parsimonious explanation. Parsimonious explanations need not be monocausal, but they do seek to explain a wide range of phenomena with very few causal factors. See, on this point, Jervis's chapter in this volume.

50. See Ringer, "Causal Analysis in Historical Reasoning"; and Roberts, *The Logic of Historical Explanation*, pp. 96–99, 104, 116–117.

because international events are often considered the result of simultaneity—the interactivity among variables, which converge at a particular historical moment. It is difficult to argue that a given cause is stronger or should be given more causal weight in the analysis when causes interact with each other so as to multiply their impact. Causes often cumulate, and aggravate or amplify the effects of other causes, pushing the international system beyond some critical threshold.[51] In sum, historians find political scientists' insistence on isolating a few fundamental causes acting independently bemusing because causality is complex, and things often happen because variables are mutually reinforcing, intersecting at particular moments in space and time. For historians, history is full of these unpredictable intersections—the result of chance and accident.[52]

PURPOSIVE BEHAVIOR AND UNINTENDED CONSEQUENCES

Perhaps to compensate for their complex views of causation, historians often seem content to explain international events with particular reference to the aims of the actors involved. By contrast, while they hold more parsimonious positions on causation, international relations theorists are trained to expect and emphasize the considerable slippage between what actors want and what they get. Many of the contributors to this volume support this distinction between the historian's explanation of goal-oriented, purposive action and the political scientist's account of irrational, unintended consequences. Deborah Welch Larson argues that historians tend to adopt a "rational calculus" where actors' reasons and goals are considered explanations for their actions in response to prevailing conditions. Whereas historians focus on "human conduct," political scientists analyze "behavior," recognizing that policymakers do not always anticipate the results of their actions. Similarly, according to Paul Schroeder, historical change is explained ultimately in terms of human conduct, the purposive acts of agents, not behavior. While external, nonhuman factors shape historical developments, historical explanation should reveal human conduct in response to these conditions.[53] For example, Gerhard Weinberg focuses on the aims and goals motivating

51. See Roberts, *The Logic of Historical Explanation*, pp. 140–141.

52. For the argument that Mill's methods of difference and agreement—common procedures used by political scientists—are unhelpful when there are interaction effects and more than one cause is operating, see Stanley Lieberson, "Small N's and Big Conclusions: An Examination of the Reasoning in Comparative Studies Based on a Small Number of Cases," in Ragin and Becker, eds., *What is a Case?* pp. 105–118.

53. See also, Schroeder, "History and International Relations Theory," pp. 67–68.

For some historians, the essence of historical explanation is the reconstruction of agents' aims, motives, and intentions. Good historical explanations are those that show how actions (not events) correspond to actors' goals and purposes; the good historian inquires about the reasons behind intentions. In other words, state behavior reflects state preferences—conflict must be provoked by an actor having a stake in the issue; cooperation will only be sought by an actor who has a reason to cooperate.[54]

By contrast, perhaps in large part due to the popularity of systems theory in general, and Kenneth N. Waltz's *Theory of International Politics* in particular, international relations theorists often label such explanations reductionist, and argue that international outcomes do not always follow from the motivations, aims, or purposes of individual policymakers. Unexpected and unwanted consequences undermine the original intent of decision-makers: cooperative international relations can emerge even when leaders are aggressive; war can result even when statesmen want peace.[55] Many international relations theorists are trained to look *first* for structural influences that generate unintended, unexpected, or ironic consequences. They believe that much of international relations can be explained without looking at actors' preferences or motivations.

"What states want is the primary determinant of what they do" may seem commonsensical, even tautological. Yet mainstream IR theory has uniformly rejected such claims for the past half-century. At the heart of the two leading contemporary IR theories, realism and institutionalism, is the belief that state behavior has *ironic* consequences. . . . What states do is primarily determined by strategic considerations—what they can get or what they know—which in turn reflect their international political environment.[56]

54. For an overview of the importance attached to purposive action in historical narrative, see Schroeder's chapter and Roberts, *The Logic of Historical Explanation*, chap. 8.

55. Elman and Elman, "History vs. Neo-realism," p. 189. To be fair, Waltz does recognize that motives can shape particular foreign policy choices. What he suggests is that if actors continually follow their preferences in ways that are contrary to international constraints, they will be punished for their free will—states are free to make mistakes, but those that do consistently will fall by the wayside.

These differing perspectives can affect both the explanation for particular cases (for example, the bulk of Schweller's chapter shows how international structure intervenes between intentions and outcomes) as well as case selection. It is not surprising, for instance, that World War I rather than World War II has received the lion's share of interest among political scientists. Because status quo–oriented states nevertheless went to war, World War I is a good case for political scientists who prefer to tease out how international constraints and opportunities can lead to unexpected outcomes. Political scientists tend to focus greater attention on the historical events that display a better "fit" with disciplinary guild rules.

To be sure, all this is not to say that historians are insensitive to unintended consequences and nonpurposive causal factors. Philosophers of history and historians—international historians included—recognize that outcomes can be unintended, and they have been quick to note that historical explanations do not require purposive action.[57] As Dale H. Porter observes, "Underlying the bulk of historical inquiries is the desire to explain the unpredictability of human affairs, to show how things did not turn out the way people thought or hoped they would."[58] Historians are interested in the contrast between how things look in hindsight and how things seemed to participants at the time. Path-dependent approaches, for example, show how events are linked to preceding events—often in ways that were unexpected by the individuals involved in the earlier episodes. According to this view, chance and coincidence play as much of a role in historical explanation as do choices and decisions. In emphasizing unexpected effects, such path-dependent explanations are more consistent with mainstream political science than one might otherwise think.

MORALITY TALES

According to Deborah Welch Larson, historians focus on purposive action because they are interested in rendering a moral judgment on the past. Since human conduct is viewed as purposive attempts to achieve certain goals, historians can hold actors morally responsible for their actions.

57. See, for example, Edward Hallett Carr, *What Is History?* (New York: Knopf, 1962), pp. 55, 57, 64; and Herbert Butterfield, *History and Human Relations* (New York: Macmillan, 1952), pp. 15–17, 19–20, 22, 77.

58. Porter, *The Emergence of the Past*, p. 34. See also Atkinson, *Knowledge and Explanation in History*, pp. 102, 127, 171, 187.

Other contributors to this book agree that historians' accounts are—and should be—colored by moral judgment. Like Larson, Jervis notes that it is perhaps "their greater interest in people" that lead historians to assess the wisdom and morality of actors; Stephen Pelz comments on the judgmental quality of history, noting that "narrative historians are often trying to teach practical or moral lessons that their stories imply;" and Paul Schroeder insists that moral judgments are inescapable ingredients of historical arguments. According to Schroeder, holding actors accountable for their actions does not mean that historians are idealists, committed to achieving a more peaceful and humane world. Rendering moral judgment does not *require* that historians make history an instrument of social change. Rather, Schroeder argues that in order to describe what people do, historians need to discuss the moral dimension of their actions: "Every attempt to construct an 'objective' value-free language to tell the story of what human beings have done and suffered not only breaks down and denatures the narrative and analysis alike, but does so without really avoiding moral judgments, instead masking, blurring, and fudging them." In addition, theories and explanations of events carry with them policy implications that can be morally suspect or ambiguous. And to explain major events and show what the countries involved were doing, historians must get involved in the "blame game," asking whether the actors involved followed the rules and norms of international politics, or whether they attempted to wreck them.[59]

By contrast, political scientists tend to address the historical record in value-free language. As Larson puts it, "their aim is explanation, not blame-fixing." Political scientists usually do not discuss whether actors behaved wisely, much less morally. This may partly have to do with the nature of their epistemology. As David Dessler argues, "generalizing causal knowledge is typically more value-neutral with respect to the problems it helps address than particularizing historical and interpretive accounts are." Generalizations of the "if *X*, then *Y*" type adopt a neutral position on the value of "*X*"—society may view it favorably or not; the generalization merely produces knowledge that the different sides can recognize as adequate.[60] As Schroeder points out, political scientists studying the democratic peace do not take a position on the relative value of democracy, as opposed to other ways of organizing the polity, when generating their causal arguments—despite the fact that the democratic peace theory entails precisely this sort of moral implication.

59. To some extent, the position adopted by Schroeder and others in this book is at odds with dominant earlier views on the nature of historical explanation. Nineteenth-century historians, adopting the Rankean ideal of an objective historical account—history "as it really was," independent of the historian's biases and preconceptions—eschewed casting blame or praise on historical figures, and criticized earlier historiography for its moralizing stance. These arguments appear frequently in twentieth-century historiography as well. See, for example, Butterfield, *History and Human Relations*, pp. 105–106, 127.

International History and Politics: Bridges

While the contributors to this volume identify several disciplinary cleavages, the chapters also suggest areas for cross-fertilization. This section notes how some of the disciplinary boundaries are weakening.

PROCESS TRACING, PATH DEPENDENCE, AND CAUSAL MECHANISMS

Mitigating the epistemological differences noted previously, several of the contributors to this book agree that political scientists and historians are increasingly employing similar understandings of process tracing, path dependence, and causality. This convergence helps overcome some of the boundaries described in our earlier section.

A third and related convergence is the increasing agreement between political scientists and historians on the importance of identifying the causal mechanisms that connect an effect with its cause.[68] Historians have traditionally, if implicitly, favored explanations that reveal causal mechanisms. As noted in the previous section, for political scientists this has not always been the case. For example, Bennett and George note that researchers using formal models and statistical methods have long held that good scientific explanations do not require establishing intervening causal processes that link cause and effect. Rational choice theorists in particular minimize the importance of causal mechanisms, arguing that we can view decision-makers "as if" they are rational actors without suggesting that they actually calculate costs and benefits in this way. That is, rational choice assumptions are useful if the theory that employs them produces accurate predictions, regardless of whether actors actually think and speak in ways consistent with rational choice theories.[69]

However, more international relations theorists now acknowledge that mere congruence between outcomes and a theory's initial conditions

68. For the argument that establishing causation *requires* the specification of causal mechanisms, see, for example, Daniel Little, *Varieties of Social Explanation: An Introduction to the Philosophy of Social Science* (Boulder, Colo.: Westview, 1991), pp. 13–38. For a contrary view, although one that recognizes the value of identifying causal mechanisms, see King, Keohane, and Verba, *Designing Social Inquiry*, pp. 85–87.

69. See, for example, Christopher H. Achen and Duncan Snidal, "Rational Deterrence

does not necessarily mean that the theory provides an adequate causal explanation. As James Lee Ray observes, today many international relations scholars agree that "there must be some essential core of correspondence between the actual calculations made by real decision makers and the calculations stipulated by formal models . . . a formal model, or any other theory, must in fact capture the fundamentals of real world processes if it is going to produce accurate predictions or valid explanations."[70] Some of our most influential research programs in the subfield—such as the democratic peace—explicitly reject "as if" arguments. For example, democratic peace theorists wish to determine how democratic norms and institutions affect the ways elites behave in negotiations, and how political actors justify their actions. The aim is not to show that decision-makers act "as if" they are constrained by democratic norms and principles, but that they consciously make the calculations posited by the democratic peace model.

~~Aimed at uncovering causal mechanisms~~, much international relations scholarship is becoming less antithetical to historical methods. As we have suggested elsewhere, "the recent resurgence of scientific realist understandings of social science, which mandate the search for 'real' causal mechanisms, may prove a more hospitable environment for a useful interdisciplinary conversation."[71] The flipside is that these political scientists who employ stylized facts, and who are drawn to the methods and approaches employed by economists, may be resistant to an interdisciplinary exchange ~~with historians~~.

SUBSTANTIVE ARGUMENTS AND HISTORICAL METHODS

Not only are international historians and qualitative international relations theorists finding common methodological bonds, but there also new areas of convergence in substantive arguments. Increasingly, historians and political scientists mean the same thing when they talk about international relations. This convergence owes much to the end of the Cold War and the break-up of the Soviet Union. Both events motivated international relations theorists to reconsider the explanatory power of the dominant structural approaches in the field (neorealism and neoliberal

pp. 163–166. For more on how political scientists have relied on the "as if" assumption, eschewing investigations of causal mechanisms, see Ray, *Democracy and International Conflict*, pp. 133–134, 144.

70. Ray, *Democracy and International Conflict*, p. 151.

71. Elman and Elman, "Diplomatic History and International Relations Theory," p. 18.

institutionalism), and opened up new space for competing approaches that emphasize the role of ideas, culture, domestic politics, statesmanship, and the possibility of change.[72] Political scientists who study international relations have moved considerably closer to international historians' perspectives on the topic. Two popular strands of contemporary international relations research, constructivism and classical or neoclassical realism, are examples of this phenomenon.

Constructivists (not to be confused with the constructionist and postmodernist historians that Stephen Pelz mentions, who have more in common with critical theorists in political science) are primarily interested in how past interactions construct future patterns of behavior among states, and at how states can extricate themselves from replicating conflictual relations.[73] Rather than identify uniform state identities throughout history, recurring patterns of conflictual international relations, or the similarity of state behavior under particular structural conditions (as neorealists do), constructivists eschew notions of fixed state interests and constant international structures. Allowing for an increased role for agency, they point out that actors can often change their environment—for the better or for the worse. ~~Alexander Wendt puts it well: "if today we find ourselves in a self-help~~ world, this is due to process, not structure . . . structure has no existence or causal powers apart from process. . . . Anarchy is what states make of it."[74] Thus, international relations is seen as historically contingent—specific international structures are created in certain periods of history, such as the current system of sovereign states—and are potentially alterable at later periods. International structures, "although difficult to challenge, are not impregnable. Alternative actors with alternative identities, practices, and sufficient

72. For extended discussions of the end of the Cold War's impact on theorizing about international relations, see the special symposium "The End of the Cold War and Theories of International Relations," in *International Organization*, Vol. 48, No. 2 (Spring 1994), pp. 155–247; and Charles W. Kegley, Jr., "The Neoidealist Moment in International Studies? Realist Myths and the New International Realities," *International Studies Quarterly*, Vol. 37, No. 2 (June 1993).

73. For overviews of the constructivist approach, see Peter J. Katzenstein, Robert O. Keohane, and Stephen D. Krasner, "*International Organization* and the Study of World Politics," *International Organization*, Vol. 52, No. 4 (Autumn 1998), esp. pp. 674–678; Ted Hopf, "The Promise of Constructivism in International Relations Theory," *International Security*, Vol. 23, No. 1 (Summer 1998), pp. 171–200; and Dale C. Copeland, "Integrating Realism and Constructivism," paper presented at the Annual Meeting of the American Political Science Association, Boston, September 1998.

material resources are theoretically capable of effecting change."[75] The explicit constructivist goal is to explain international change over time, and it is this interest that makes them far more interested in history *qua* history.

This shared interest in the study of international change provides an important link between international historians and constructivist political scientists. As Richard Rosecrance points out, "historians typically seek to explain particular turning points in international or domestic history such as the causes of the French Revolution, the origins of World Wars I and II, and the causes of the Cold War." Historical arguments about international relations involve identifying decisive changes in international relations over time—historicizing means viewing the past as constructed, recognizing that international relations' categories and identities are not given and fixed, but made and remade.[76] The historian's enterprise involves showing what led to changes in international relations, and what resulted from those changes. Like constructivists, international historians are aware of the contingent nature of their generalizations—what at first look like repetitive patterns are often only recent behaviors and thus alterable.

Not only was the end of the Cold War a catalyst for the constructivist perspective, it also raised substantial dissatisfaction among realists over the adequacy of the structural or neorealist research program. Many realists have taken a decidedly "classical realist" turn.[77] Neorealists, who dominated the security studies subfield before the end of the Cold War, did not theorize about state motives and intentions, treating them as fixed. They also focused primarily on international outcomes, stripped of any purposive element. For example, neorealists argued that balances of power emerge and reemerge automatically, and that states balance against other states' accumulations of material power regardless of these states' intentions. It is partly for these reasons that international historians have found works based on the neorealist perspective so troubling. As Schroeder notes, neorealism's "insistence on the sameness effect and on the unchanging, structurally determined nature of international politics make it unhistorical, perhaps anti-historical."[78] By contrast, classical realists—and the neoclassical realists of today—emphasize foreign policy and decision-makers' perceptions rather than international patterns of behavior; argue that state intentions are variable instead of fixed; and suggest that international conflict can frequently be traced back to evil-minded leaders rather than to the tragedy of the situation. These are all premises that international historians would find attractive.

The central tenets of neoclassical realism—that the distribution of relative capabilities is only one factor affecting threat perceptions, and that the goals and purposes for which material power will be used are equally important—resonate well with the way many international historians understand international change and stability. As the following chapters show, For international historians war is always intended by someone and the key questions are whether power is used to manage the system and make states feel more secure, or whether its purpose is to threaten other states and undermine the rules of the game. So too for neoclassical realists, who reject the neorealist understanding of war as the result of decision-makers' misperceptions, shared situational constraints, or offensive military technologies. For both international historians and neoclassical realists in political science, war is not a tragedy but an evil. States do not go to war because of security dilemmas that arise in an anarchical international system, but because aggressor states seek to overturn the prevailing order.[79]

Political scientists are not historians, nor should they be. There are real and enduring epistemological and methodological differences that divide the two groups, and there is great value in recognizing, maintaining, and honoring those distinctions. It is helpful to have historical events analyzed by two groups of scholars, each trained to use distinct skills and to emphasize different aspects of a case. That being said, the chapters that follow demonstrate that despite some previous misreadings and misunderstandings, historians and political scientists have a lot to gain from continuing and deepening their dialogue.

The relationship between like-minded historians and political scientists promises to be valuable both in its own right, and as a way to offset increasing isolation within their own disciplines. In part this means a new appreciation of our comparative strengths, but it also requires transcending previous stereotypical and caricatured readings of the other subfield.

76. See Schroeder, "History and International Relations Theory," p. 67.

77. For an overview of recent works, see Gideon Rose, "Neoclassical Realism and Theories of Foreign Policy," *World Politics*, Vol. 51, No. 1 (October 1998), pp. 144–172.

78. Schroeder, "Historical Reality vs. Neo-realist Theory," p. 148.

79. For more on the distinction between tragedy and evil in the writings of neorealists and neoclassical realists, see Michael Spirtas, "A House Divided: Tragedy and Evil in Realist Theory," *Security Studies*, Vol. 5, No. 3 (Spring 1996), pp. 385–415.

Chapter 1

Explaining Events and Developing Theories: History, Political Science, and the Analysis of International Relations

Jack S. Levy

Historians and social scientists generally agree that although they study the same social phenomena, they do so in different ways. There is less agreement, however, on precisely what those differences are. The dialogue between historians and sociologists, which has continued for a century, is reflected in a 1994 symposium in the *British Journal of Sociology*, and the contrasting views of diplomatic historians and international relations theorists are presented in a 1997 symposium in *International Security*.[1] In this essay I focus primarily on differences in how diplomatic historians and political scientists study international relations—and my references to "historians" and "political scientists" should be interpreted in this way—although many of my arguments apply to the discipline of political science as a whole or to the social sciences more generally.[2]

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1. See *British Journal of Sociology*, Vol. 45, No. 1 (March 1994). This responds to John H. Goldthorpe, "The Uses of History in Sociology: Reflections on Some Recent Tendencies," *British Journal of Sociology*, Vol. 42, No. 2 (June 1991), pp. 211–230. See also *International Security*, Vol. 22, No. 1 (Summer 1997).

2. Although some have argued that international relations constitutes a distinct field of study, many now argue that any gap in theory and method between international relations and other empirically oriented fields in political science—particularly American politics and comparative politics—had diminished by the late 1990s, as scholars have increasingly incorporated theories of domestic politics into theories of international relations. See Helen V. Milner, "Rationalizing Politics: The Emerging Synthesis of International, American, and Comparative Politics," *International Organization*, Vol. 52, No. 4 (Autumn 1998), pp. 759–786.

The criterion that best defines the different "identities" of the two disciplines is a variation of the traditional distinction between idiographic and nomothetic orientations: the primary goal of historians is to describe, understand, and interpret individual events or a temporally and spatially bounded series of events, whereas the primary goal of political scientists is to generalize about the relationships between variables and, to the extent possible, construct law-like propositions about social behavior.[6]

In this chapter I develop this argument and show that the idiographic/nomothetic distinction underlies many of the other criteria that scholars have advanced to identify differences between the disciplines—including the value of parsimonious explanations, the importance of primary sources, the value of predictions and policy relevance, the feasibility of universal laws, the nature of the scope conditions that limit theoretical generalizations, the different types of scope conditions that define the domain of the theory, and the role of covering laws in social explanation. ~~Because the idiographic/nomothetic distinction subsumes~~ these other criteria, it is far more useful than any single criterion, and provides a comprehensive and powerful framework for analyzing the differences between the two disciplines. I also consider the distinction between narrative-based explanations and theory-based explanations, and conclude that there are too many exceptions to make this a useful criterion for distinguishing between history and political science.

In highlighting the importance of the idiographic/nomothetic distinction, I emphasize that idiographic does not imply atheoretical, that it is necessary to distinguish between what scholars try to explain and how they explain it, and that the two disciplines differ in *how* they use theory, not *whether* they use theory. To say that historians attempt to explain events does not imply that they are atheoretical, for historians sometimes use law-like propositions to explain those events. I also refine my earlier argument by acknowledging that historians often generalize. Most of these generalizations refer to particular countries or periods, whereas political scientists' generalizations refer to certain theoretically defined conditions. In other words, historians' generalizations are bounded by temporal and spatial scope conditions, whereas political scientists' generalizations are bounded by analytical scope conditions.

~~There is a smaller set of historians~~ who claim to generalize beyond the spatial and temporal bounds of their historical analyses, but here too they differ from political scientists. The difference is based on the distinc-

6. Jack S. Levy, "Too Important to Leave to the Other: History and Political Science in the Study of International Relations," *International Security*, Vol. 22, No. 1 (Summer

tion between the logic of discovery and the logic of confirmation, between constructing generalizations and validating them against the empirical evidence. Political scientists not only generalize from their observed data to a more broadly defined class of phenomena; they give primacy to the question of how to test those generalizations empirically and to the task of constructing research designs for that purpose. Historians sometimes generalize but they rarely give explicit attention to the research designs and methodologies through which their generalizations might be empirically confirmed.

Before developing this argument about the differences between history and political science in their study of international relations, it is necessary to recognize that neither discipline is monolithic. There is substantial variation in the scholarship within each discipline at a given point of time, within each discipline across national boundaries, and within each discipline over time. This significantly complicates the task of identifying any single criterion that fully captures the fundamental differences between historians and political scientists in their study of international relations.

First, history and political science both incorporate an enormous range of scholarly research, to the extent that in many respects the variations in theoretical approaches and methodological orientations within each discipline may be as great as the variations between them. Diplomatic historians and international relations scholars have far more in common with each other—in terms of substance, epistemology, and methodology—than they do with many of their colleagues in other fields in their own disciplines. Diplomatic history has been less sensitive than other branches of history to changing theoretical orientations and methodological fads in the field, from the rise and fall of quantitative history to the rise of postmodernism.[7] It has consistently insisted on the empirical validation of its interpretations and in the utility of narratives and primary sources for that purpose. These considerations lead Stephen Haber, David Kennedy, and Stephen Krasner to argue that diplomatic historians and international relations scholars are really "brothers under the skin."[8]

7. The international relations field in political science has generally reflected various trends in theory and method in the discipline as a whole, from quantitative methods to rational choice and game theory to the growing influence of constructivism.

8. Stephen H. Haber, David M. Kennedy, and Stephen D. Krasner, "Brothers Under the Skin: Diplomatic History and International Relations," *International Security*, Vol.

between historians and international relations scholars is more applicable to the United States than to European countries, where disciplinary identities and the relationships between them are often different. The study of international relations in the United Kingdom, for example, is less influenced by positivistic social science and more influenced by some elements of critical theory than it is in the United States, whereas the study of diplomatic history in the two countries is characterized by few fundamental differences.[9]

In addition, a succession of "great debates" and paradigmatic battles have helped structure the international relations field as a whole in the United States. The international relations field is less cohesive in the United Kingdom, France, and Germany, where local influences are greater. Finally, the growing influence of rational choice theory in the international relations field in the United States has reinforced the field's links with other key fields within political science. European international relations scholars have been more resistant to rational choice, in part because of their disciplinary associations with sociology, anthropology, and philosophy, and perhaps also because the individualistic foundations often associated with rational choice are less appealing to Europeans.[10]

A third consideration that complicates the task of comparison across disciplines is the fact that each discipline evolves over time with the rise and fall of competing paradigms within it. Although there are some striking parallels in the evolution of history and the social sciences over the course of this century, there are important points at which they diverge, so that the differences between the disciplines as well as their distinct identities have changed over time.[11]

History and political science were much closer in the 1960s—when leading schools of thought in each discipline were quite confident in the feasibility of "scientific" knowledge and in the utility of quantitative

9. While the study of international relations in the United Kingdom is more historical than it is in the United States, there is in many respects a greater separation between historians and international relations scholars in Britain than in the United States. See Christopher Hill, "History and International Relations," in Steve Smith, ed., *International Relations: British and American Perspectives* (Oxford: Basil Blackwell, 1985), pp. 126–145.

10. Milner, "Rationalizing Politics;" and Ole Wæver, "The Sociology of a Not So International Discipline: American and European Developments in International Relations," *International Organization*, Vol. 52, No. 4 (Autumn 1998), pp. 687–727.

11. On parallels between history and the social sciences see Keong-il Kim, "Genealogy of the Idiographic vs. the Nomothetic Disciplines: The Case of History and Sociology in the United States," *Review*, Vol. 20, Nos. 3–4 (Summer/Fall 1997), pp. 421–

methods for discovering that knowledge—than in 1980s, by which time the decline of quantitative history and the "revival of narrative" had moved history further away from political science. The two disciplines are even further apart today, after the growing influence of postmodernism and the "linguistic turn" in history, and after the further spread of quantitative methods and particularly game-theoretic models in political science.[12] Comparisons between the disciplines would have looked much different in the nineteenth century, when the Rankean focus on explaining unique events contrasted sharply with the attempts by Marx, de Tocqueville, Durkheim, Weber, and other sociologists to construct historically grounded generalizations about social structures and processes.[13] Thus any comparisons between history and political science may be historically contingent.

~~These significant variations in the scholarship within each disci~~pline—at any given point in time, over time, and across countries—complicate the task of identifying any essential differences between diplomatic history and international relations. Nevertheless, the central tendencies of the two disciplines differ, and most historians and political scientists agree that they differ.[14] I argue that the one criterion that best captures the differences between most leading historians and most leading international relations scholars—at least in the United States—that best reflects the scholarship that is most valued within each discipline, that involves the fewest significant exceptions, and that underlies many of the other criteria, is the idea that historians attempt to explain individual events or series of events, whereas political scientists attempt to construct generalizations (universal or contingent) about classes of events and to test those generalizations empirically. This argument represents a slight modification of the traditional distinction between idiographic and nomothetic approaches to the study of social phenomena.

The reader should understand that my generalizations about histori-

12. Lawrence Stone, "The Revival of Narrative: Reflections on a New Old History," in Lawrence Stone, *The Past and the Present Revisited* (London: Routledge and Kegan Paul, 1987); and John E. Toews, "Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience," *American Historical Review*, Vol. 92, No. 4 (October 1987), pp. 879–907. On the growing gap see Kim, "Genealogy," and the chapter by Robert Jervis in this volume.

13. Leopold von Ranke, "On the Character of Historical Science," in Georg G. Iggers and Konrad von Moltke, eds., *The Theory and Practice of History* (Indianapolis: Bobbs-Merrill, 1973).

~~ans and political~~ scientists refer to the central tendencies of the most influential "mainstream" scholars within each discipline, and that the large variances around these central tendencies mean that there will be numerous individual exceptions to my arguments. I limit my argument ~~to the United States because of significant~~ differences across the Atlantic.

The Idiographic/Nomothetic Distinction

In the late nineteenth century, Wilhelm Windelband and Heinrich Rickert emphasized the contrast between the idiographic method of the historical and social sciences (*Geisteswissenschaften*) and the nomothetic method of the natural sciences (*Naturwissenschaften*). The first aims to explain or understand unique sequences of events and the second seeks to develop explanatory laws. After social science diverged from history by adopting positivistic natural science as its disciplinary model, scholars began to apply the idiographic and nomothetic concepts to distinguish between history and the social sciences.[15]

Among contemporary political scientists, Joseph Nye asserts that "history is the study of events that have happened only once; political science is the effort to generalize about them." Similarly, Bruce Bueno de Mesquita argues that "the social scientist is more likely to emphasize general explanations of social phenomena, while the historian is more likely to emphasize particularistic, unique features of individual episodes of social phenomena." Stephen Van Evera argues that political scientists ~~see the task of explaining individual cases as~~ "the domain of historians."[16]

Sociologists make similar distinctions. Seymour Martin Lipset argues that "the task of the sociologist is to formulate general hypotheses . . . and to test them. . . . History must be concerned with the analysis of the particular set of events or processes." Edgar Kiser and Martin Hechter note that "historians' methodology stresses the accuracy and descriptive

15. Georg G. Iggers, *The German Conception of History*, rev. ed. (Middletown, Conn.: Wesleyan University Press, 1968), chap. 6. On the debate in psychology see Robert R. Holt, "Individuality and Generalization in the Psychology of Personality," *Journal of Personality*, Vol. 30, No. 3 (September 1962), pp. 377–404. See also the recent symposium on "Nomothetic vs. Idiographic Disciplines: A False Dilemma," *Review*, Vol. 20, Nos. 3–4 (Summer/Fall 1997).

16. Joseph S. Nye, "Old Wars and Future Wars: Causation and Prevention," *Journal of Interdisciplinary History*, Vol. 18, No. 4 (Spring 1988), p. 581; Bruce Bueno de Mesquita, "The Benefits of a Social Scientific Approach to Studying International Affairs," in Ngaire Woods, ed., *Explaining International Relations Since 1945* (New York: Oxford University Press, 1996), pp. 52–54; and Stephen Van Evera, *Guide to Methods for Stu-*

completeness of narratives about particular events . . . the events they seek to describe and explain—are both unique and complex." Robert Bierstedt notes that "history, as idiographic, is interested in the unique, the particular, the individual; sociology, as nomothetic, in the recurrent, the general, the universal."[17]

Similar views can be found among philosophers. Schopenhauer argued that "the sciences . . . speak always of kinds; history always of individuals." Isaiah Berlin explains that "in history we more often than not attach greater credence to particular facts than to general propositions" and that "the purpose of historians . . . [is] to capture the unique pattern and peculiar characteristics of its particular subject." Michael Oakeshott insists on the "absolute impossibility of deriving from history any generalization of the kind which belong to a social science," noting that "where comparison begins, as a method of generalization, history ends."[18]

This idiographic conception of what most historians do is also shared by many historians. In 1848 H.H. Vaughan stated that the first quality of a good historian was the "principle of attraction to the facts," and Leopold von Ranke argued that the aim of historians was to discover the unique in every event. For Ranke, "if generalizations were forced upon history . . . all that which is interesting about history would disappear" and "history would lose all scientific footing."[19]

Among contemporary historians, Lawrence Stone, citing Pierre Chaunu's comment that "the discipline of history is above all a discipline of context," argues that history "deals with a *particular* set of actors at a *particular* time in a *particular* place."

17. Seymour Martin Lipset, "History and Sociology: Some Methodological Considerations," in Seymour Martin Lipset and Richard Hofstadter, eds., *Sociology and History: Methods* (New York: Basic Books, 1968), pp. 22–23; Edward Kiser and Martin Hechter, "The Role of General Theory in Comparative-historical Sociology," *American Journal of Sociology* Vol. 97, No. 1 (July 1991), p. 2; and Robert Bierstedt, "Toynbee and Sociology," *The British Journal of Sociology,* Vol. 10, No. 2 (June 1959), pp. 96–97.

18. Schopenhauer is quoted in R.G. Collingwood, *The Idea of History* (Oxford: Oxford University Press, 1956), p. 167. See also Isaiah Berlin, "History and Theory: The Concept of Scientific History," *History and Theory,* Vol. 1, No. 1 (1960), pp. 9, 19; and Michael Oakeshott, *Experience and Its Modes* (New York: Cambridge University Press, 1990), p. 166, quoted in Thomas W. Smith, "Histories and the 'Science' of International Relations," paper presented at the annual meeting of the International Studies Association/South, Atlanta, Georgia, October 20–22, 1995, p. 13.

19. H.H. Vaughan, in the inaugural lecture of the Regius Professor of History at the University of Oxford, cited in Stone, *The Past and the Present Revisited,* p. 5; and von Ranke, "On the Character of Historical Science," p. 38.

Many professional historians—perhaps most—reject the idea that generalization is the goal of history. We all respond, in Marc Bloch's phrase, to "the thrill of learning singular things." Indeed, it is the commitment to concrete reconstruction as against abstract generalization—to life as against laws—which distinguishes history from sociology.[20]

Other historians recognize this as an accurate description of most historiography but argue that historians ought to be more nomothetic in orientation. In 1946, the Committee on Historiography of the Social Science Research Council concluded that for Americans "facts had become detached from any hypothesis or interpretation," and urged historians to generalize more by constructing testable hypotheses.[21] Similarly, Gordon Craig contends that historians should "overcome our congenital distrust of theory and our insistence upon the uniqueness of the historical event . . . and treat unique cases as members of a class or type of phenomenon." John Lewis Gaddis faults many historians in the security field for their lack of comparative focus and for their tendency "to preoccupy themselves with the particular."[22]

Some have interpreted the idiographic/nomothetic distinction and its application to history and social science to suggest that history, unlike the social sciences, tends to be atheoretical. This can be very misleading. We should not confuse the argument that (1) historians aim to explain particular sequences of events, whereas social scientists aim to generalize about classes of events with the assertion that (2) historians base their explanations on factors unique to an individual event or episode whereas political scientists base their explanations on theoretical models. These are two different statements. The first concerns the question of *what* we are trying to explain and the second concerns the question of *how* we explain it. I am *not* arguing that most historians are atheoretical. Rather, I am arguing that historians and political scientists tend to use theory in different ways.

20. Stone, *The Past and the Present Revisited,* p. 31; and Arthur Schlesinger, Jr., *The Bitter Heritage,* rev. ed. (Greenwich, Conn.: Fawcett, 1967), p. 90–91.

21. *Theory and Practice in Historical Study* (New York: Social Science Research Council, 1946), Bulletin 64, p. 31, cited in Kim, "Genealogy," p. 426. This SSRC report was followed by another, Louis Gottschalk, ed., *Generalization in the Writing of History* (Chicago: University of Chicago Press, 1963).

22. Gordon Craig, "The Historian and the Study of International Relations," *American Historical Review,* Vol. 88, No. 1 (February), pp. 1–11; and John Lewis Gaddis, "Expanding the Data Base: Historians, Political Scientists, and the Enrichment of Security Studies," *International Security,* Vol. 12, No. 1 (Summer 1987), p. 13.

THE MULTIPLE ROLES OF THEORY

Theories can be used both to explain generalized patterns of social behavior and to guide an interpretation of a particular episode or sequence of events. Although some historians attempt to explain singular events in terms of factors unique to those events, other historians resort to more general theoretical propositions or "covering laws" to explain those events.[23] The use of theory to explain singular events or sequences of events fits Lijphart's conception of an "interpretive" case study, Eckstein's notion of a "disciplined-configurative" case study, and Van Evera's idea of a "case-explaining case study," as opposed to a hypothesis-generating case study or a hypothesis-testing case study.[24]

The use of theory for explaining individual episodes or "cases" is also common in political science, though it is not as highly valued in the profession as theory construction and testing.[25] The highest professional rewards in political science go to scholars who develop pathbreaking theoretical frameworks or models. Some very influential books in the international relations field in the past half-century have relatively little empirical content other than illustrative material.[26] Studies that involve rigorous empirical tests of significant theories or hypotheses are also valued, and probably constitute the majority of articles published in the top journals in the field, at least in the United States.

23. On covering laws see Carl G. Hempel, "The Function of General Laws in History," *Journal of Philosophy*, Vol. 39 (1942), pp. 35–48.

24. Arend Lijphart, "Comparative Politics and the Comparative Method," *American Political Science Review*, Vol. 65, No. 3 (September 1971), pp. 682–693; Harry Eckstein, "Case Study and Theory in Political Science," in Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science*, vol. 7: *Strategies of Inquiry* (Reading, Mass.: Addison-Wesley, 1975), pp. 79–137; and Van Evera, *Guide to Methods*, pp. 74–75. It is interesting to note that political scientists, but not historians, often speak in terms of "cases." Nomothetically oriented political scientists conceive of a case not in its own terms but rather as an *instance* of a broader class of phenomena, one to which they want to generalize. On the diverse meaning of "case" in social science see Charles C. Ragin and Howard S. Becker, eds., *What Is a Case?* (New York: Cambridge University Press, 1992).

25. Van Evera (*Guide to Methods*, p. 75), who believes that case-explaining case studies are an important but neglected activity in political science, states that "political scientists seldom do case-explaining case studies, partly because they define the task of case-explaining as the domain of historians." Van Evera understates the number of case-explaining analyses in political science but accurately reflects the relatively low value attached to them in the discipline.

26. The clearest examples are Kenneth N. Waltz, *Man, the State, and War* (New York: Columbia University Press, 1959), and Waltz, *Theory of International Politics* (Reading,

historical case studies, but the cases are vehicles for theory development rather than ends in themselves. Significantly fewer rewards go to those who use theory to guide historical analyses for the primary purpose of illuminating the case (which is quite common), and even less to those who do "descriptive" (atheoretical) case studies (which has become less common).

For political scientists, the worst thing that can be said of a dissertation, job talk, or article is that it is primarily descriptive or that it makes little theoretical contribution, even if it adds to our body of empirical knowledge.[27] For historians, the worst thing that can be said is that a historical study or interpretation is incorrect, that it doesn't fit the facts, tie them together, and make them comprehensible. Political scientists are less concerned about "getting the facts right," and believe that theories can be useful even if they are descriptively inaccurate.[28] Historians are less concerned about explicitly specifying the assumptions and causal propositions underlying their historical interpretations.[29]

Just as political scientists complain that historians are not theoretical enough, or at least not explicit enough about their underlying assumptions and causal propositions, historians complain about political scientists' use (or abuse) of history. They argue that political scientists allow their theories to take priority over the evidence, focus on those historical

27. The notable exception is in area studies in the field of comparative politics, where "thick description" of the politics and culture of a country long dominated the field. The influence of area specialists has declined since its peak thirty years ago, however, and there has been a strong shift toward a more nomothetic orientation in the study of comparative politics in the 1990s. Robert H. Bates, "Area Studies and the Discipline: A Useful Controversy?" *PS: Political Science and Politics*, Vol. 30, No. 2 (June 1997), pp. 166–178.

28. The priority given to theory over historical accuracy is suggested by the fact that international relations theorists continued to assign Graham Allison's original treatment of alternative explanations of the Cuban Missile Crisis (*Essence of Decision: Explaining the Cuban Missile Crisis* [Boston: Little Brown, 1971]) in their graduate and undergraduate courses until the late 1990s, long after the release of new information left Allison's historical analysis badly out of date, and, in numerous places, simply inaccurate. These empirical problems have been corrected in Graham Allison and Philip Zelikow, *Essence of Decision: Explaining the Cuban Missile Crisis*, 2nd ed. (New York: Longman, 1999).

29. Some political scientists even emphasize the "heuristic value" of theory over its explanatory power. A theory can be useful if it generates new research questions and stimulates new approaches, even if it is weak on other grounds. James Rosenau once argued (in a talk at the University of Wisconsin in the early 1970s) that "bad theory is better than no theory." Although political scientists might debate this point, they would probably see more merit in Rosenau's argument than historians would find in

events that confirm their theories, and ignore the larger context in which events occur and in the absence of which those events cannot be fully understood. This is closely related to the charge by historians that the emphasis on constructing and testing theories in political science, reinforced by the reward structure in the discipline, leads political scientists to try so hard to confirm their theories that they are unreceptive to contrary evidence.

Historians contrast what they regard as the rigid use of "theory" in political science with their own preference for more flexible "hypotheses" (or theoretical "hunches") that guide historical research but that can be abandoned in the face of conflicting evidence.[30] Historians' conceptions of the rigid or dogmatic use of theory in political science are reflected in Isaiah Berlin's comment that an "addiction to theory—being doctrinaire—is a term of abuse when applied to historians; yet it is not an insult if applied to a natural scientist."[31]

One can undoubtedly find many cases in which political scientists cling rigidly to their theories in the face of substantial contrary evidence, or in which they focus only on those historical cases that fit their theories. But one can also find many examples of historians guilty of the same rigidities and biases in their case selection and interpretations.[32] Good scholars in each discipline, however, are sensitive to disconfirming evidence and critical of their colleagues who are not, and the question of whether one discipline is more guilty than the other of ignoring inconsistent evidence is less important than questions relating to the different criteria for disconfirmation in the two disciplines given their different scholarly purposes, methodologies, and data.

THE ISSUE OF OBJECTIVITY

Interdisciplinary debates about the proper role of theory are often unproductive because scholars use "theory" to mean many different things, ranging from a logically connected set of propositions deduced from

30. I thank David Fogelsong (private correspondence) for emphasizing this line of argument.

31. Berlin, "History and Theory," p. 9.

32. A.J.P. Taylor begins his study of the origins of modern wars since 1789 with an arbitrary exclusion of cases that do not fit his argument: "Two major wars—the American civil war of 1861 to 1865 and the Russo-Japanese war of 1904 to 1905, being fought entirely outside Europe, do not fall into the pattern of the others and I have therefore omitted them from my survey of how modern wars begin." A.J.P. Taylor, *How Wars Begin* (New York: Atheneum, 1979), p. 1. Noted in Thomas C. Walker, "Peace, Rivalry, and War: A Theoretical and Empirical Study of International Conflict" (Ph.D. dissertation, Rutgers University, chap. 8).

It is now the conventional wisdom in both history and political science that all empirical observations are filtered through a priori mental frameworks, that all facts are "theory laden." This is accepted by both practitioners and philosophers of social science and history. As Goethe wrote, "every fact is already a theory."[33]

Political scientists and historians agree in principle that one's theoretical preconceptions affect the question one asks, the data one selects to study, and the explanations that one constructs—of singular events as well as of more general patterns. J. David Singer, who constructed the most widely used data set in the study of international conflict, has repeatedly emphasized that data are "made" rather than simply collected. The historian E.H. Carr argues that facts are like "fish swimming about in a vast and sometimes inaccessible ocean; and what the fisherman catches will depend, partly on chance, but mainly on what part of the ocean he chooses to fish in and what tackle he chooses to use—these two factors being determined by the kind of fish that he wants to catch."[34] The disagreement between historians and social scientists is not so much over the influence of theoretical preconceptions, but rather in how explicit scholars should be about the analytic assumptions and causal propositions upon which their explanations of social phenomena are based. Political scientists are far more concerned than historians about making their assumptions and causal propositions explicit.

Historians have not always believed that empirical facts are interpreted through intervening mental frameworks. The leading school of historiography by the later part of the nineteenth century was the "scientific history" of Leopold von Ranke and his followers. Ranke insisted that the aim of the historian was to show history "as it really was," to recreate the past that exists independently of the preconceptions and prejudices of the historian, and to achieve value-free, scientific certainty. The aim was not just to get the facts right, but to understand how discrete facts were interconnected. For Ranke this involved the hermeneutic method, the critical analysis of texts with particular emphasis on primary sources, including diplomatic documents, memoirs, diaries, letters, and the like.[35]

33. Goethe is cited in Kenneth N. Waltz, "Evaluating Theories," *American Political Science Review*, Vol. 91, No. 4 (December 1997), p. 913.

34. J. David Singer, "Data-Making in International Relations," *Behavioral Science*, Vol. 10, No. 1 (January 1965), pp. 68–80; and E.H. Carr, *What Is History?* (Harmondsworth, UK: Penguin, 1964), p. 23.

35. Ranke, "The Character of Historical Science"; Georg G. Iggers, "The Image of

This view of understanding history "as it really was" is implicit in John Goldthorpe's argument that the distinctive difference between history and sociology (and by implication the social sciences more generally) is that historians *discover* evidence while sociologists *invent* evidence.[36] We might call this the "Dragnet" conception of history: "Just the facts, ma'am, just the facts."[37]

Historians soon reacted against the Rankean idea of an objective, value-free history.[38] Critics pointed out that Rankean history assumed the centrality of the state and focused narrowly on political history to the exclusion of social, economic, and cultural history. Its methodology utilized national archives as its main sources. The substance of Rankean historiography varied from country to country, primarily in a way that reflected their separate political cultures, mythical national pasts, and perceived threats to the state. In Germany, for example, the opposition to social history was clearly linked to the fear of democratization.[39]

Historical "idealists" have long argued that the study of history reflects the preconceptions of the historian and the social context in which she writes rather than any objective reality.[40] Oakeshott, reflecting the

Ranke in American and German Historical Thought," *History and Theory,* Vol. 2, No. 1 (1962), pp. 17–40; and Georg G. Iggers, *New Directions in European Historiography,* rev. ed. (Middletown, Conn.: Wesleyan University Press, 1984). See also Richard J. Evans, *In Defense of History* (New York: W.W. Norton, 1999), chap. 8.

36. Goldthorpe, "The Uses of History in Sociology," p. 214.

37. The same charge—of underestimating the role of theoretical preconceptions underlying all observation and believing in the possiblity of a value-free science—could be leveled against some strands of political science, particularly during the behavioral revolution in the 1960s. But just as Rankean epistemology has gone out of favor, so has the atheoretical "number-crunching" that characterized cruder forms of quantitative analysis.

38. The theoretical ideas subsumed in Ranke's *Aussenpolitik* are still quite influential, both among realist international theorists and their counterparts in diplomatic history.

39. Iggers, *New Directions,* chap. 1.

40. For example, "progressive historians" in the United States reflected the democratic values of the progressive era. See, for example, Charles A. Beard and Mary R. Beard, *America in Midpassage* (New York: Macmillan, 1939). Most contemporary cultural and postmodern historians study the past "from the bottom up," from the perspective of the powerless, the voiceless, the marginalized, and with a clear normative bias in favor of these groups. For an argument on why the study of the "voiceless" lends itself to a postmodern orientation, see Haber, Kennedy, and Krasner, "Brothers Under the Skin," pp. 38–40. But this link is far from perfect. Some postmodernists study elites, and though it is true that it is difficult to apply Rankean methods in the absence of "documents," one can certainly study the powerless from a more positiv-

historical idealist perspective, argued that "history is a historian's experience. It is 'made' by nobody save the historian; to write history is the only way of making it." Similarly, Carl Becker wrote that "the facts of history do not exist for any historian till he creates them."[41]

Contemporary "constructivists" share this idealist perspective. So do postmodernists, but in a more extreme way. Postmodernists reject the possibility of objective knowledge because the very concepts the analyst uses to describe the world are fundamentally shaped by their cultural context, by power relationships, and by language. For postmodernists, language is a self-contained system that exists independently of its relation to the external world. The text does not reflect reality but instead constructs that reality. In Jacques Derrida's words, "there is nothing outside of the text." As Hayden White argues, "historical narratives are verbal fictions, the contents of which are as much *invented* as *found* and the forms of which have more in common with their counterparts in literature than they have with those in the sciences."[42] Postmodernists reject the distinction between fact and fiction, between history and rhetoric, and thus reject empirical accuracy as a criterion for the evaluation of theories.[43]

E.H. Carr offered a powerful but balanced statement of the theory-laden character of all empirical observation, rejecting the extremes of both Rankeans and historical idealists. Carr criticized the "fetishism of facts" in Rankean historiography and emphasized that all observation involves the "selective filtering of the facts." He also criticized the exclusive reliance on the documents, "the Ark of the Covenant in the temple of facts" in Rankean historiography. At the same time, however, Carr rejected the idealists' argument that empirical observations were entirely determined

41. Oakeshott, *Experience and Its Modes,* p. 99; Carl Becker, writing in *Atlantic Monthly* (October 1910), p. 528, cited in Carr, *What Is History?* (London: Macmillan, 1961) p. 21. See also Collingwood, *The Idea of History.*

42. Derrida and White are each cited in Georg C. Iggers, *Historiography in the Twentieth Century: From Scientific Objectivity to the Postmodern Challenge* (Hanover, N.H.: Wesleyan University Press, 1997), pp. 9, 118–119.

43. This distinguishes postmodernists from "softer" constructivists, who recognize limitations on the transhistorical and transcultural validity of theoretical concepts but who are more open to the empirical validation of particular historical interpretations. The possibility of generalizing across cases is contested territory among constructivists (or interpretivists). Ted Hopf argues that "interpretivists are most hesitant to ever generalize across cases, and see even within-case generalizations to be problematic," but then goes on to emphasize the limits of particularism as well as universalism. See Ted Hopf, "The Limits of Interpreting Evidence" (Ohio State University, unpublished manuscript, 2000).

by theoretical preconceptions. Carr argued that "the historian is neither the humble slave nor the tyrannical master of his facts," and that history is "a continuous process of interaction between the historian and his facts, an unending dialogue between the present and the past."[44]

This middle ground between a Rankean positivist and historical idealist viewpoint is one with which many contemporary diplomatic historians and political scientists would feel quite comfortable. Haber, Kennedy, and Krasner argue that "the interplay of fact and theory has been the defining characteristic of the study of international politics. . . . Social behavior can be objectively observed even if it is based on intersubjectively shared understanding." James Lee Ray and Bruce Russett argue that although "observations are inevitably theory-*laden* they are not theory-*determined*." Most political scientists would accept Carr's argument that the scholar engages in a "continuous process of molding his facts to his interpretation and his interpretation to his facts," that there is an unending dialogue between theory and evidence.[45]

The idiographic/nomothetic distinction—defined in terms of what is to be explained rather than how to explain it—underlies the logic of several of the other criteria of demarcation between the disciplines, including the relative preferences for parsimony, the role of primary and secondary sources, the importance of prediction and policy relevance, beliefs in the feasibility of universal laws, the nature of scope conditions of generalizations, and the role of covering laws. By linking these other criteria within an overarching framework, the idiographic/nomothetic distinction gains considerable analytic power.

PREFERENCES FOR PARSIMONY

Few would disagree that political scientists are far more interested than are historians in "parsimonious" theories and explanations. By this I mean that political scientists attempt to explain as much as possible with as little theoretical apparatus as possible. They prefer one theory to another if the first explains as much empirical phenomena as the second but with fewer assumptions.[46] Historians prefer "total" explanations that

44. Carr, *What Is History?* pp. 20–21, 26–30.

45. Haber, Kennedy, and Krasner, "Brothers Under the Skin," pp. 36–37; and James Lee Ray and Bruce Russett, "The Future as Arbiter of Theoretical Controversies: Predictions, Explanations, and the End of the Cold War," *British Journal of Political Science*, Vol. 26, No. 4 (October 1996), pp. 441–470. Carr, *What Is History?* p. 29.

46. This is the conventional use of parsimony in political science. A theory is not parsimonious in the abstract but only relative to other theories that purport to explain the same phenomenon. Preferences for parsimonious theories go back to Occam's

recognize the complexity in the world and attempt to explain much of that complexity in their interpretations in order to account for a set of events in their entirety.[47]

The nature of total explanations varies by historical school. For idealists, it involves *verstehen*, an empathetic understanding of the beliefs, emotions, intentions, reasoning, and very personality of the actors themselves in an attempt to understand the meanings individuals attached to their own actions.[48] This is associated with the idea that the historian aims at understanding and interpretation rather than causal explanation.[49] The concepts of total explanation and "understanding" take us far from the more parsimonious theorizing of most political science.[50]

Many political scientists also recognize complexity in the world, but attempt to abstract from that complexity to explain the most fundamental features of social phenomena. The preference for parsimony derives from the goal of theorizing about relationships between classes of events rather than explaining individual events, and the belief that theoretical generalization must be based on models that are considerably less complex than the world they aim to represent. The more complex and nuanced an explanation, the less likely that it will "travel well" across cases. No two cases are exactly alike, and the more one explains what is unique to a particular case, the less one can use the same conceptual apparatus to explain the essential features of another case.

Razor from the fourteenth century and to Karl Popper's argument that simpler theories are easier to falsify and consequently they contain more explanatory power. See Karl Popper, *The Logic of Scientific Discovery* (New York: Harper Torchbacks, 1965). In this view parsimony relates to theories that one constructs to explain the world, not to beliefs about the simplicity of the world itself, which is an alternative conceptualization of parsimony. This alternative view is adopted by Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inquiry in Qualitative Research* (Princeton: Princeton University Press, 1994), who refer to the first conception of parsimony as "maximizing leverage." See also the chapter by Jervis in this volume.

47. As Eric Hobsbawm argues, in *On History* (New York: The New Press, 1997), p. 109, "basically all history aspires to what the French call 'total history'." By this he means that "history . . . cannot decide to leave out *any* aspect of human history *a priori* . . . ," that ideally all aspects of an episode must be included in a historical explanation.

48. Collingwood, *The Idea of History;* Benedetto Croce, *History: Its Theory and Practice* (New York: Russell and Russell, 1960); and William Dilthey, *Meaning in History* (London: Allen & Unwin, 1961).

49. Historians themselves debate the utility of this distinction. See Roberts, *Historical Explanation*, chap. 11.

50. An important exception is constructivism, which shares an interest in the complex social contexts of human behavior and the meanings individuals attach to their actions. See Martin Hollis and Steve Smith, *Explaining and Understanding International Relations* (Oxford: Clarendon Press, 1990); and Hopf, "The Limits of Interpreting Evidence."

Given their interest in constructing parsimonious theories and explanations, political scientists often complain that the nonparsimonious explanations of historians, area specialists, and others tend to be overdetermined—in that the analyst advances more causes for an outcome than are needed to explain it. To the political scientist, this represents a failure to differentiate primary from secondary causal factors and diminishes the analytical power of the argument and the ability to generalize to other cases. Historians, on the other hand, often complain that the so-called parsimonious explanations of political scientists are underdetermined—they fail to capture the nuances of individual events or periods, and they also fail to explain the variation across historical episodes.[54]

integrating them into an overarching theoretical structure increases the likelihood of logical inconsistencies and contradictions among different theoretical propositions, and is an important line of criticism that deductively-oriented theorists make of both historians and inductively-oriented political scientists.[59]

It is useful to contrast the "toolbox" conception of theory with the norm that has developed in political science for scholars to analytically distinguish their own theories (or explanations of individual cases) from competing theories or explanations and to explicitly test their theory against the leading alternatives. Many case studies in political science, for example, are organized around competing theories rather than a single narrative. To the extent that historians deal with competing theories in their narratives, this is much less explicit and rarely serves as an organizing device. The political scientist's preference for pitting theory against competing theory rather than integrating elements from different perspectives into a single, more complex theory is consistent with the goal

57. Melvyn P. Leffler, "New Approaches, Old Interpretations, and Prospective Reconfigurations," *Diplomatic History*, Vol. 19, No. 2 (Spring 1995), p. 179.

58. The "toolbox" metaphor comes from Edgar Kiser, "The Revival of Narrative in Historical Sociology: What Rational Choice Theory Can Contribute," *Politics and Society*, Vol. 24, No. 3 (September 1996), p. 258.

59. Isaiah Berlin makes a similar argument (in "History and Theory," p. 9) when he says that the "crucial difference" between history and the natural sciences is that "the generalizations of history, like those of ordinary thought, are largely unconnected." Waltz has something like this in mind when he describes the evidence that Paul Schroeder compiles against neorealist theory as "a melange of irrelevant diplomatic lore," though Schroeder's work has been quite influential in political science, and rightfully so. See Paul W. Schroeder, "Historical Reality vs. Neo-Realist Theory," *International Security*, Vol. 19, No. 1 (Summer 1994), pp. 108–148; and Waltz, "Evaluat-

of making theories as parsimonious as possible. One can debate, however, whether this increases our understandings of individual cases.

Political scientists agree with historians that no single theory can provide a complete explanation of a set of events. Unlike historians, however, political scientists have no interest in providing complete explanations. They only want to explain theoretically relevant aspects of the case, as determined by their own conceptual framework, and to generalize to the broader universe of all comparable cases.[60]

This difference between attempting to maximize descriptive accuracy in a particular case and insisting upon a more parsimonious theory to facilitate generalization reflects the basic tradeoff between internal validity and external validity—between providing an exact and precise explanation of a particular "case" or set of events or data, and providing a reasonable basis for generalizing beyond the data to other similar instances of the same class of events. Historians give primacy to internal validity, while political scientists are willing to sacrifice some internal validity in order to increase external validity.[61]

PRIMARY AND SECONDARY SOURCES

The different tradeoffs historians and political scientists make between internal and external validity, which derive from their respective idiographic and nomothetic aims, helps explain the emphasis each places on primary sources. Historians have traditionally insisted on the central importance of primary sources, while political scientists have been more willing to rely on secondary sources based on the work of historians.

One problem that political scientists must confront in their use of secondary sources is that the implicit (or explicit) theoretical questions that guided the historian's study may have been quite different from the questions the political scientist wants to answer, and this may limit the utility of particular secondary sources for the political scientist. This mismatch between theory and data, along with other considerations, leads Deborah Larson to call for political scientists to rely less on histo-

60. Area specialists constitute an important exception.

61. On different conceptions of validity, see Thomas D. Cook and Donald T. Campbell, *Quasi-Experimentation* (Chicago: Rand McNally, 1979), chap. 2. This discussion of tradeoffs suggests the potential utility of multi-method approaches to social and political analysis, in which the combination of two or more methods can help to compensate for the limitations of any single method. The combination of case study and statistical or game-theoretic methods (or both) has become more common in political science, and its potential utility is demonstrated by research on the democratic peace.

rians' secondary sources and to do more archival work themselves in the construction and testing of their theories.[62]

This may be good advice in principle, assuming that the types of data that one's theory calls for are available in the archives. But an important practical problem arises from the kinds of research designs that political scientists construct for the purposes of theoretical generalization, which require a test of the theory either against a large number of cases in a quantitative study, or against a more modest number of cases for the purposes of controlled comparison. Either way, it is simply not possible for a single scholar to engage in a thorough investigation of all available primary sources for each case. This is particularly true given diplomatic historians' recent emphasis on the use of multi-archival sources from different countries. As Theda Skocpol notes with respect to historical sociology, "a dogmatic insistence on redoing primary research for every investigation would be disastrous; it would rule out most comparative-historical research."[63]

A second problem in the use of secondary sources is the potential for selection biases. Given the large number of secondary sources from which to choose, how does the analyst select which to use or to rely upon most heavily? The analyst may be drawn to precisely those sources that reflect her own theoretical preconceptions, which precludes a fair test of the author's theory against alternative explanations.[64] It may be possible, however, for the comparative researcher to minimize these selection biases by securing advice from several leading historians regarding the major debates among historians, the best secondary sources, and the analytical biases of particular historians.

It is not clear that the problem of selection bias in the use of secondary sources is any more serious than the potential biases that affect the analyst who works alone in the archives. There is no perfect solution here. Insisting that the political scientist work the archives and in addition read all relevant secondary sources, and do this for enough cases to facilitate the ability to generalize, is impractical. Insisting that researchers using both primary and secondary sources be more sensitive to the potential biases in their sources and in their own minds, and more cognizant of the wide range of interpretations in various secondary sources, while helpful, does not fully eliminate the problem.

PREDICTION AND POLICY IMPLICATIONS

The nomothetic/idiographic distinction also helps explain why political scientists are generally more interested than historians in prediction and possibly also in the utility of scholarship for statecraft.[65] Gaddis argues that with respect to prediction (or at least policy implications), "most historians shy from these priorities like vampires confronted with crosses. Many political scientists embrace them enthusiastically."[66] Edward Ingram argues that "political scientists are interested in the past only as it affects the present. The past interests historians for itself." He also maintains that "for political scientists, what matters is not what mattered at the time but what contributes to what will matter later on."[67]

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62. See Deborah Welch Larson's chapter in this volume. See also Gaddis, "Expanding the Data Base." Political scientists often use some primary sources in doing historical case studies. Quantitative studies based on content analysis also rely heavily on primary sources. One example of the latter is the Stanford 1914 Project, directed by Robert North in the 1960s and 1970s. For a review see Francis W. Hoole and Dina A. Zinnes, eds., *Quantitative International Politics: An Appraisal* (New York: Praeger, 1976), part V.

63. Theda Skocpol, "Emerging Agendas and Recurrent Strategies in Historical Sociology," in Theda Skocpol, ed., *Vision and Method in Historical Sociology* (New York: Cambridge University Press, 1984), p. 382.

64. Ian S. Lustick, "History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias," *American Political Science Review*, Vol. 90, No. 3 (September 1996), pp. 605–618; and Paul W. Schroeder, "History and International Relations Theory: Not Use or Abuse, but Fit or Misfit," *International Security*, Vol. 22, No. 1 (Summer 1997), p. 71. The general problem, as Skocpol ("Emerging Agendas," p. 382) has noted, is that "comparative historical sociologists have not so far worked out clear, consensual rules and procedures or the valid use of secondary sources as evidence."

65. These differences over the importance of policy relevance may have deep historical roots. Once the natural sciences had become associated with technological progress, the social sciences, struggling to establish a disciplinary identity distinct from history, sought legitimation by emphasizing its pragmatic and policy-relevant side. See Kim, "Genealogy," pp. 423–428.

66. John Lewis Gaddis, "History, Theory, and Common Ground," *International Security*, Vol. 22, No. 1 (Summer 1997), p. 84. Gaddis argues, however, that while we cannot predict the future, we can prepare for it (pp. 84–85), and that an understanding of the past is one form of training that helps us prepare for the future. The implication is that we can better understand which events are more likely to occur than others. While Gaddis clearly rejects the idea of making "point predictions" about the future, his comment about preparing for the future is not dissimilar to political science predictions based on statistical probabilities.

67. Edward Ingram, "The Wonderland of the Political Scientist," *International Security*, Vol. 22, No. 1 (Summer 1997), pp. 54–55. In contrast to Ingram, however, it has also been said that "History is the use the present makes of the past for the sake of the future." Cited in *New York Times*, January 1, 2000, p. A1.

attitudes toward prediction. He states that historians "privately regard history as its own reward; they study it for the intellectual and aesthetic fulfillment . . . but for no more utilitarian reason. They understand better than outsiders that historical training confers no automatic wisdom in the realm of public affairs." Yet a page later he argues that generalizations however defective, are possible, and that they "can strengthen the capacity of statesmen to deal with the future."[68]

It is probably true that political scientists are more interested in prediction than are historians. Some people undoubtedly choose to become political scientists rather than historians precisely because they want to influence policy and because the generalizing aims of political science are more conducive to prediction than the particularizing tendencies of history. This does not necessarily imply, however, that political scientists are always more influenced by contemporary policy concerns than are historians. All historiography involves, to some extent, seeing the past through the eyes of the present. As Benedetto Croce argued, "all history is contemporary history." Similarly, Frederick Jackson Turner wrote that "each age writes the history of the past anew with reference to the conditions uppermost in its own time."[69]

In fact, it is often more difficult to identify the social, political, and cultural biases in theoretical models in contemporary social science, which often prides itself on its "objectivity," than in the work of historians, which can be quite evaluative. As Ingram argues, historical narratives tell a story, and a morality play is often part of the story.[70] The influence of these analytic and normative biases on historical studies does not necessarily imply, however, that these studies are conducted or written in such a way that might generate specific future predictions or policy prescriptions that are well grounded in either theoretical logic or historical evidence.[71]

There is another reason why political scientists are interested in

68. Schlesinger, *Bitter Heritage*, pp. 90–91.

69. Croce cited in Carr, *What Is History?* pp. 20–21; and Frederick Jackson Turner, "The Significance of History," in *The Early Writings of Frederick Jackson Turner* (Madison: University of Wisconsin Press, 1891/1938), p. 52.

70. See Edward Ingram's chapter in this volume; Robert Jervis and Paul W. Schroeder also make similar points in their chapters.

71. It is interesting that some of the historical studies with the greatest policy relevance have had greater impact on international relations scholars than on historians. One example might be Paul M. Kennedy, *The Rise and Fall of the Great Powers: Economic Change and Military Conflict from 1500 to 2000* (New York: Random House, 1987).

prediction. By using a theory to make predictions and then testing the accuracy of those predictions, one can ensure that a theory is tested against data that played no direct role in the generation of the theory.[73] The aim is to avoid the common error of using the data to generate a theory and then using that same data to test the theory. For this purpose prediction refers not only to forecasts about future events, but also to "predictions" of past events that are unknown to the analyst, or at least that played no direct role in the formulation of his theories. Such predictions are often referred to as postdictions or retrodictions.[74] The importance of postdictions springs from the scientific imperative to derive from a theory as many testable implications as possible, in as many varied temporal and spatial domains as possible, and to subject those predictions to multiple empirical tests.[75]

The methodological mandate to avoid testing a theory with the same data that were used to construct the theory raises a particular problem for historical interpretation. Historical narratives are always written with a knowledge of the outcome of the story, but this raises the danger that the known outcome influences the interpretation of chronologically earlier events. As C.V. Wedgwood wrote, "History is lived forward, but it is written in retrospect. . . . We know the end before we consider the beginning and we can never wholly recapture what it was to know the beginning only."[76]

73. This is Milton Friedman's argument for the importance of prediction in "The Methodology of Positive Economics," in Milton Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953). It is also the basis of Imre Lakatos's argument that the prediction and confirmation of "novel facts" is a central component of scientific progress. See Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (New York: Cambridge University Press, 1970), pp. 91–196. For a review of debates regarding exactly what constitutes a "novel fact" see Colin Elman and Miriam Fendius Elman, "Appraising Progress in International Relations Theory: How Not to Be Lakatos Intolerant," unpublished manuscript, 1999.

74. See Ray and Russett, "The Future as Arbiter," p. 447.

75. King, Keohane, and Verba, *Designing Social Inquiry;* and Van Evera, *Guide to Methods*. The need to make predictions in unfamiliar domains is emphasized by Nobel Prize–winning physicist Richard Feynman: "If you will never say that a law is true in a region you have not already looked you do not know anything. If the only laws that you find are those which you have just finished observing then you can never make any predictions." See Richard Feynman, *The Character of Physical Law* (Cambridge, Mass.: MIT Press, 1965), p. 76.

I have argued that to the extent that historians attempt to generalize, their generalizations are usually restricted to a well-defined period.[85] The conditional nature of the historian's generalizations does not differentiate her from the social scientist, because many social science generalizations are conditional rather than universal, so that the social scientist must specify the "scope conditions" under which her generalizations or theory is valid. The difference lies in how the limiting conditions on generalizations are specified. Historians use temporal and spatial criteria whereas social scientists use analytical criteria, as contained in the explicit assumptions underlying their theories. The social science emphasis on analytical scope conditions is expressed in the injunction that in their theoretical propositions scholars should replace the identity of countries, places, and dates with conceptual variables.[86]

While most political scientists conceive of generalizations in terms of relationships between conceptual variables, many historians do not. They are willing to speak of "generalizations" about particular periods, particular countries, or even particular individuals, and treat these generalizations as fully valid and law-like within particular spatial and temporal bounds.[87] Such generalizations are based on knowledge of the period (or country or individual) rather than on universal or contingent covering laws.

This is the argument advanced by N. Rescher and O. Helmer for the role of explanatory laws in historical cases. They argue that statements like "heretics were persecuted in seventeenth-century Spain" are law-like generalizations.[88] An even more restricted form of generalization would be what Gilbert Ryle calls "dispositional explanations" of the behavior of a particular individual, which he claims are law-like in nature. Ryle gives the example of the statement "Disraeli was ambitious."[89]

While philosophers of history debate whether statements like this are restricted law-like generalizations,[90] political scientists would generally reject such arguments. They generalize not about particular countries or individuals, but rather about *kinds* of countries or individuals. They might generalize about countries *like* seventeenth-century Spain, defined in terms of certain political, social, or cultural characteristics. They might generalize about the motivations of individuals *like* Disraeli, and say that individuals with certain personalities, social backgrounds, or belief systems tend to behave in predictable ways. But political scientists prefer not to generalize about particular countries or individuals, and they would not claim that such statements were law-like generalizations.[91] These different meanings that historians and political scientists attach to the notion of generalization helps to explain their strong differences regarding their respective answers to the question of how much historians generalize.

This difference in the specification of scope conditions in history and in political science is revealed in the titles of some of the more influential recent books in peace, war, and security in the two disciplines. The inclusion or noninclusion of spatial and temporal scope conditions in the title provides a strong indicator of the author's disciplinary affiliation.[92]

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85. This is reflected in some historians' response to questions about other countries or other periods: "It's not my period." Comparative history provides significant exceptions.

86. Adam Przeworski and Henry Tuene, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970). Kiser ("Revival of Narrative," pp. 256–259) differentiates between historical and abstract scope conditions.

87. This discussion builds on William Dray, "The Historical Explanation of Actions Reconsidered," in Patrick Gardner, ed. *The Philosophy of History* (London: Oxford University Press, 1974), pp. 66–89, especially pp. 80–83.

88. N. Rescher and O. Helmer, "On the Epistemology of the Inexact Sciences," *Management Science*, Vol. 5 (October 1959), pp. 25–40; see also N. Rescher and C. B. Joynt, "The Problem of Uniqueness in History," *History and Theory*, Vol. 1, No. 2 (1961), pp. 150–162.

89. Gilbert Ryle, *The Concept of Mind* (London: Hutchinson, 1966), chap. 6.

90. Dray, "Historical Explanations of Actions Reconsidered," pp. 80–84.

91. An interesting exception is James N. Rosenau's argument that we can have "single-country" theories of foreign policy, in "Toward a Single-Country Theory: The USSR as an Adaptive System," paper presented at the Conference on Domestic Sources of Soviet Foreign and Defense Policies, University of California at Los Angeles, 1985.

92. Consider the following books by historians contributing to this volume: Carole Fink, *The Genoa Conference: European Diplomacy, 1921–1922* (Chapel Hill: University of North Carolina Press, 1984); Edward Ingram, *The Beginning of the Great Game in Asia, 1828–1834* (New York: Oxford University Press, 1979; John A. Lynn, *Giant of the Grand Siècle: The French Army, 1610–1715* (Cambridge: Cambridge University Press, 1997); Stephen E. Pelz, *Race to Pearl Harbor: The Failure of the Second London Naval Conference and the Onset of World War II* (Cambridge, Mass.: Harvard University Press, 1974); Schroeder, *The Transformation of European Politics, 1763–1848;* and Gerhard L. Weinberg, *A World at Arms: A Global History of World War II* (Cambridge: Cambridge University Press, 1994). Books by political scientists contributing to this volume include Miriam Fendius Elman, ed., *Paths to Peace: Is Democracy the Answer?* (Cambridge, Mass.: MIT Press, 1997); Robert Jervis, *Perception and Misperception in International Politics;* Richard Ned Lebow, *Between Peace and War* (Baltimore: Johns Hopkins University Press, 1981); Jack S. Levy, *War in the Modern Great Power System, 1495–1975* (Lexington: University

THE ROLE OF THEORY IN HISTORY

The hypothesized dichotomy between narrative and theory implies that historians mainly write narratives and that these narratives are atheoretical. This is misleading, as many narratives are guided by a well-defined theoretical perspective, and several important schools of historiography do not utilize narratives.

Although political scientists are generally more explicit about their analytical assumptions than historians are, there are many important historical paradigms that are quite explicit in the assumptions and causal laws upon which their frameworks and interpretations are based. Some of the best examples can be found in Marxist economic history, the *Annales* school in France, or the "New Economic History" in the United States.[111] These approaches clearly reject the view of history as narrative containing its own explanation and seek to base historical explanations on theories and causal laws from the social sciences and to demonstrate their validity through methods that most social scientists would find acceptable. *Annales* historians, for example, are explicit in their conception of a causal hierarchy that consists of a fundamental level of geographic, climatic, biological, and economic factors; a second level of enduring social structures; and a ephemeral level of political events, religion, culture, and intellectual developments.[112] There are numerous other examples of historical studies that are more nomothetic than idiographic.[113]

Although these historical paradigms have for the most part focused on social and economic history, one can find a number of studies of diplomatic history that organize their historical data around analytic categories instead of (or perhaps in conjunction with) chronological narratives. Gaddis's analysis of the long peace since World War II focuses on several alternative theoretical explanations. In form it is indistinguishable from the work of political scientists, and in substance it was the first good theoretical study of that topic in either discipline. James Joll begins his study of the origins of World War I with a brief discussion of alternative interpretations, follows with a narrative overview of events of the July crisis, and then organizes the bulk of the book around theoretical variables.

112. Braudel, *The Mediterranean and the Mediterranean World.*

113. Two that come to mind are William Hardy McNeill, *Plagues and Peoples* (New York: Anchor, 1989), and *Keeping Together in Time: Dance and Drill in Human History* (Cambridge: Cambridge University Press, 1995).

Other examples include Paul Kennedy's study of imperial overextension and the rise and fall of great powers, and Arno Mayer's work on the domestic sources of war.[115]

The role of theory is also quite explicit and quite influential in postmodernist history, but here theory takes on a different form. History after the "linguistic turn" is theoretical,[116] but it a theory influenced by literary criticism and symbolic anthropology and not by theories of social science.[117] Although postmodernism has had a substantial influence on the philosophy of history, it has had less influence on the actual writing of history. There are relatively few postmodern diplomatic histories, for postmodern historians have shifted the focus from political, diplomatic, and even economic history to questions of culture, *mentalité*, and subalternity.[118] Many diplomatic historians argue, however, that the rise of postmodernism in history has contributed to the declining influence of diplomatic history within the discipline, at least in the United States.[119]

The influence of postmodernism on political science has been much more limited, though "softer" forms of constructivism that are open to an empirical research agenda are growing in influence. Unlike their colleagues in history, however, critical theorists, feminists, and constructivists in political science have given considerable attention to international relations. The early focus was on metatheoretical and methodological issues, but that has begun to change, and empirically oriented studies are now more common.

116. See Toews, "Intellectual History after the Linguistic Turn."

117. An interesting anecdote on the influence of "theory" on history is provided by Gordon Schochet. On meeting a good friend who had returned from a summer of research in London, Schochet said, "it must have been very difficult and crowded at the British Library. I hate doing research in London in the summer, what with all those Americans climbing all over one another." His friend, an intellectual historian, replied: "No, it wasn't bad at all; there was hardly anyone there. No one's using books any more. They're all doing theory." Gordon Schochet, "Where Have All the Historians Gone?" Presented at the Annual Meeting of the American Society for Eighteenth-Century Studies, Austin, Texas, 1996.

118. This is slowly beginning to change. For recent constructivist or postmodern approaches to diplomatic history, see the symposium on "Culture, Gender, and Foreign Policy," *Diplomatic History*, Vol. 18, No. 1 (Winter 1994); and Frank Costigliola, "Unceasing Pressure for Penetration: Gender, Pathology, and Emotion in George Kennan's Formation of the Cold War," *Journal of American History*, Vol. 83, No. 4 (March 1997), pp. 1309–1339.

119. Haber, Kennedy, and Krasner, "Brothers Under the Skin." See also Ernest R. May, "The Decline of Diplomatic History," in George Athan Billias and Gerald N. Grob, eds., *American History: Retrospect and Prospect* (New York: Free Press, 1971), pp. 399–430; and John A. Lynn, "The Embattled Future of Academic Military History," *Journal of Military*

Chapter 15

International History and International Politics: Why Are They Studied Differently?

Robert Jervis

When I was an undergraduate, an assignment in my Introduction to Religion class was to make a list of the characteristics that differentiated human beings from other animals. At first the task seemed easy, but difficulties became apparent as soon as my classmates and I got more deeply into the exercise. I find it as least as difficult to understand the differences between the approaches of political scientists and diplomatic historians. Part of the problem is that some of the differences within each group are as great as those between them; part is that it is hard to discriminate style from substance and the superficial from the fundamental.[1]

I also reject the more widely held view that a key difference is that political scientists are more policy-oriented, or more influenced by current policy concerns, in large part because they want to discern if not influence future events. Ingram argues that "being political scientists, [Thompson and Modelski] ask the dead to show us the way to the future."[3] I doubt whether this is the case for Thompson and George Modelski and I know it is not true for many of my colleagues, who care about understanding the past in its own terms and who doubt their ability to predict the future and lack the stomach for trying to influence it. It is simply not the case that most political scientists wish to whisper in the ear of policymakers, let alone take their place. This marks a change from forty or fifty years ago, when the connections between the study and the practice of politics were much closer. But as the discipline has sought to become more scientific, both the people recruited into it and the patterns of socialization have changed. This is not to say that political scientists are uninfluenced by current international politics or their own political outlooks. But this is at least as true for historians: as Jerald Combs has shown, interpretations of past events in U.S. foreign policy were strongly influenced by what was happening when scholars were writing.[4]

A Tale of Two Disciplines

The nature of our academic disciplines and the outlooks of the individuals who populate them mutually affect one another and co-evolve over time. I think an observer from Mars would be puzzled at how our disciplines carve up—and create—areas of knowledge. It would be easy if different disciplines studied different phenomena, even if the edges blurred. But it will not do to say that sociology studies societies, political science studies politics, and history studies the past. If we could start all over, we might do a better job of advancing knowledge by redrawing disciplinary lines, or abolishing them. But of course we cannot, and each of us reacts to the different mores and incentives of our discipline.

One noteworthy difference is that the study of international politics from the perspective of political science is almost exclusively an enterprise of the United States, or at least one of the English-speaking world.[5] There is much less international politics studied elsewhere, and most U.S. scholars are almost completely ignorant of what there is, in part because of language barriers. The community of historians, especially those who study the history of world politics, is much more international. I believe that it is this fact rather than the concern for the future that leads the analyses of Thompson, Modelski, and many other scholars to seem to Edward Ingram to be so marked by their North American origins and interests.

Even more important, I believe, were the different responses of the two disciplines to the major upheavals of the war in Vietnam and the campus protests. I suspect that opposition to the war was a major factor in the move of history to the left in its politics, to the marginalization of

4. Jerald Combs, *American Foreign Policy* (Berkeley: University of California Press, 1983).

5. Interesting discussions can be found in Ole Waever, "The Sociology of a Not So International Discipline: American and European Developments in International Relations," *International Organization*, Vol. 52, No. 4 (Autumn 1998), pp. 687–727; and Knud Erik Jorgensen, "Continental IR Theory: The Best Kept Secret," *European Journal of International Relations*, Vol. 6, No. 1 (March 2000), pp. 9–42.

international, political, and military history, and to the growth of the study of non-elites and an antipositivist methodology. ~~These trends themselves fit together, but do not form an inevitable~~ package. That is, it was certainly imaginable—and indeed was my expectation twenty-five years ago—that while the field would move to the left, it would retain a heavy focus on political history. One can also imagine the dominance of social and even cultural history coupled with the maintenance of history's deep roots in tangible evidence and a commitment to the search for objectivity. One could even see these subjects analyzed with mathematical techniques: much of sociology consists of studying non-elites with quantitative methods, and it too is generally critical of capitalism.

It is particularly noteworthy that history resisted the onslaughts of the cliometricians. The strengths and weaknesses of this approach—or rather this family of approaches—is not relevant here. In any event, I do not think we can account for the success or failure of alternative approaches by how well they explain previously agreed upon evidence. Twenty years ago, an economist and I served on a university committee called to judge the proposed appointment of a mathematical sociologist. When I admitted to my colleague not only my inability to follow much of the material, but my skepticism about whether it could answer the questions that were posed, he replied: "Bob, I can't tell you whether he is right or wrong, but I've watched this methodology in economics and it is like a steamroller: it will flatten everything in its path." Well, in history it did not. Part of the reason is that such methods were seen as flattening the material whose meaning history was supposed to convey. But its rejection may also have been linked to politics. Civil rights was the major domestic issue of the 1960s and 1970s and while it did not divide the academy as the war in Vietnam did, it was a deep moral commitment. The key book that developed and applied statistical and economic methods to history was *Time on the Cross*, which among other things argued that the experience and effects of slavery may not have been quite as dreadful as was previously believed.[7] I wonder if a different ~~conclusion would have gained more adherents for the approach.~~

The political and methodological trends in political science were quite different. Politically, the central tendency in the discipline remained relatively unchanged, with liberal Democrats remaining the strong majority. Although the popularity of economic models has been coupled with

7. Robert Fogel and Stanley Engerman, *Time on the Cross: The Economics of American*

some increase in libertarian politics and faith in free markets, movement has not been great. Marxism grew rapidly in the 1960s but did not thrive. Some argue that its conceptual framework was wrong and that the analyses it produced were badly flawed. Others reply that Marxists were discriminated against and excluded from the profession. (My centrist view is that both these explanations capture a measure of the truth.) At the same time, the discipline became more strongly engaged in analysis of quantitative data and the application of formal models borrowed from economics.[8] These approaches were slower to catch on in international politics than in some of the other subfields of the discipline but in recent years have become much more popular, with modeling now perhaps the most prestigious although far from the most common technique. What is relevant here is that these techniques grew in political science at just the time that history was moving in the opposite direction, thus widening the gap between the two fields. Indeed, the nonquantitative but evidence-based approaches represented by the chapters in this volume may have more in common with each other than they do with most of the work in their home disciplines.

Intellectual Differences

Significant intellectual differences between the fields remain, and it is to them that I now want to turn. Edward Ingram and Paul W. Schroeder have stressed that the conventional wisdom is wrong: one should not associate political science with theories and generalizations and history with description and treatments of each event as unique.[9] They are correct that one should not exaggerate, but, with John A. Lynn, I do not think the conventional view is entirely wrong. Both political scientists and diplomatic historians seek to explain events, and, in a sense, explanations must always involve general theories and particular cases, but they go about the task characteristically differently. The field of political science places priority on generalizations and explicit, parsimonious theorizing. These values are not shared by historians. To call a work of history nuanced and subtle is to pay it a compliment; for political scientists, these adjectives, if not supplemented by others, have a critical undertone. Too

8. People outside the discipline tend to see these two tendencies as the same. They are not, and their proponents argue bitterly with each other.

9. Ingram, "The Wonderland of the Political Scientist"; and Paul W. Schroeder, "History and International Relations: Not Use or Abuse, but Fit or Misfit," *International*

much subtlety, too many nuances, mean that the theoretical edge is likely to be lost.

PARSIMONY

Parsimony is extremely important for most political scientists. This does not have to mean monocausality or even simplicity, although sometimes it does. Rather, it refers to a favorable ratio between the explanatory factors deployed and the range of behavior explained. Parsimony is necessary for theory-building, discussed further below, and this is why political scientists are willing to forego a complete account of any individual event in order to seek explanations that apply to behavior that occurs in very different times and circumstances. It is particularly impressive to construct an explanation that helps unravel behavior that previously seemed very different. One attraction of collective goods theory, for example, is that it explains—or purports to explain—significant phenomena in alliance politics, tariffs, interest group behavior, the internal organization of legislatures, political leadership, internal group maintenance, and budgeting practices, to give just an incomplete list. This sort of "theoretical robustness" is highly prized both because it allows us to understand a lot with a little and because the range of phenomena being accounted for (a phrase that sets off alarm bells among historians) indicates that the posited cause is indeed potent.

There are two basic and quite different sources of the drive for parsimony. The first is convenience—Occam's Razor. Simpler is better because it makes handling explanations easier: things should not be complicated unnecessarily. Of course the world is more complicated than our theories; that is the reason we need them. The second justification for parsimony is more interesting because it constitutes a claim about the world, not about the pragmatic foundations of research strategy: parsimonious theories are more likely to be correct because the world is actually built around a manageably small number of important factors. This belief is particularly strong in physics.[10] Thus during the 1960s, many physicists were deeply disturbed by the discovery of one new "fundamental" particle after another. Rather than being overjoyed at these discoveries and claiming that they brought new understanding, most physicists felt that they indicated crippling flaws in the prevailing theories. Nature simply could not be built out of forty or fifty fundamental particles. The search for smaller and more basic particles out of which others were constituted was driven in large part by the belief that parsimony was a law of nature. Although political scientists rarely explicate a parallel claim, my sense is that many of them, but only few historians, do believe it.

This implies that some but not all subjects are appropriate for theorizing and that, relatedly, we should not confuse theorizing with monocausality. Thus Kenneth Waltz, perhaps the most influential contemporary theorist of international politics, stresses that the theory of structural realism that explains how the search for security in an anarchic world plays out differently under bipolarity than under multipolarity applies only to international politics, not to foreign policy.[11] While the general patterns of international politics can be understood in terms of parsimonious theory because the systemic pressures and interactions induce common results despite differences in state attributes, this approach cannot be applied to individual foreign policies because their sources are so many and can form so many combinations. Interestingly enough, some historians do see a single cause as dominating at least some states' foreign policies. Thus the New Left believes that the key to U.S. foreign relations is the economic interest of the ruling class, a conviction that produces both intriguing insights and bizarre interpretations. As in theory-driven political science, the story is told in a way that makes it fit the single posited motivating force.[12]

Even when parsimony is correctly applied, however, it does not come cheaply. At a minimum, it means that individual cases will not be fully explained and that some of them will have to be pushed and pulled to fit at all. Most historians are unwilling to pay this price. To them, the point is to understand the past and if this means that different events have to be explained quite differently, so be it. To develop incomplete or, even worse, distorted explanations for particular events in order to construct some grand overall view has the whole exercise backwards: we develop mental constructs to fit the world; we do not—or should not—present a picture of the world for the purpose of showing its fit with theories.

10. As Einstein said, "When I am evaluating a theory, I ask myself, if I were God, would I have made the universe in that way?"

11. Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979); Waltz, "Realist Thought and Neorealist Theory," in Robert Rothstein, ed., *The Evolution of Theory in International Relations* (Columbia: University of South Carolina Press, 1991), pp. 21–38. The issues are brought our clearly in the exchange between Colin Elman and Waltz in *Security Studies*, Vol. 6, No. 1 (Autumn 1996), pp. 7–61.

12. See, for example, Gabriel Kolko and Joyce Kolko, *The Limits of Power: The World and United States Foreign Policy* (New York: Random House, 1968); and Robert Buzzanco, "What Happened to the New Left? Toward a Radical Reading of American Foreign Relations," *Diplomatic History*, Vol. 23, No. 4 (Fall 1999), pp. 575–607.

For political scientists, individual cases are just that—cases of *something*, and the task of the scholar is to determine exactly what they are cases of. While the cases need to be understood, they are also in service of developing an explanation of a wider range of phenomena, and, reciprocally, can only be understood in light of what happened elsewhere. For historians, to talk of "a case" is to assume generality that is not likely to be present and to downplay if not deny what is central to their concerns, which is to understand why things happened as they did in that instance.

I think this is why historians are less troubled than political scientists by multiple sufficient causation—i.e., the possibility that the same outcome or behavior can be produced by different causes and through quite different pathways. For political science, this is a real problem because it constitutes a menace to one of its prime methodologies.

Historians are not likely to be upset because they do not expect this sort of parsimony. Indeed, I suspect that they would regard as a delight rather than an inconvenience a finding that radically different kinds of explanations are needed for apparently similar behavior in different periods of time or different geographic areas. They are ready to accept the notion that the causes of a war, peace, an alliance, deteriorating relations, and anything else of interest can be quite different from one case to the next. They also believe that they can understand the story by examining how all the factors fit together (see the chapters by Ingram and Gaddis) and by studying the progression of internal discussions and events by methods that are akin to process tracing as described by Andrew Bennett and Alexander George. Many political scientists are somewhat skeptical about these approaches and feel more comfortable when they can compare several cases in which the suspected independent variable in fact varies to determine whether the outcome changes as well, even though this cannot eliminate the possibility of multiple sufficient causation.

THEORY-BUILDING

Political scientists place a high priority on theory-building. This means developing, elaborating, and criticizing constructs at a fairly high level of abstraction, which is one reason for the interminable debates that strike outsiders—and many insiders—as akin to theology.

A prime purpose of this method of exposition (the "hypothetico-deductive method") is to take the theory seriously, to deduce what the world would look like if it were true. This inevitably produces some flattening of the historical record in order to gain analytical rigor and parsimony.

the theory. This is brought out by the differences between the way Ernest May and I studied how decision-makers learn from history.[16] Both of us argued that statesmen tend to see situations in terms of historical analogies, and we both supported our claim by showing that statesmen make explicit references to past situations when diagnosing current ones and adopt policies that they believe would have worked in previous cases. But a greater concern with testing these propositions led me to see whether I could eliminate competing explanations for this pattern, particularly the possibility that the analogies might have been post-hoc rationalizations for preferences arrived at on other grounds and the role of third factors in influencing both the lessons that were drawn about the past and the preferences for current policies (a problem of "spurious correlation"). Thus I tried to see whether the lessons of the past case were established *before* the arrival of the problem to which they were then applied, and I also looked to see whether people with different interests and outlooks drew the same lessons from historical events. For most historians these excursions are irrelevant if not confusing; for political scientists, they are crucial for establishing the causal role of historical learning. May, on the other hand, explored his cases in much more depth, thereby both telling a full story that has value aside from the analytical point and bolstering the conclusion by the detailed narrative. For historians this keeps the focus on the events, where it belongs; for political scientists it fails to meet the central requirement for establishing causation.

Their stress on asking what one would expect to happen if the theory is correct leads to what political scientists consider to be the crucial step of looking for "dogs that do not bark," to take the line from a Sherlock Holmes story. In "The Hound of the Baskervilles," Watson did not see why Holmes thought it was significant that the dog did not bark the evening its master was murdered; for Holmes, this was crucial because it showed that the killer gave the dog no reason for alarm and so must have been known to his victim. Thus political scientists keep an eye out for events that did not occur, but that a plausible theory indicates should have. Turning this around, they try to avoid "searching on the dependent variable," that is, looking only at cases in which a particular kind of outcome occurred (e.g., the outbreak of war, the formation of an alliance, successful deterrence). If one looks only at those cases, there is no way to tell whether the factors that the scholar believes to be causal were also present when the outcome was very different. If they were, they could not be the whole story because they would not discriminate cases in which the outcome was of one type from those which turned out quite differently.[17] The reply from historians is that this method, although not without its virtues, takes things out of context. It is possible—indeed likely—for one "factor" to have a very different outcome depending on several other factors. Thus showing that a particular historical lesson led to one kind of behavior in one instance but not in another would not show that it was unimportant in either, but just that the historical explanations deployed by statesmen, like those of historians, are inevitably multifaceted and involve the interaction of many considerations.

Because of their stress on theory, the distance between description and explanation in the discipline of political science is greater and more explicitly demarcated than in history. It is not that the latter is atheoretical, but that the explanation is more deeply embedded in the description. For a political scientist, a sharp division between the description and the explanation is a badge of honor; for a historian, it would be nonsensical. When political scientists read works of history, they are often annoyed that the discussion of what happened is entangled with an analysis of why it happened. By contrast, many international relations monographs consist of an introductory chapter that sets out several possible theories, a second chapter that develops the author's own proposed argument, a number of case studies, and a concluding chapter. In the empirical chapters the author frequently pauses to point out how the evidence fits or does not fit with various theories. A reader who is in a hurry, who trusts the author's judgment, or who is uninterested in history can skip these chapters. This would be unthinkable in a book of history. Indeed, many of the latter contain only brief introductions and conclusions. It would be pointless, indeed a violation of the historian's craft, to present an explanation for the history, let alone an analysis that might be applied to several cases, divorced from a discussion of the events to be explained.

Relatedly, political scientists often implicitly assume that people and states behave quite consistently. This does not mean that the behavior is constant, but that the changes are responses to alterations in incentives and, secondarily, in beliefs about how desired goals can be reached. As I noted earlier, theories gain credibility by being able to account for behavior across widely disparate realms.

17. This does not mean that these factors are unimportant—indeed, they might be

MORAL CONCERNS

Another difference in sensibility is less often remarked upon. I think historians are more influenced by moral concerns and are quicker to make moral judgments than are political scientists. While the latter put their theories up front and disguise or bury their conclusions about the appropriateness of the actors' behavior, historians do the reverse. Many political scientists want to be scientific and believe that evaluations of the actors, especially on moral dimensions, have no place in such an enterprise. The other side of this coin is that many historians, but fewer political scientists, feel a responsibility to educate the public. A leading diplomatic historian ends an exchange with a colleague with these words: "the test of one's scholarly credentials and ethical values is. . . . whether one is committed to history as a way of learning about how to preserve a more decent and humane world."[18] I think that few historians would have trouble accepting this standard; most political scientists would, arguing instead that the test is to understand the world, not to change it. ~~I think this explains much of Vietnam's impact on many historians.~~ Although in fact U.S. policy cannot be explained by Marxism and economic interests, its apparent pointlessness and futility not only opened space for non-orthodox accounts, but generated moral revulsion that animated a condemnation of U.S. motives and behavior throughout its history.

Political scientists often decline to discuss whether the actors behaved wisely, let alone morally. Partly because they are studying not actors but people, historians do not shy away from such judgments. Indeed, in some cases making them seems to be the point of the enterprise.

expected behavior at other times may indicate that the actor was not as consistent as the theory.

It remains unclear how consistent human behavior is. When serial murderers are finally caught, their neighbors are often stunned: "But he seemed like such a nice man; he always looked after my cats when I was away." Most of us instinctively expect quite a bit of consistency in the world. People who are humane in one sphere of life are expected to display this characteristic in others. Psychologists have found that in explaining individual behavior we usually place excessive weight on personal predispositions, underestimate the power of the situation the person is in and, as a result, incorrectly project behavior in one instance

18. Deborah Welch Larson, *Origins of Containment: A Psychological Explanation* (P

Chapter 16

International History: Why Historians Do It Differently than Political Scientists

Paul W. Schroeder

This title is chosen not to indicate a dispute but to acknowledge a debt. It conveys first what this essay represents: unabashed piggybacking on Robert Jervis's essay, including its title. I have concluded that an attempt on my part to comment on all the previous essays or to discuss the general topic of the book independently would serve little purpose, but that something worthwhile and germane may be contributed by commenting on Jervis's arguments from a historian's point of view. These are comments, I emphasize, not basic disagreements or attempts at refutation. I largely agree with what Jervis says; the demurrers generally amount to saying "Yes, but." My purpose is to develop some points he makes further, and to respond to questions he raises and possible answers he suggests with some of my own from a historian's point of view.

In regard to parsimony, again I agree with Jervis's identification of this as a prime source of difference between historians and political scientists, and with his remarks on the inclination of the latter to seek and prize parsimony and of historians to be suspicious of it. His observations on how many historians conceive of the search for parsimony and react to it strike me as accurate. My contention, however, is that the view commonly encountered among historians, that the quest for parsimony is per se inappropriate for history because its true nature and genius lies in its complexity and richness of detail, and that the historian therefore should eschew parsimonious explanations for richer, more complex, and ambiguous ones, is fundamentally wrong, a misunderstanding of history's task. In fact, history as actually practiced and presented by good professional historians is full of parsimonious explanation, if not theory, and historians should be as interested in achieving parsimony in their way and for their purposes as political scientists are in theirs.

typically address. Political scientists typically, though not always, attempt to explain classes of phenomena (wars, revolutions, international crises, foreign policy decision-making in general, etc.). A normal, necessary question in political science is, "Of what general phenomenon, development, pattern of behavior, etc., is this particular action an instance?" Historians more often (though again far from always) propose to explain particular instances of these same classes of phenomena, and are not satisfied to treat them simply or mainly as examples of some general law or pattern. Partly they occur, as Jervis again says, because the respective parsimonious explanations are arrived at by different routes—the political scientist's ideally by the hypothetico-deductive method, the historian's by a more inductive method of process tracing. But on a third aspect, what Jervis says is, I think, not wrong but needs further development: his remarks on how political scientists are uncomfortable with the phenomenon of inconsistency and apparent irrationality in behavior, and dislike leaving cases of such apparent inconsistency (e.g., in Soviet behavior in the early Cold War, one of the examples he mentions) unresolved and unexplained.

The reason for insisting on this point is not to contradict what Jervis says on this score, but to prevent it from inadvertently strengthening a common stereotype: that historians are mainly or solely interested in telling stories, recounting developments in all their rich and fascinating detail, and that political scientists are interested instead in explaining developments, categorizing them into classes, determining their causes. Historians are just as much interested in the latter pursuits and engage in them as much as political scientists—or at least they should be.

Neither political scientists nor historians value parsimony for its own sake, as a good desirable in and of itself. Both desire it, as Jervis notes of political scientists, for the sake of robustness, explanatory power, effectiveness in explaining as many phenomena as far as possible with as few explanatory elements as possible. Historians, I insist again, are as much engaged in the business of providing robust explanations as political scientists and should recognize parsimony as one element or feature of it. They have no business ignoring Occam's razor and multiplying either entities or causes beyond necessity. The trouble is that the historian's experience often forces him or her to the conclusion that particular parsimonious explanations and theories, including those of political scientists, are not robust, do not integrate and accommodate the pertinent evidence but ignore or distort it.

the round and picture it in its richness of detail and great interest; he did so because he found them all not sufficiently robust, leaving too much relevant evidence unexplained or explained in an unacceptable way.

Both this reason for the historian's frequent skepticism about social scientific theories in his field—not their parsimony but their lack of effective explanatory power—and the historian's greater willingness to accept and live with inconsistency in human affairs, including international politics, point also to a major difference between historians and political scientists: how they conceive of causes in human affairs and deal with them. Presumptuous though it is of me as a historian to discuss the mindsets and assumptions of political and social scientists, my impression is that without being rigid about it they try to conform as closely as possible to a Humean concept of cause: an antecedent condition or set of conditions that regularly and predictably produces a particular result. Historians, without thinking much about the question (less than they should, in fact), almost automatically or unconsciously use the term "cause" in the far richer, more varied, but entirely legitimate human understanding of the term, learned from inside through life itself and merely refined and developed by many scholarly disciplines, scientific and humanist. In this definition, "cause" is anything that effectively prompts or influences human beings to do certain things, and therefore varies almost indefinitely in kind and is unpredictable in precise effect and outcome. Instinct, learning, socialization, custom, habit, rational conviction, irrational or nonrational belief, emotion, impulse, example, need, persuasion, influence of others, etc., all can and do serve as "causes" for the historian, just as they are recognized by each of us as possible "causes" in our own lives and those of others. I would agree, even insist, that historians generally should be more aware of how they conceive of causality, more careful in how they use the term "cause" and its many synonyms, and more explicit in defining what they mean by it in particular instances. Doing this would at least help reduce the fog and smoke surrounding many historical controversies. I cannot agree, however, that history should forfeit its birthright by abandoning this wider, richer, and deeper concept of cause, with all its variety and the confusion it unquestionably introduces into every discussion, in exchange for the dubious pottage of social science rigor.

A further observation: Historians are less enamored of parsimony and more comfortable with inconsistency than political scientists in part because of a still more basic difference in their approaches: the tendency of political scientists to treat the common subject matter, international politics, as behavior, while historians insist on treating it as human conduct. This connects with observations by Jervis which I found intriguing,

but also in need of further development—those on morality. Here my contribution can be more substantial and less parasitic than elsewhere. It consists of offering reasons why historians not only are more inclined to make moral judgments than political scientists, but why they must and should do so—why moral judgments are not superfluous and harmful addenda to historical investigation or mere appendages of it, but embedded, inescapable ingredients in it.

First, a mild objection to one of Jervis's propositions. Quoting Melvyn Leffler to the effect that the historian's scholarship and ethical values are closely tied to a commitment to history as a means to help achieve a more decent and humane world, Jervis writes: "I think that few historians would have trouble accepting this standard; most political scientists would, arguing instead that the test is to understand the world, not to change it." On this score, many if not most historians, including me, would agree with the political scientists, on precisely the same grounds. Scholarly history has traditionally conceived its task and goal predominantly in a Rankean sense, as that of portraying the past *"wie es eigentlich gewesen,"* rather than the Marxian one of understanding the world in order to change it. There are, of course, serious scholars who consciously attempt to make history an instrument of social change. A good example is the program of doing history as critical historical science (*kritische Geschichtswissenschaft*) directed toward emancipation and social progress, practiced by Hans-Ulrich Wehler and others in Germany, especially at the University of Bielefeld. These efforts, however, represent controversial protest movements against the mainstream, and the historical criticism they regularly face on evidential grounds often forces them in practice to subordinate their emancipationist-reformist goals to the general canons of historical investigation.[3] Moreover, while many historians, perhaps most, would deny that Ranke's ideal of an objective historical account corresponding to past reality is attainable (thereby incidentally misinterpreting what Ranke meant and intended by his aphorism), the claim that objective historical truth cannot be attained, which has now become the

3. An excellent example of this is Dietrich Geyer's *Der russische Imperialismus* (Göttingen: Vandenhoeck and Ruprecht, 1977), translated into English as *Russian Imperialism: The Interaction of Domestic and Foreign Policy, 1860–1914* (New Haven, Conn.: Yale University Press, 1987). Geyer, a student of Hans-Ulrich Wehler, set out to test and substantiate Wehler's theory of imperialism as secondary integration. He did not end up rejecting or disproving it, but being a very good historian he did show that the interaction between domestic and foreign policy was more nuanced and complex than any theory of the primacy of domestic politics would allow—a finding which, while it made much sense historically, largely rendered the book useless for emancipationist-reformist purposes.

conventional wisdom, is not a denial that the proper goal of history is to improve our understanding of the historic past. The argument I will make here is that while a historian's scholarship and ethical values do involve a moral commitment and task, it is not the task of changing the world or providing the means for doing so. Instead, moral values and purposes are firmly embedded within the historian's task of understanding the past and are inseparable from it.

The first reason why doing history inescapably involves making moral judgments is the easiest to understand: the unavoidable moral content and dimension involved in the very language required if we wish, even in strictly Rankean terms, to describe historical actions, to state what really happened. The subject matter of history is, as traditionally stated, all that humankind has done and suffered (suffered in the sense of experienced, gone through). One cannot tell this story, or any substantial part of it, in value-free language devoid of reference to its moral dimension. One is forced, simply in order to give any sort of coherent narrative and analysis, to use adjectives such as good, bad, rational, irrational, harmful, beneficial, selfish, unselfish; verbs like kill, injure, massacre, slaughter, insult, wound, help, console, save, rescue, heal; nouns like courage, cowardice, honesty, lies, loyalty, betrayal, honor, dishonor, strength, weakness—etc. ad infinitum. Every attempt to construct an "objective" value-free language to tell the story of what human beings have done and suffered not only breaks down and denatures the narrative and analysis alike, but does so without really avoiding moral judgments, instead masking, blurring, and fudging them. To try, for example, to construct an objective, nonmoral standard for judging the German attacks on Poland in 1939 and on Russia in 1941 in terms of their motives, actions, and immediate and long-range consequences—to do so, say, in terms purely of their success or failure to advance Nazi goals, concluding that in the strictly immanent terms of Nazi aims the former was a success and the latter a failure, and to leave it there—would not merely be extremely morally insensitive, but unhistorical, a failure to come to grips with the real, main story of what happened, what human beings here did and suffered. Apply this same sort of procedure to the Holocaust and it becomes wholly inhuman, obscene. The language of history, used to tell and analyze it, is inescapably moral in one of its essential dimensions, as is that of everyday life. That language can be used well or badly, sensitively or insensitively, subtly and by implication or crudely and by imprecation, but it cannot be morally neutered without neutering history itself.

The task of doing history does not merely contain a moral aspect or dimension but is intrinsically a moral pursuit, belonging to one of humankind's supreme moral obligations, "Know thyself." I am of course not saying or hinting that history is the only discipline with this lofty purpose and function. All the liberal arts and the sciences, including the social sciences, share it. Nor am I hinting that social scientists in abstaining from making moral judgments in order to achieve their purposes become thereby less moral or truth-seeking or profound than historians, and offer less self-knowledge. But I would say that a difference between the two fields and approaches stems from the same source as their different meanings and uses of cause and causality, namely, the difference between treating human actions mainly as behavior or mainly as conduct. It is the latter emphasis which I think makes history especially valuable for self-knowledge, and it is a reluctance to abstract from it and the moral dimension it contains that makes me sure that I could never be a political scientist. However, as Jervis says, this is no reason why we cannot be friends and, in some cases, allies.

to imagine so distinguished a historian as J.H. Elliott spending great time and energy in researching the career of so unsympathetic a character as the seventeenth-century Spanish statesman the Count-Duke of Olivares had he not been convinced, as he tells us, that however unlikeable Olivares was and however disastrous his policies turned out to be, history had not dealt justly with him.[11] Not only are a great many individual works of history powerfully influenced by this feeling, but it also helps account for the emergence of new fields and emphases in history. The history of women, gender history, the history of everyday life, history from below, the history of various ignored or suppressed groups, and other such fields have arisen not just because their practitioners had career ambitions to pursue or ideological axes to grind, but because of a widespread conviction, often justified, that in these areas justice had not been done to the past.

It is harder to explain just what the second half of the axiom means: "not for the sake of the past, but for our own."

4. David S. Landes, *The Wealth and Poverty of Nations* (New York: W.W. Norton, 1998).

5. Richard Ned Lebow, *From Peace to War: The Nature of International Crisis* (Baltimore: Johns Hopkins University Press, 1981); and Paul W. Schroeder, "Failed Bargain Crises, Deterrence, and the International System," in Paul C. Stern et al., eds., *Perspectives on*

The Eternal Divide? History and International Relations

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Like most long-running interdisciplinary relationships, the liaison between International Relations (IR) and history has taken many turns. In some respects, history has always been a core feature of the international imagination. On both sides of the Atlantic, leading figures in the discipline such as E. H. Carr, Hans Morgenthau, Martin Wight and Stanley Hoffman employed history as a means of illuminating their research. Indeed, Wight (1966) made searching through international history the *sine qua non* of international theory, the best that could be hoped for in a discipline without a core problematique of its own. Although seemingly banished to the margins of the discipline by the emergence of behaviouralism and the association of "real theory" with deductive, nomological methods, history never really went away as an important feature of IR. Rather, history became part of a broader tug of war between "classical" approaches, which retained history as their central locomotive and IR's neo-positivist laboriticians, who saw history as providing the main ammunition for their experiments. As this essay shows, history has been employed, albeit unevenly, throughout the discipline. And given this, the rise – or reconvening – of historically-oriented research programmes such as constructivism, neo-classical realism and the English School should be seen less as a novel breakthrough than as a return to business as usual.

However, there is a tension that remains unresolved in the relationship between history and IR, one that is long-standing and that reappears with regularity, even in those texts that explicitly straddle the IR-history frontier. The issue is revealed in a passage from one of the best known of these texts (Elman and Elman, 2001):

> Political scientists are not historians, nor should they be. There are real and enduring epistemological and methodological differences that divide the two groups, and there is great value in recognising, maintaining and honouring these distinctions.

These passages point the way towards a clear division of labour between theory-building political scientists and chronicling historians, a first-order demarcation on which other contributors to *Bridges and Boundaries* overlay a number of second-order distinctions: methods (secondary sources vs. primary sources); aims (identification of regularities and continuities vs. the highlighting of contingency and change); orientation (nomothetic vs. idiographic); sensibility (parsimony vs. complexity); notions of causation (transhistorical vs. context specific); levels of analysis (structure vs. agency) and so on. As a result, essential differences are formed in which one discipline (IR/political science) acts as binary opposite for and, more often than not, coloniser of the other.

This essay questions the grounds for the construction of this "eternal divide" between history and IR. Nevertheless, despite the surface-level closeness of the relationship between IR and history, much IR scholarship is predicated on a view of history caught between two equally unsatisfactory stools. On the one hand, history becomes a predetermined site for the empirical verification of abstract claims. In this sense, history serves as "scripture", as the application of timeless "lessons" and inviolate rules removed from their context and applied to ill-fitting situations: the "lessons of appeasement" become a shorthand for the necessity of confronting dictatorial regimes across time and place, or the U.S. retreat from Vietnam is invoked to halt talk of withdrawal in Iraq. Such a move runs counter to the avowed aims of the historical return in IR: due regard for particularity, context and complexity.

A second, equally prominent tendency in IR scholarship is to see history as the "if only" realm of uncertainty (Versailles less punitive, Bin Laden assassinated before 9/11, Pearl Harbour never taken place), a "butterfly" of contingent hiccups upon which IR theorists provide ill-fitting maps – maps that apparently reveal the distortions of their ideological prisms rather than the shape of history itself. Curiously, despite a sense in which this turn seeks to foster a kind of "pure history", it is also ahistorical in that it fetishises the particular and the exceptional, failing to see how historical events are part of broader processes, sequences and plots that provide a shape – however difficult to discern – within historical development itself. The result of the "if only" school of history is a reduction of the past to a "pick and mix" sweet shop that is raided in order to satisfy the tastes and tropes of the researcher. Significantly, it is also a vision of history contrary to how the majority of historians themselves conduct their research and characterise their discipline.

The existence of these two forms of ahistoricism – history as scripture and as butterfly – are forged by the working practices of IR scholarship itself. Most mainstream approaches adopt a form of "history as scripture", using history in order to code findings, mine data or as a source of *post factum* explanations (Smith, 1999; Isacoff, 2002). Most post-positivist approaches – particularly post-structuralism – assume a form of the latter, using history as a means of disrupting prevalent power-knowledge nexuses (Ashley, 1989; Walker, 1988; Vaughan- Williams, 2005). Few IR scholars spend sufficient time asking what it is we mean when we talk about history. The central aim of this essay is to delve beneath the surface of these debates in order to demonstrate how a *historical* mode of explanation based on context and narrative, allied to a *social-scientific* mode of enquiry centred on eventfulness and ideal-typification, can reveal the necessary co-implication of each enterprise.

Scripture and Butterfly: History and International Relations

Although there have been many encounters between historians and IR theorists, history has often served as a passive backdrop for theorist's experiments. Indeed, as one of IR's most celebrated quantifiers puts it, "for the social scientist, history is primarily a laboratory by which to test both their claims about how variables are associated with each other and their propositions about causation". In general, history is often assumed to be removed from mainstream IR, best captured by Waltzian neo-realism and Keohane-inspired neoliberal institutionalism. As is well known, both of these approaches work within an assumption of a continuous structural context to international relations (anarchy) which, in turn, generates a number of characteristics of the system – self-help system, the need for states to prioritize survival, a recurring security dilemma, the balance of power. Because anarchy stands as a constant structural condition, so the international sphere appears as a continuous, almost static, holding pen for "actually existing" international relations. In turn, this means that IR scholarship is – or should be – primarily concerned with mapping the ceaseless struggle for survival (as in neorealism) or the conditions for cooperation (as in neoliberalism) that take place within a timeless and spaceless anarchical system. And much of the criticism of "neo-neo" theories, particularly neorealism, is that it lacks a transformative logic and, as a consequence, is unable to explain processes of change – especially systems change – over time.

If the ahistoricism of neo-neo approaches is a commonly held assumption, equally widespread is the idea that historical sensitivity is something that has become a core feature of IR scholarship only relatively recently. In fact, a concern with temporality has long been a feature of international studies. Indeed, it is possible to discern a classical tradition in IR theory, perhaps most obvious in figures such as Niebuhr, Carr and Morgenthau, which intimately associated the craft of international theory with deep immersion in history. Of course, these scholars had quite different views of history. While Niebuhr saw history as contingent and diverse, Morgenthau (like George Kennan) argued that history was singular but repetitive, thereby delivering an "ordered register" of practical knowledge for the prudential policy maker to learn from. Even within the classical tradition, therefore, can be found contrasting views of history: the tragic perennialism of Niebuhr alongside the contingent discontinuity of Morgenthau, Carr and Kennan. But despite their surface-level differences, these scholars shared a common interest in historical research, whether this was conducted in order to reveal the ceaseless precariousness of international politics, or as a means of generating practical advice for the prince.

In this sense, therefore, the neo-neo hold on IR could be seen as an unwelcome interlude in a much longer, more fruitful, association between IR and history. However, even during the high-water mark of the neo-neo grip on IR theory, few advocates of either sensibility denied the importance of history as a means of testing their approach. Figures as varied as Robert Gilpin (1981), John Mearsheimer (2003) and Arthur Eckstein (2006) have carried out major pieces of historical research aiming to validate, fill-in or challenge the Waltzian frame. In a similar vein, Robert Keohane (1984) and others have applied historical analysis to a neoliberal institutionalist research program in order to draw out its explanatory potential. In this way, the return of classical liberalism, the rise of neoclassical realism and constructivism, and the reconvening of the English School mark less the emergence of a historical turn in IR, but more a deepening of trends already present in the discipline. History has always served as a tool for testing the validity of theoretical positions, and mainstream scholarship is perfectly content to use history as a barometer or litmus test for adjudicating between rival schemas.

However, although mainstream approaches – particularly neorealism and neoliberal institutionalism – *do* employ historical research, it is not clear that the latter serves much purpose in their accounts. Although "history" as a point of data collection may be present, *historicism* – an understanding of the contingent, disruptive, constitutive impact of local events, particularities and discontinuities – is absent. As such, these approaches illustrate the ahistoricism of seeing "history as scripture" – of abstracting concepts such as "anarchy", "the balance of power" or "self-help" as timeless analytical entities. In this way, neo-neo approaches are home to what we might call a "continuist mystique" in which history is not considered on its own terms but ransacked in order to explain the present. Thus, the contest between Athens and Sparta is transplanted to the Cold War in order to elucidate the stand-off between the United States and the Soviet Union; all wars, whether they be guerrilla insurgencies or total conflicts, are explained by international anarchy; and all political units – city-states, nomadic tribes, empires, nation-states and transnational alliances – are functionally undifferentiated. Neo-neo approaches, therefore, suffer problems associated with any theory that begins from a general abstraction (such as the timeless logic of anarchy). If the researcher starts with a picture of the whole that is already filled-in, s/he will see conforming details rather than possible alternatives. This form of research is a type of motivated bias – a cognitive disability to recognise historical anomalies and discrepancies, which, in turn, generates an apparently unbridgeable gap between theoretical assertions and historical analysis.

There is an alternative, of course, to such approaches – to accept the contingency, accident and indeterminacy that are constant companions to world history. Nick Vaughan-Williams (2005), for example, favours an historical epistemology that seeks not to "resolve history" but to see it as an "open problem", a realm of uncertainty that remains constantly "out of reach". Critiquing the "interpretative closure" of mainstream IR, Vaughan-Williams prefers to see history as a tool of destabilisation that can reveal the distortions of fundamental presumptions. In short, for Vaughan-Williams, history is a "butterfly" of contingent hiccups without shape, form or reason, a site of dissidence that can unmask and disrupt hegemonic readings. Vaughan-Williams is part of a broader field of post-structuralist scholars in IR, including Rob Walker (1988), David Campbell (1998) and Richard Ashley (1989), who see history as far removed from mainstream accounts of *post-factum* closure. Rather, for these scholars, history is inherently contestable, unstable and disruptive. As such, researchers need to shift from an understanding of history as delivering "essential truths", "timeless categories" and "unchanging reality" to one which sees history as contested and contingent.

However, such an approach also has its difficulties. At the very least, post-structuralist understandings of history run the risk of "overdetermination" – the provision of a laundry list of causes that includes all sorts of weak or insignificant factors. Perhaps more importantly, by stressing contingency, accident and particularity, there is a possibility of omitting bigger, more important commonalities. Where the neo-neo conception of history irons out discontinuities by creating isomorphic transhistorical categories, post-structuralist approaches obscure the sense in which history is a *social* process, one in which historical events, dramas and processes are part of broader chain of historical development. Just as mainstream approaches make history a singular realm of certainty and regularity, so post-structuralist approaches assume history to be a singular realm of difference and instability. As such, where the former fetishise general abstractions, the latter fetishise the particular. Neither provides much help in terms of building durable links between history and IR theory. And neither provides much help in terms of generating theoretically appealing *and* empirically rich accounts of events, processes and dynamics in world politics.

In many ways, "middle-way" approaches to IR theory claim the closest association with historical research. Indeed, it could be argued that the English School has the most intimate association with history of any of the major approaches to international relations, several members of whom were practicing historians (including Martin Wight and Herbert Butterfield), while many contemporary advocates of the English School (such as Barry Buzan, Richard Little and Hidemi Suganami) continue to play an active role in bridging the theory-history divide. However, even the English School has a tendency to replicate core features of this divide. On the one hand, a number of English School theorists see history as "useful knowledge", serving as a means of illuminating concrete puzzles in world politics. But they remain suspicious to the point of hostility at attempts to capture history within broader explanatory frameworks. This understanding of history as a necessarily limited realm stands some distance away from the attempts by figures such as Buzan and Little to test the utility of the "international system" as a transhistorical theoretical concept (2001). As such, although there is certainly an underlying historical sensibility to English School theory, there is no consistent philosophy of history or historical method that can be clearly associated with the approach.

It is equally difficult to establish a distinctly constructivist mode of historical enquiry. While there are many strains of constructivist IR, all variants reject a neo-neo instrumentalist rational actor model in which actors' interests are pre-determined and universal across time and space. As such, constructivism is propelled towards accounts of time and place specificity, context and change that render the approach *necessarily* historical in orientation. Chris Reus-Smit (2008) argues that constructivists tend to adopt an interpretivist approach to historical research, giving special attention to processes of social change and the role historians themselves play in constructing history. However, there is little distinctiveness about the theory-history relationship in constructivist studies other than, as with the English School, a broad commitment to historical research as a means of explicating theoretical work. As such, it is nearly impossible to pick out a discrete philosophy of history associated with constructivist IR.

Beyond the Eternal Divide: Context, Eventfulness, Narrative, Ideal-Typification

Of course, this broad-brush survey of contemporary IR theory is necessarily crude. Nor should the metaphor of "scripture" and "butterfly" be seen as an attempt to provide a complete categorisation. Rather, the goals here are more limited – to simplify the relationship between history and IR as caught between “scripture” and “butterfly” to tease out a number of issues that lie submerged beneath the surface of existing debates about disciplinary partitions based on apparently eternal distinctions about appropriate levels of abstraction, degrees of causal determinacy and “proper” methods. This section of the essay provides a more measured assessment of the relationship between social science and history based on four mechanisms – context, eventfulness, narrative and ideal-typification – drawn from history and social science. By highlighting these commonly used tools, it becomes clear that beneath the surface neither social science nor history requires a particular level of abstraction, mode of explanation, methodology or epistemology. Rather, apparently essential differences between the two enterprises – nomothetic vs. idiographic, parsimony vs. complexity, general vs. particular – have been constructed from the requirements of disciplinary gatekeeping rather than any hard-and-fast intellectual distinctiveness. Problematising the history/theory binary means acknowledging that history is a social science just as social science, including IR, is necessarily historical.

Context

At first glance, “context” does not appear to be the most obvious place by which to establish shared focal points between history and social science. Indeed, the modern study of history is often associated with figures such as Leopold von Ranke, who sought to establish history not as a narrative of specific situations but as a truth-revealing science. Ranke, for example, was broadly indifferent to the context in which historical events took place and in things that occurred outside the realm of high politics. Rather, his concern was to generate a legion of well-trained archivists capable of trawling documents-of-state in order to reveal the true motivations of the great and the good. The past two centuries has seen a reduction in Ranke’s influence on historical research. Perhaps most crucial here is the emergence of “history from below” – the writing of history from the perspective of those who are at once both its principal agents and, just as commonly, its main victims. Advocates of history-from-below argue that history cannot be told merely from a Rankerean focus on truth-as-high-politics-revealed-by-official-sources. A commitment to history from below also entails a broadening of historical method towards oral testimony and biography, and away from “documentary fetishism”. Equally importantly, this shift in the status of primary materials has gone hand-in-hand with a more widespread interrogation of the status of historical facts themselves. R. G. Collingwood

(1994) argued that history revealed little more than the mind of the historian, promoting a form of "historical imagination" in which history became a history of ideas and historical research the re-enactment of past thought.

Much history during the Cold War seemed more in tune with E. H. Carr's (1967) desire to see history as a "selective system", an inherently social process best considered as a dialogue between present and past societies. For Carr, the first step in studying history was to study both the historian and the broader context (the social, political and economic environment) within which they carried out their research and within which historical facts were accumulated. For Carr, the fact that historical relics could not speak for themselves but were embedded within broader social matrices meant that there could be no absolute truth about the past in the way promoted by Ranke. Rather, for Carr, historical explanations were inherently approximate. This did not mean the end of adequate explanation; the conversation between past and present contained within Carr's vision of historical method tasked the historian with differentiating between significant and accidental causes, providing intelligible meaning in a world of incessant change, and remaining open to new interpretations of a subject.

Carr's understanding of the construction of historical knowledge as a fundamentally *social* process rooted in interrogation of the multiple *contexts* within which historical knowledge is produced acts as the first step towards shared conceptualisations of history and social science. Carr saw historical research as concerned with adjudication between rival interpretations based on an open conversation – and contestation – between facts, sources and scholarship. As such, he favoured a historical epistemology in which history was conducted not merely by the uncovering of new facts but by immersion in the "knowledge cultures", modes of thinking and reasoning practices that emerged in specific contexts and that helped to translate historical materials into social facts. Carr's enquiry points to an understanding of history as "recollecting the past", the study of events that are always part of broader structures of meaning. This signifies a move towards what Margaret Somers (1996) calls the "context of discovery" – a focus on how historiography itself enables findings to emerge.

History is always viewed from the vantage point of the present – we are, as Friedrich Kratochwil (2006) acknowledges, "historical beings" in that we are situated in broader milieus within which we conduct a dialogue between present and other times. Quentin Skinner and other members of the Cambridge School of historians attacked the Rankerean notion of a historical text speaking for itself and of history being concerned with "fundamental concepts", "timeless elements" and the like, arguing that we can only know history from our own times. As such, for Skinner, reading the past requires an avoidance of the twin dangers of parochialism and anachronism that transplant concepts (such as balance of power) and viewpoints (such as political realism) to contexts where they are inappropriate. In a similar vein, Skinner critiques "the mythology of doctrines": the tendency to find advocates of a position in earlier times and earlier places. Hence, Thucydides, Machiavelli, Hobbes, *et al* are taken to be part of a realist canon not because they would have recognised modern usage of the term – or necessarily have related to contemporary concepts and ideas – but because we find affinities between their work and our times. In short, we read us into them. For Skinner, the predilection for "text without context" generates an overly neat packaging of history, generating a mythology of coherence that makes ideas *appear* translatable beyond their particular utterance. As such, Skinner is dismissive of the fetish towards seeing "history as scripture" – if scholars have a particular view to peddle, they will find evidence to support it. For Skinner, this is not history but mythology. And it is how much IR theory approaches the subject of history.

Skinner's way out of the "history as scripture" quagmire comes via asking what an author is trying to *do* when s/he write a particular text, in other words to seek out their intentions. Importantly, Skinner is concerned more with intellectual and linguistic contexts than he is with establishing the political, economic and social constraints on historical knowledge. In other words, historical research should seek out the uses and practices in order to understand the "internal tradition" within which they are articulated. In "interrogating" historical texts and writers, we need to "see things their way". Such a viewpoint places Skinner squarely within the tradition of historical research outlined above – the attempt to move away from the testing of abstract lessons and anachronistic fallacies towards acceptance of the contextual limits of both history and our understanding of it. Indeed, such a move allows social scientific research to move away from abstractions towards explanation of specific historical processes, building from identification of processes that take place *within* time-space-linguistic contexts towards establishing the extent to which comparable processes occur in *alternative* milieus. As such, it is rooted in mid-range theorising, occupying the messy eclectic centre of social (and historical) theory by combining analytical rigour with conceptual sophistication and empirical reach.

Eventfulness

Historians, it is often claimed, act as process tracers *par excellence*, establishing the ways in which events become linked, threaded and sequenced in broader configurations. Although, as William Sewell (2005) notes, historians often begin with the facts of contingency, complexity and causal heterogeneity, few make the case against there being significance to the sequence within which events take place, or that the context within which they occur is insignificant. In other words, accepting the contingency of events does not preclude these being placed in broader analytical narratives. In this sense, Sewell argues, we need two forms of research: synchronic study of the form, content and structure of social relations; and diachronic study of how these social relations emerge, are reproduced and transformed. Even events that appear to be new are themselves part of broader dynamics, as such, events themselves can be seen as theorisable categories, part of broader sequences that reproduce and transform existing patterns of social relations. Events have cascading, sequential effects in that they both break and reproduce existing formations. Sewell uses the example of the fall of the Bastille to illustrate his point. The importance of the storming of the Bastille in 1789 was that it was imbued with significance "beyond itself". In other words, the event contained a recognition within broader political and cultural fields that broke existing configurations and reconstructed categories of meaning, amongst them notions of "people" and "revolution". And it is not difficult to find contemporaneous events that contain comparable effect: the fall of the Berlin Wall, 9/11, etc. In short, an "eventful" approach allows researchers to see how *historical* events enable *social* formations to emerge, reproduce, transform and, potentially, break down.

If the first step, therefore, in understanding the interconnection of history and social science is to recognise that temporality is rooted in contexts that are examinable via social scientific enquiry, the second step is to see events as theorisable in that certain "happenings" have outcomes that can be studied via the ways in which social formations emerge, become institutionalised and change. Historical events are interpreted within relatively contained plot structures drawn from the intersection of events and the milieus within which they take place. In this sense, historical accounts contain a sense of "followability": a narrative intelligibility in which contingency is conjoined with an account of adequate causation.

Narrative

As Hidemi Suganami (1999; 2008) notes, historical accounts tend to contain three dimensions: chance (contingency), agency and causation. Although social scientists often focus on causation, and historians on agency and chance, this does not make the social world beyond the comprehension of either set of researchers. In fact, both historians and social scientists are concerned with establishing "causal narratives", structured stories that explain events and make them intelligible to others. By focusing on particular moments, events and "critical junctures", it is possible to attain an explanation of the movement of historical processes alongside broader analytical attempts. This is the method employed by a number of historical sociologists studying periods of rapid change such as organic crises, revolutions and wars. Regardless of sometimes stark disagreements over epistemology, subject matter and sensibility, most historians see one of their core tasks as "emplotment" – the process by which events are given a sense of order and hierarchy. To put this simply, historians may tell stories, but most consider their stories to be superior to others. And although they may be comfortable with the world of alternative futures and unintended consequences, nevertheless most historians generate a logic to their explanations in which ordering events into causal narratives plays a central role.

This focus on the ordering of events into intelligible stories provides the kernels of a third way in which social science and history are intertwined: narrative. The role of narrative in historical research is well chronicled. Some thirty years ago, Lawrence Stone (1979) discerned a "revival of narrative", a trend most evident in the work of metahistorians such as Hayden White, who perceived emplotment as the means by which historians mediated between their fields of enquiry, the records they encountered, existing historical accounts and their audiences. White's most radical move was to claim that research of this kind was a "poetic act" in which historians discerned plot structures largely on aesthetic and normative grounds. As such, historical work developed its "explanatory effect" via a three-dimensional analysis: aesthetic perception (emplotment), cognitive operation (argument) and ideological prescription (implication). In this way, White argued, the task of the historian was not to produce a final, exhaustive reading of the historical record – indeed, he recognised that there would always be "surplus meanings" to historical texts, rather, historical narratives performed roles of knowledge construction, revision and destruction (of existing accounts) which served to render the social world both ordered and meaningful.

More important is the sense in which narrative as a *social* process can illuminate links between history and social science. In White's account is a sense in which emplotment contains a *social* logic in which history is less removed from the social sciences than part of a broader panorama of story-telling disciplines. Perhaps, therefore, it is worth acknowledging that just as "theory is always for someone and for some purpose" (Cox, 1981), so history too is always for someone and for some purpose. History is not a flat realm of incontrovertible facts for theorists to mine. Rather, via an understanding of the importance of narrative, whether understood as an analytical tool or as an emplotment device, in *all* social scientific stories, it is clear that history has far more in common with social science than is often considered to be the case.

Ideal-Typification

A focus on events, context and narrative in the historical formation of social facts constitutes important points of interaction between social science and history. A fourth point is through the construction of what Max Weber (1949) evocatively called "thought pictures" (*Gedankenbilder*); ideal-types that serve as heuristic devices for the examination of empirical reality. For Weber, ideal-types served as expository tools by which to clarify history, organising certain aspects of social life into internally consistent, logical constructs. As such, ideal-types were tasked with the "analytical ordering of empirical social reality", a key aspect of the "empirical science of concrete reality" (*Wirklichkeitswissenschaft*) that Weber developed. For Weber, ideal-types served as "simplifications for the purposes of increasing comprehension" (Jackson, 2010). Researchers adopting this method "trace and map how particular configurations of ideal-typified factors come together to generate historically specific outcomes in particular cases". Importantly, ideal-typifications are not meant to represent "actual history" but to act as simplified maps of historical reality with the goal of specifying causal configurations. In short, ideal-types are a method for exploring the casual relationships contained in historically specific configurations and, potentially, tools of comparison beyond these specific instantiations. In this way, ideal-typification isolates key features of historical events and processes, highlights their most important features and, in turn, examines their salience in alternative arenas. The result, Weber argued, was a means of tacking effectively between empirical material, conceptual abstractions and causal explanations, or as Michael Mann (1986) puts it, "carrying out a constant conversation between the evidence and one's theory".

There are a number of existing enterprises that illustrate the potency of this type of research. For example, there is the emergence of what Andrew Bennett and Alan George (2005) call "typological theory" – the development of contingent generalizations that begin with events and that understand that some occurrences take on path dependencies that can be effectively traced. The task for such research is to identify these events and the complex causal mechanisms that flow from them. This form of research – what we might call "nomothetic history"– begins with the identification of specific causal mechanisms *in* time and place allied to how such typifications operate *across* time and place. Such research fits squarely within the tradition of "classical social analysis" defined by C. Wright Mills (1959) as being mutually occupied by concerns of structure, history and biography. For Mills, interest in *structure* arises from the fact that human behavior is always shaped by particular patterns of social relationships. *History* adds the sense that these social structures are always specific to given times and places, that they vary enormously from one period or setting to another, and that they are themselves subject to change over time. Finally, *biography* connects these larger-scale phenomena of structure and change to the experiences of individuals – revealing how their lives are shaped by broader social and historical processes and how their agency, in turn, effects these processes.

By triangulating these three registers (structure, history, biography), Mills concluded, "classical social analysis" produced an idiom of understanding so rich and compelling that it provided the "common denominator" for the modern social sciences. Examples of this form of "concretely embedded research" are many, ranging from Michael Mann's (1986) sweeping account of world historical development to IR studies of the global genesis of the modern states-system (Hobson, 2004). These accounts are sensitive to historical particularity and complexity, while retaining a social scientific commitment to "systematic inquiry designed to produce factual knowledge" (Jackson, 2010). Importantly, this research is rooted in concern not just for the logical ordering of historical events, but with how intuition, imagination and judgement play major roles in deriving analytical narratives. In short, ideal-typification represents a *Wirklichkeitswissenschaft* that sees history and social science as inexorably conjoined.

Beyond the Eternal Divide

Ideal-typification enables a form of research far removed from the siren songs of history as either "scripture" or "butterfly". As such, alongside the other tools outlined in this essay, it serves as a vibrant site of connection between history and social science. To date, both sides of the imaginary, but powerfully constructed, "eternal divide" have been reticent to engage fully with the other. As a result, a number of unhelpful – at times even false – dichotomies have been established, which this essay has sought to critique. Although history and social science are necessarily intertwined with each other, this relationship is often occluded by a focus on secondary differences of method, sensibility and aesthetics. If we are to make claims that avoid such a reliance on false binaries, we require tools of mediation between abstractions and empirics, and approaches that consciously incorporate both the sociological imagination and the historian's craft. This essay has taken a first cut at this task via a focus on context, eventfulness, narrative and ideal-typification.

But whichever tools are employed, it is clear that much of the time, social scientists and historians converge in terms of their modes of enquiry and tools of explanation, albeit while simultaneously appearing to hide this synergy. The argument here, therefore, is that history and social science should not be considered as autonomous enterprises separated by virtue of distinct orientations, approaches and subject matters, but as a common enterprise. By ordering and sequencing events into intelligible narratives, recognising how people act within certain contexts, history does not abhor social science – rather, it requires it. As such, the choice is not one between a historical enterprise that can do with or without theory, but acceptance of the fact that history *is* a social science. It is an approach that emplots, narrates and analyses causal stories. In this way, history takes its place as an indispensable part of the panoply of social sciences just as social science appears as one amongst many story-telling enterprises. Both are necessarily implicated in each other, something made clear by a focus on context, eventfulness, narrative and ideal-typification.

For the IR researcher, there are at least two steps that follow from this focus on shared ground: first, awareness of the way in which diverse theoretical schools interpret, assess and adjudicate a particular historical subject matter; and second, maintaining an eye out for variance, conflict and heterogeneous opinion at least as much as convergence, clusters and patterns of received wisdom. In short, researchers should look to history in order to be wrong, to look for interpretations, surprises and contradictions that do not fit with prevailing theoretical explanations. Perhaps, then, what is required is a degree of humility about what we can know, an understanding that theoretical explanations are always partial, provisional and contained within tightly bound historical domains. Big events don't require big causes – rather history is best seen as a conjuncture of chance, agency and confluence that comes together in particular sequences that, in turn, can be usefully and powerfully emplotted.

The results of such an exercise would not, therefore, seek either total explanation nor the maintenance of a Maginot Line between history and IR, but the generation of "analytical narratives" that accept that temporality is social, events are theorisable, and that narrativity is an indispensable part of causal stories, best captured by varieties of ideal-typical research. History does not belong to a single theoretical approach in IR: history comes in plural modes rather than in singular form. Indeed, history is, in many ways, the lowest common denominator of theoretical approaches within the discipline. As such, it is particularly important to establish precisely what we mean by "history in IR" – the scholar's choice of historical sensibility is, in turn, constitutive of the way in which s/he theorises the international realm. Accordingly, if we are all historians, at least on some level, we are differentiated not simply by our choice of theory but also by our selection of a particular historical mode of explanation. And in developing this selection, it should become clear that both social science and history form part of a single intellectual journey, one in which both are permanently in view and in which neither serves as the coloniser of the other.

Chapter 3

Social Science and History: Ranchers versus Farmers?

Richard Ned Lebow

Ranchers and farmers were often enemies in the Old West. Ranchers wanted open land to graze their cattle herds and bring them to market. Farmers enclosed land to grow and protect their crops. As farms became more numerous, it became increasingly difficult for ranchers to drive their herds from the grass lands of Texas to the markets and slaughterhouses of the Middle West. Desperate ranchers sometimes resorted to violence in an ultimately futile attempt to preserve the open spaces of the West.

Like ranchers and farmers, historians and social scientists lay claim to the same terrain with very different purposes in mind. Historians study the past as a valuable exercise in its own right. They also use the past to illuminate the present by discovering the origins of values, ideas, and institutions and tracing their subsequent development. Social scientists regard the past as data that might help them develop and test theories of human behavior. Since the 1950s social scientists have had the upper hand, measured in salaries and access to jobs and research funds. Fortunately, the conflict between the two professions has never gone beyond rhetorical violence, but it has at times been acute and, in my opinion, counterproductive to both. This volume represents one of a growing number of opportunities to bring together interested representatives of the two intellectual communities to discuss matters of common interest.

I offer as my contribution a critical evaluation of the ongoing historical reevaluation of the Cold War. I identify some methodological pitfalls and urge adoption of some conceptual tools not commonly employed by Cold War historians that could facilitate this enterprise. I use the Cold War as an accessible platform for my arguments, which are aimed at a wider audience of historians. Although I use some of the language and concepts of neopositivism, I am not urging historians to become social scientists. My paper is not intended—and I hope, is not read—as an exercise in disciplinary imperialism. Elsewhere, I have pleaded with international relations theorists to study the methods and findings of Cold War historians.[2] Learning in our scholarly neighborhood should be a two-way street.

Good research starts by identifying important questions or puzzles. I argue at the outset that Cold War history and international relations theory have been surprisingly unreflective about where their questions come from, and that this has led to some dead ends in both fields. The search for answers has been equally problematic. In Cold War history, answers most frequently take the form of single case narratives that attribute key decisions or events to multiple causes. I argue that failure to rank order these causes and explore the relationship between or among them can make such explanations difficult to refute and easy to confirm tautologically. Answers to questions require evidence, and I contend that Cold War history has been too narrowly based on the written record. Documentary evidence is obviously critical, but needs to be augmented—and often corrected—by oral history and interviews.

A common view holds that historians practice narrative-based explanation, and that this is something very different from theory-based explanation.[3] This is a false distinction. Narratives are compatible with and generally rooted in theory, although that theory may not be articulated. Historian Edward Ingram observes: "The historian's description is a form of analysis (it explains); likewise, narrative (which has nothing to do with chronology) is applied theory, an analytical test of a proposition; each presupposes the other and, without the other, neither can be carried out."[2]

For international relations scholars in the 1980s, the preeminent question in the security subfield was the "long peace" between the superpowers. Specialists considered it remarkable that the superpowers had avoided war, unlike rival hegemons of the past. They were also impressed by the seeming durability of superpower spheres of influence. According to John Lewis Gaddis, "the very fact that the interim arrangements of

3. Edward Ingram, "The Wonderland of the Political Scientist," *International Security*, Vol. 22, No. 1 (Summer 1997), p. 53; see also Colin Elman and Miriam Fendius Elman, "Diplomatic History and International Relations Theory: Respecting Difference and Crossing Boundaries," *International Security*, Vol. 22, No. 1 (Summer 1997), pp. 5–21.

1945 have remained largely intact for four decades would have astonished—and quite possibly appalled—the statesmen who cobbled them together in the hectic months that followed the surrender of Germany and Japan."[5] The burning question in international political economy was the survival of the postwar international economic order despite the seeming decline of the United States, the hegemon that had created this order. Some political economists were surprised that neither Germany nor Japan had attempted to restructure international economic relations to suit their respective interests.[6] Both questions assumed that the robustness of the political and economic status quo was an extraordinary anomaly that required an equally extraordinary explanation.[7]

Attempts to explain the unexpected stability of the postwar political and economic order, and the controversy these explanations provoked, pushed the problem of change out of the pages of the principal journals and into obscurity, where it remained until the Berlin Wall was breached.[8] No major theory of international relations made change its principal focus. Even theories that incorporated some concept of change made no attempt to specify the conditions under which it would occur.[9] In the absence of a theoretical interest in change, there was no debate about how or why the postwar order might evolve or be transformed. Scholars became insensitive to the prospect that such change could occur.

In a deeper sense, my field's blindness was attributable to the political assumptions that shaped leading scholars' worldviews and research agendas. The absence of superpower war seemed extraordinary because of the widely shared belief that the Soviet Union was an aggressive and expansionist adversary; for some, it was the linear descendant of Hitler's Germany. If scholars had regarded Soviet leaders from Khrushchev as fundamentally satisfied with the status quo and concerned less with making gains than with avoiding losses—and there is much evidence to support this interpretation—the nonoccurrence of World War III would not have required any extraordinary explanation.

Cold War critics were equally myopic. Those who considered the nuclear arms race and its escalatory potential to be the major source of tension in East-West relations directed their scholarly attention to the domestic and international causes of the arms race and the ways it might be halted or stabilized through arms control and security regimes. Once again, there was little recognition or study of the possibility that the underlying conflict might undergo—or indeed, had already undergone—a profound transformation.

The same bias affected the study of political economy. The reigning orthodoxy, imported from classical economics, assumes that states are rational and seek to maximize gain. If scholars had started from the premise that German and Japanese bankers and industrialists, like their counterparts elsewhere in the world's capitalist establishment, were anxious above all else to preserve order and predictability—especially in a system from which they profited so handsomely—they would not have viewed the survival of the postwar international economic framework as anomalous. Japanese and Western European efforts to preserve the system would have been judged simple common sense.

Theory is supposed to free scholars from their political, generational, and cultural biases. In social science, it often does the reverse, and worse still, confers an aura of scientific legitimacy on subjective political beliefs and prejudices. Logical positivism and other "unity of science" approaches depict science as independent of the culture, life experiences, and personalities of scientists.[10] According to these epistemologies, sci-

5. John Lewis Gaddis, *The Long Peace: Inquiries Into the History of the Cold War Era* (New York: Oxford University Press, 1987), p. 218.

6. See Charles Kindleberger, *The World in Depression, 1929–1939* (Berkeley: University of California Press, 1973); Robert Gilpin, *War and Change in World Politics* (Cambridge: Cambridge University Press, 1981). For critical discussions and alternate explanations, see Robert Keohane, *After Hegemony* (Princeton: Princeton University Press, 1984); Keohane, *International Institutions and State Power* (Boulder, Colo.: Westview, 1989); Duncan Snidal, "The Limits of Hegemonic Stability Theory," *International Organization*, Vol. 39 (Autumn 1985), pp. 579–614; and Volker Rittberger, ed., *The Study of Regimes in International Relations* (New York: Oxford University Press, forthcoming).

7. The focus of realism is great power relations. In describing the postwar political order as stable, realists are referring to the stability of Europe, and the de facto and later de jure acceptance of its division by East and West. The postwar political "order" in other regions of the world could hardly be called stable.

8. A literature search reveals that between 1970 and 1990, *International Organization, World Politics*, and *International Studies Quarterly* published no more than a half-dozen articles whose primary focus was major foreign policy or systemic change.

9. An exception is Gilpin, *War and Change in World Politics*. This point is also made by John Gerard Ruggie, "Continuity and Transformation in the World Polity: Toward a Neorealist Synthesis," Robert O. Keohane, "Theory of World Politics: Structural Realism and Beyond," and Robert W. Cox, "Social Forces, States and World Orders: Beyond International Relations Theory," all in Robert O. Keohane, ed., *Neorealism and Its Critics* (New York: Columbia University Press, 1986), pp. 148–149, 179–181, 197–198, 243–245. For a critique of cognitive psychology's failure to deal adequately with change, see Richard Ned Lebow and Janice Gross Stein, "Afghanistan, Carter and Foreign Policy Change: The Limits of Cognitive Models," in Dan Caldwell and Timothy J. McKeown, eds., *Diplomacy, Force, and Leadership: Essays in Honor of Alexander L. George* (Boulder, Colo.: Westview, 1993), pp. 95–128.

10. "Unity of science" approaches see no differences between the goals and proper practices of the social and physical sciences. Logical positivism has for many years

ence is supposed to respond to its own imperatives; previous discoveries unearth anomalies or open up promising lines of inquiry that are investigated by subsequent scientists. But the ideas that propel science to the next stage of inquiry rarely grow out of existing research. Thomas Kuhn and others have shown how revolutions in science are triggered by fundamental shifts in *gestalt* that identify new problems and new kinds of solutions to them.[11] To explain these gestalt shifts—in all scholarly enterprises—one must generally look beyond the lab and the archive.

Research agendas, especially in history and social science, reflect political, institutional, and personal agendas.[12] The historiography of World War I and the Cold War illustrate how ideology and current events drive scholarship. The Treaty of Versailles justified reparations by holding Germany responsible for war in 1914. The German government signed the Treaty, but categorically denied its responsibility for the war, and published a selective and carefully edited collection of documents to buttress its claim of innocence. Diplomatic history in the 1920s and 1930s was dominated by the *Kriegschuldfrage*. Predictably, works that upheld the Allied position provoked an equal and opposite reaction: revisionist scholarship shifted the mantle of blame onto the shoulders of Russia, France, and Britain, and attempted to undercut the justification for reparations. Decades later, the Berlin and Cuban missile crises revived interest in World War I. This time, concern that World War III might arise from miscalculation, accidents, loss of control, or runaway escalation led historians and political scientists to mine the crisis of 1914 for contemporary policy lessons. Around the same time, historians began to reexamine the deeper causes and meaning of World War I, its links to World War II, and

been associated with this position, but other approaches, among them positivism and empiricism, subscribe to it as well. There is a lot of confusion in political science; practitioners routinely use the term logical positivism to refer to all of these approaches. For appropriate definitions, see Friedrich Kratochwil, "Why Sisyphus is Happy: Reflections on the 'Third Debate' and on Theorizing as a Vocation," *Sejong Review*, Vol. 3 (November 1995), pp. 3–36.

11. Theodore S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

12. Michael Oakeshott, *Experience and Its Modes* (New York: Cambridge University Press, 1990); Benedetto Croce quoted in E.H. Carr, *What is History?* (Harmondsworth: Penguin, 1964), pp. 20–21; Carr, *What is History?* pp. 16, 29–30; Levy, "Too Important to Leave to the Other," pp. 26–27; and Stephen H. Haber, David M. Kennedy, and Stephen D. Krasner, "Brothers Under the Skin: Diplomatic History and International Relations," *International Security*, Vol. 22, No. 1 (Summer 1997), pp. 34–43, especially pp. 37–38.

implications for developments elsewhere in the world.[13] This process has accelerated since the end of the Cold War.[14]

The historiography of the Cold War underwent a parallel evolution. In the 1950s and 1960s, scholarship focused on the question of Cold War "guilt." Conservatives and Cold War liberals blamed Stalin, communism, and the Soviet Union. Revisionist scholarship, which began in the 1950s but really flourished a decade later in response to the Vietnam War, held capitalism and the United States responsible for the Cold War. The collapse of the Soviet Union, and the access this permitted to hitherto unavailable documents, have led some anti-Stalinists in the West to claim victory, but by the 1970s the question of who started the Cold War had become largely passé. In response to détente, students of the Cold War shifted their attention to the questions of how a war-threatening conflict was gradually transformed into a more stable rivalry. Now that the Cold War is over, historians will presumably begin to examine the broader meaning of the Cold War, and do so with an eye on the issues of the moment. A case in point is a paper by Paul Schroeder that uses the concept of the *longue durée* to analyze World Wars I and II and the Cold War as a part of an iterative cycle of the creation, entrenchment, decline, collapse, and reconstitution of legitimate international orders.[15]

Research on World War I and the Cold War shifted in response to contemporary political developments. Scholars looked to the past for guidance about the present. The answers they found reflected their political views and starting assumptions. Neopositivism and the "new international history" are naive in their belief that anything else is possible. It is process, not motive, that distinguishes good from bad scholarship.

Process begins with the identification of an important question or puzzle. These arise when we encounter behavior at odds with our expectations. Expectations are always theory-driven; they are based on underlying beliefs about how the world works. Sometimes these beliefs are well specified, more often they are unspoken. When we observe a business buy dear and sell cheap, or a state attack a more powerful neighbor, we

13. See, for example, Hajo Holborn, *The Political Collapse of Europe* (New York: Alfred Knopf, 1963); and Arno J. Mayer, *Wilson versus Lenin: Political Origins of the New Diplomacy, 1917–1918* (New Haven: Yale University Press, 1959).

14. The historiography of World War I and the Cold War became intertwined in the Federal Republic of Germany in the 1960s. Anticapitalist and anti-American feeling ran high among intellectuals, and found expression in the Fischer thesis, which stressed the continuity of German history, from Bismarck through Adenauer.

consider the behavior anomalous because it appears to violate well-established principles of economics and international relations. If we dismiss the actors as ill-informed, incompetent, or crazy, the puzzle disappears, although it may give rise to the secondary one of how such people could have achieved positions of authority. To make "sense" of seemingly anomalous behavior, that is, to square it with accepted principles without relaxing the assumption of rationality, we look for other considerations specific to the situation that may have dictated choice and can ultimately be reconciled with the principles. A business may sell for a loss if there is a glut on the market or its managers expect prices to decline precipitously. A weak state may attack a strong one if its military has a strategy and tactics that it expects to negate the adversary's putative advantages.

Another way to make sense of anomalous behavior is to revise the beliefs or principles that make it appear anomalous. Do they rest on inappropriate assumptions? Ignore more important determinants of behavior? Leave unspecified, or improperly specified, the scope conditions under which they hold? The debate about the end of the Cold War is at its core a controversy about the validity of the assumptions that shaped Western understanding of the Soviet Union and its foreign policy. Conservatives, including some realists, insist that the end of the Cold War validated their assumptions that foreign policy is driven by power calculations; the Soviet Union's decline compelled Gorbachev to seek accommodation with the West on unfavorable terms. Other realists contend that their assumptions are valid, but that the outcome was anomalous because Mikhail Gorbachev made serious miscalculations. Critics of realism have used the end of the Cold War to argue that structure is indeterminate, and that policy choices are significantly shaped by ideas, domestic politics, and the preferences of leaders.[16]

Scholars must always remain open to the possibility that there is something wrong with their premises. Gerhard Weinberg maintains that historians, unlike social scientists, recognize the transience of their arguments and theories and are open to the possibility that new evidence may compel their revision or rejection.[17] The historiography of major controversies indicates to me that historians are just as committed to their interpretations as social scientists are to their theories. They are more likely to assimilate new "facts" to their theories than to use them as catalysts for rethinking. Historians and social scientists alike need to make explicit the underlying assumptions that guide their research, and ask themselves what kinds of evidence could falsify their theories or interpretations or lead them to approach the problem from a different set of assumptions. Self-awareness and self-questioning is the most difficult and most neglected part of good process, and, as the following section argues, one of the most essential.

False Confirmation

Historical debates are most productive when they focus scholarly attention on underlying assumptions and principles. It often takes seemingly anomalous behavior to spark such a debate. Gorbachev's withdrawal from Afghanistan, liberation of jailed dissidents, push for free elections, and willingness to let Eastern European people decide their own political futures are cases in point. This is because of the cognitive tendency to assimilate information, even disconfirming information, to existing beliefs, principles, or theories.[18]

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16. See, for example, Kenneth N. Waltz, "The Emerging Structure of International Politics," *International Security,* Vol. 18, No. 2 (Fall 1993), pp. 5–43; William C. Wohlforth, "Realism and the End of the Cold War," *International Security,* Vol. 19, No. 3 (Winter 1994/95), pp. 91–129; Kenneth A. Oye, "Explaining the End of the Cold War: Morphological and Behavioral Adaptations to the Nuclear Peace?" in Richard Ned Lebow and Thomas Risse-Kappen, eds., *International Relations Theory and the End of the Cold War* (New York: Columbia University Press, 1995), pp. 57–84; Thomas Risse-Kappen, "Ideas Do Not Float Freely: Transnational Coalitions, Domestic Structures, and the End of the Cold War," in Lebow and Risse-Kappen, eds., *International Relations Theory and the End of the Cold War,* pp. 187–222; Richard Ned Lebow, "The Long Peace, the End of the Cold War, and the Failure of Realism," in Lebow and Risse-Kappen, eds., *International Relations Theory and the End of the Cold War,* pp. 23–56; Robert G. Herman, "Identity, Norms and National Security: The Soviet Foreign Policy Revolution and the End of the Cold War," in Peter J. Katzenstein, ed., *The Culture of National Security: Norms and Identity in World Politics* (New York: Columbia University Press, 1996), pp. 271–316; and Jeffrey T. Checkel, *Ideas and International Political Change: Soviet/Russian Behavior and the End of the Cold War* (New Haven: Yale University Press, 1977).

17. Comments at "History and International Relations Theory Conference," Tempe, Ariz., January 15–18, 1998.

18. Human beings use knowledge structures, or "schemas," to cope with the enormous amount of information they receive. Information is assimilated to these schemas. On the limitations of human information-processing, see Richard Nisbett and Lee Ross, *Human Inference: Strategies and Shortcomings of Social Judgment* (Englewood Cliffs, N.J.: Prentice-Hall, 1980); and James Galambos, Robert Abelson, and John Black, eds., *Knowledge Structures* (Hillsdale, N.J: Lawrence Erlbaum, 1986). On biases and heuristics in information-processing, see Daniel Kahneman, Paul Slovic, and Amos Tversky, *Judgment Under Uncertainty: Heuristics and Biases* (New York: Cambridge University Press, 1982). For a critique of this model, see Susan T. Fiske and Shelley E. Taylor, *Social Cognition,* 2nd ed. (New York: McGraw-Hill, 1991), esp. pp. 554–558.

19. Irving L. Janis, *Victims of Groupthink: A Psychological Study of Foreign Policy Decisions and Fiascoes,* 2nd ed. (Boston: Houghton Mifflin, 1972); Irving L. Janis and Leon

~~Scholars who have built careers on~~ particular interpretations are generally reluctant to recognize problems with those interpretations. When beliefs reflect strong emotional needs to maintain a particular construction of reality, they can be altogether impervious to discrepant information.

The deterrence debate—for many years, a nondebate—gives ample testimony to how cognitive and motivational biases can reinforce each other. Modern deterrence theory developed in response to the recognition that nuclear wars were too destructive to be a rational instrument of war, but that their very destructiveness might be exploited to prevent war. The classic formulation of this paradox is found in Bernard Brodie's 1946 study, *The Absolute Weapon*.[20] In the "golden age" of deterrence theory, the 1950s and 1960s, Bernard Brodie, William Kaufmann, and Thomas Schelling developed formal models of nuclear deterrence. They argued that it could be rational to threaten an irrational act, and explored ways in which deterrent and compellent threats of nuclear annihilation might be made credible.[21]

Deterrence theory gained widespread acceptance in academe and government for intellectual, political, and psychological reasons. Its elegance and simplicity appeared to offer scholars a powerful and widely applicable instrument to analyze and predict strategic behavior. For policymakers, it held out the prospect of exploiting an unusable weapon to achieve political goals. On a deeper level, deterrence was a psychological bulwark against nuclear war. If, as deterrence theory maintained, nuclear war could only come about because an adversary believed that its enemy could not retaliate in kind, war could be prevented by possession of a secure second-strike capability.[22]

Deterrence was confirmed tautologically. The United States buttressed its commitments in Berlin (1948–49, 1958–69, and 1961), the Taiwan Straits (1954 and 1958) and other parts of the world when they appeared threatened by the Soviet Union or China. When no military challenge occurred, politicians and analysts attributed communist restraint to U.S. deterrence. When deterrence failed—the most notable example is the Soviet attempt to deploy strategic missiles in Cuba in 1962—it was also explained in terms of deterrence theory. Kennedy administration officials and scholars assumed that Khrushchev challenged the United States because the president's youth and his lackluster performance in the Bay of Pigs crisis and at the Berlin and Vienna summits had given him good grounds to question U.S. resolve. They attributed Khrushchev's withdrawal of the missiles to Kennedy's credible display of military capability and resolve to use force, if necessary, to take the missiles out.[23] Deterrence was also given credit for the overall absence of nuclear war and the end of the Cold War. The conventional wisdom holds that Gorbachev sought an accommodation because the Soviet Union could no longer compete economically or militarily with the United States.

Recent evidence from Soviet and Chinese archives offers little support for any of these interpretations. In the Taiwan Straits crisis, the Chinese government's goal was to deter Taiwan and the United States from using force against the Chinese mainland.[24] Khrushchev sent missiles to Cuba not to force a trade-off in Berlin, as the Kennedy administration surmised, but to protect Castro from an expected U.S. invasion,

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Mann, *Decision Making: A Psychological Analysis of Conflict, Choice and Commitment* (New York: Free Press, 1977); and Richard Ned Lebow, *Between Peace and War: The Nature of International Crisis* (Baltimore: Johns Hopkins University Press, 1981).

20. Bernard Brodie, *The Absolute Weapon* (New York: Harcourt, Brace, 1946).

21. William K. Kaufmann, *The Requirements of Deterrence* (Princeton: Center of International Studies, 1954); Bernard Brodie, *Strategy in the Missile Age* (Princeton: Princeton University Press, 1959); Henry A. Kissinger, *The Necessity for Choice* (New York: Harper, 1960); Thomas Schelling, "Controlled Response and Strategic Warfare" (London: International Institute of Strategic Studies, June 1965); and Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966).

22. For the psychological roots of deterrence, see Philip Green, *Deadly Logic: The Theory of Nuclear Deterrence* (Columbus: Ohio State University Press, 1966); Robert Jervis, *The Illogic of American Nuclear Strategy* (Ithaca: Cornell University Press, 1984), esp. pp. 22, 36, 37; and Steven Kull, *Minds at War: Nuclear Reality and the Inner Conflicts of Defense Policymakers* (New York: Basic Books, 1988).

23. Elie Abel, *The Missile Crisis* (Philadelphia: Lippincott, 1962), pp. 35–36; James Reston, "What Was Killed Was Not Only the President But the Promise," *New York Times Magazine*, November 15, 1964, p. 126; Arthur M. Schlesinger, Jr., *A Thousand Days: John F. Kennedy in the White House* (Boston: Houghton Mifflin, 1965), pp. 391, 796; Theodore C. Sorensen, *Kennedy* (New York: Harper & Row, 1965), pp. 676, 724; Arnold Horelick and Myron Rush, *Strategic Power and Soviet Foreign Policy*, (Chicago: University of Chicago Press, 1966), pp. 142–143; Graham T. Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little, Brown, 1971), pp. 231–235; and Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974), p. 465. For a critique, and a different interpretation of Khrushchev's decision to send missile to Cuba, see Richard Ned Lebow, "The Cuban Missile Crisis: Reading the Lessons Correctly," *Political Science Quarterly*, Vol. 98 (Fall 1983), pp. 431–458; Raymond L. Garthoff, *Reflections on the Missile Crisis*, rev. ed. (Washington, D.C.: Brookings, 1989); and Richard Ned Lebow and Janice Gross Stein, *We All Lost the Cold War* (Princeton: Princeton University Press, 1994), especially chap. 4.

24. For an early version of this thesis, see Melvin Gurtov and Byong-Moo Hwang, *China Under Threat: The Politics of Strategy and Diplomacy* (Baltimore: Johns Hopkins University Press, 1980), pp. 63–98. See Shu Guang Zhang, *Deterrence and Strategic*

offset U.S. strategic superiority, and to get even with the president for deploying Jupiter missiles in Turkey. Kennedy had viewed these measures as prudent, defensive precautions against perceived Soviet threats. His actions had the unanticipated consequence of convincing Khrushchev of the need to protect the Soviet Union and Cuba from U.S. military and political challenges. Khrushchev withdrew the missiles to avoid war, but also because of Kennedy's public promise not to invade Cuba and his secret promise to withdraw the missiles in Turkey after a decent interval.[25]

The ultimate irony of nuclear deterrence may be that the strategy of deterrence undercut much of the political stability that the reality of deterrence should have created. The arms buildups, threatening military deployments, and confrontational rhetoric that characterized the strategy of deterrence obscured deep and mutual fears of war. Fear of nuclear war made leaders inwardly cautious, but their public posturing convinced their adversaries that they were aggressive, risk-prone, and even irrational. In Cuba, we now know, deterrence provoked the behavior it was meant to prevent.[26]

The intellectual history of deterrence highlights the disturbing ease with which beliefs can become entrenched. Even dramatically disconfirming events—this is how I read the Cuban missile deployment and Gorbachev's foreign policy revolution—can be explained away by true believers. Change, to the extent it occurs, is more likely to be generational; younger scholars, responding to novel political situations and intellectual currents, adopt new points of view. This is a slow and inefficient process. In the twentieth century it has also had disastrous political consequences. The hard-line deterrence strategy that characterized the U.S. approach to the Soviet Union throughout most of the Cold War was a response to the failure of appeasement in the 1930s. Appeasement in turn was a reaction to the more confrontational policies that were believed to have led to World War I. In each conflict, statesmen and generals prepared to prevent or fight the previous war.

Culture: Chinese-American Confrontations, 1949–1958 (Ithaca: Cornell University Press, 1992), for a more recent study making extensive use of Chinese documents and interviews.

25. Garthoff, *Reflections on the Missile Crisis;* Lebow and Stein, *We All Lost the Cold War;* and James G. Blight, Bruce J. Allyn, and David A. Welch, *Cuba on the Brink: Castro, The Missile Crisis and the Soviet Collapse* (New York: Pantheon, 1993).

26. Lebow and Stein, *We All Lost the Cold War;* and Ted Hopf, *Peripheral Visions: Deterrence Theory and American Foreign Policy in the Third World, 1965–1990* (Ann Arbor: University of Michigan Press, 1994).

Overdetermination

The late Sir Isaiah Berlin popularized the Greek poet Archilochus's distinction between hedgehogs and foxes.[27] Hedgehogs know one big thing, know it very well, and succeed by invoking it repeatedly. Foxes know many things, are inventive, and tailor their strategies to circumstances. Social scientists are more likely to be hedgehogs. They look for parsimonious explanations for seemingly complex events and assume that those explanations, and any strategies based on them, will be applicable in a wide range of situations. Historians are more likely to be foxes. They tend to treat every historical situation as unique, and are likely to propose varied and layered explanations on the assumption that complex events have complex causes. Reality, in the words of Melvyn Leffler, "is too complex to be captured by a single theory."[28] This approach has limitations of its own.

Multiple causation can take two forms. The first, known as overdetermination, occurs when several causes are present, any one of which could have produced the observed outcome. The second is when the combined effects of two or more causes are necessary to bring about the outcome. Historians, like their social scientist colleagues, need to specify which use of multiple causation they intend. Historical treatments of the Cold War sometimes fail to do this.

John Gaddis's writings illustrate this problem. In *The Long Peace,* he accepts Kenneth Waltz's contention that bipolarity was the principal structural cause of peace, and, Gaddis adds, of the unexpected stability of the postwar division of Europe.[29] It was an easy structure to maintain, encouraged stable alliances, and reduced the importance of individual defections from either alliance system. But Gaddis also contends that "what has really made the difference in inducing unaccustomed caution" was nuclear deterrence. He then offers a third cause of peace: the "rules" the superpowers evolved to regulate their competition. These rules in-

27. Isaiah Berlin, *The Hedgehog and the Fox: An Essay on Tolstoy's View of History* (New York: Simon and Schuster, 1966).

28. Melvyn P. Leffler, "New Approaches, Old Interpretations, and Prospective Reconfigurations," *Diplomatic History,* Vol. 19, No. 2 (Spring 1995), pp. 173–196, quote on p. 179; Edward Kiser, "Revival of the Narrative in Historical Sociology: What Rational Choice Theory Can Contribute," *Politics and Society,* Vol. 24 (September 1996), pp. 249–271, offers the view of theory as a "toolbox," an approach that should be appealing to historical "foxes."

29. Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979); and John Lewis Gaddis, *The Long Peace: Inquiring into the History of the Cold War* (New York: Oxford University Press, 1987).

cluded respect for each other's sphere of influence, and a commitment to avoid direct military confrontation and to use nuclear weapons only as an ultimate resort.

Waltz distinguished between "peace" (the absence of superpower war) and "stability" (the endurance of the bipolar system). Gaddis elides the two concepts, ruling out the possibility—which came to pass a few years after the publication of his book—that a bipolar system could be transformed without a war between its poles. Gaddis fails to tell us whether any or all of his structural and behavioral causes of peace are necessary and sufficient. Could peace have been preserved by any one of them? If not, which was (or were) the most important? And what about the relationship between these several causes? Surely, bipolarity and nuclear weapons were not unrelated; the latter helped to establish the former. Many realists would probably argue that the rules of the road Gaddis finds so important were a response to bipolarity or nuclear weapons.

His most recent book, *We Now Know: Rethinking Cold War History*, has the same problem.[30] He attributes the Cold War to Stalin's personality and ideology. Gaddis has no doubts about it; Stalin sought a Cold War the way "a fish seeks water."[31] Stalin also sought to extend Soviet territory and territorial control for security reasons. In Eastern Europe, where this control was ensured through military occupation and the imposition of Soviet-style puppet governments, Stalin's policy posed a direct challenge to Britain and the United States. But what *really* made the Cold War inevitable, Gaddis argues, was the coercive and crude way in which Stalin pursued his goals. Churchill, Atlee, Roosevelt, and Truman had to defend their policies to voters, and Stalin's failure to mask the extension of Soviet power behind the outward forms of democracy (plebiscites, elections, indirect rule through dependent but popularly elected governments) made it unacceptable to British and U.S. leaders.

Following Norman Naimark, Gaddis argues that Stalin's reliance on coercion and brutality was a reflection of the political-economic limitations of the Soviet system.[32] This was most evident in the occupation of Germany, where rape and pillage were unconstrained, and whole factories, rolling stock, equipment, and scientific personnel were forcibly removed to the Soviet Union. The United States was able to exercise influence in more subtle and effective ways, and worked collaboratively with elected governments. Washington also won the support of Western Europeans by providing extensive economic aid and credits for reconstruction. The asymmetry in political, administrative, and economic resources between the superpowers accounted "more than anything else, for the origins, escalation and ultimate outcome of the Cold War."[33]

Gaddis also maintains that Stalin's foreign policy was a direct extension of his domestic policy, and the overriding goal of both was to intimidate or, better yet, eliminate potential challengers. Cooperation, other than for purely tactical reasons, was alien to his nature. This leaves the door open to the possibility that another Soviet leader would have pursued a different policy in Eastern Europe and the Far East. But elsewhere Gaddis slams this door shut with his insistence that Stalin, and other officials, like Molotov, were prisoners of Marxist ideology. They believed that sooner or later—perhaps in as little as fifteen years—there would be another crisis of capitalism that would compel the leading capitalist powers to go to war to deflect domestic unrest. The United States would unleash a rearmed Germany against the Soviet Union. For protection, Soviet forces needed to control Germany and extend their defensive *glacis* as far west as possible.

There is an unresolved ambiguity in Gaddis's *We Now Know* about the Cold War and Stalin's relation to it. The regime, personality, and ideological explanations for the Cold War all point to an underlying defensive motivation: Stalin's personal, political, and *real politik*–driven need to expand Soviet influence. But Gaddis also advances a more offensive explanation. Stalin simply wanted to dominate Europe, and ultimately the world, but unlike Hitler, he was patient and "prepared to take as long as necessary to achieve his ambitions." But in Asia, Stalin threw caution to the wind, succumbed to "ideological euphoria," and allowed Kim Il-Sung to talk him into an invasion of South Korea. Some of these explanations are contradictory; others are related, but that relationship is left undefined; others may be epiphenomenal (i.e., due to other causes), and still others conflate cause and effect. Historians who offer multilayered explanations need to identify what kind of multiple causation they mean (Gaddis uses both interchangeably), distinguish *between* competing causes (offensive vs. defensive goals, and personality vs. ideology in this case), and rank order those that could be reinforcing (for Gaddis, regime capabilities, ideology, and personality). They also need to describe what-

30. John Lewis Gaddis, *We Now Know: Rethinking Cold War History* (New York: Oxford University Press, 1997).

31. Ibid., p. 25.

32. Norman N. Naimark, *The Russians in Germany: A History of the Soviet Zone of Occupation, 1945–1949* (Cambridge: Harvard University Press, 1995).

33. Ibid., p. 17.

ever relationships exist between or among these causes. Failure to do this makes the overall argument impossible to sustain or falsify.

Counterfactual Arguments

Some prominent historians have dismissed counterfactual thought experiments as idle parlor games.[34] Counterfactual assumptions nevertheless lie at the core of all historical inference.[35] Implicit in every historical interpretation is the counterfactual that the outcome would *not* have occurred in the absence of the stated causes. If the Cold War was Stalin's fault, it follows that it would not have happened if a different leader had occupied the Kremlin—unless that leader had wanted that conflict for reasons of his own. Counterfactuals of this kind most often go unexamined. In the Soviet case, one counterfactual has received considerable attention: would communism have evolved differently if Lenin had lived longer, or if he had been succeeded by someone other than Stalin?[36] While this question is unanswerable, attempts to address it have usefully focused attention on the assumptions that guide and sustain different arguments about the role of Stalin and the nature of the Soviet system, and in doing so, have encouraged a more sophisticated historical debate.

Counterfactual thought experiments have an important role to play in Cold War scholarship. As noted above, they are a useful device for prodding historians and political scientists to make explicit the assumptions that guide their analysis and interpretations. They are also a useful tool to help formulate and specify these theories. John Gaddis alleges that Stalin was responsible for the Cold War. If Gaddis had asked himself if there still would have been a Cold War in the absence of Stalin, he would

34. According to A.J.P. Taylor, "a historian should never deal in speculation about what did not happen." *Struggle for the Mastery of Europe, 1848–1918* (London: Oxford University Press, 1954). M.M. Postan, writes: "The might-have beens of history are not a profitable subject of discussion," quoted in J.D. Gould, "Hypothetical History," *Economic History Review*, 2nd ser., Vol. 22 (August 1969), pp. 195–207. See also David Hackett Fischer, *Historian's Fallacies: Towards a Logic of Historical Thought* (New York: Harper Colophon Books, 1970), pp. 15–21; and Peter McClelland, *Causal Explanation and Model-Building in History, Economics, and the New Economic History* (Ithaca: Cornell University Press, 1975).

35. James D. Fearon, "Counterfactuals and Hypothesis Testing in Political Science," *World Politics*, Vol. 43 (January 1991), pp. 169–195.

36. George W. Breslauer, "Counterfactuals Reasoning in Western Studies of Soviet Politics and Foreign Relations," in Philip E. Tetlock and Aaron Belkin, eds., *Counterfactual Thought Experiments in World Politics: Logical, Methodological, and Psychological Perspectives* (Princeton: Princeton University Press, 1996), pp. 69–94, discusses this

have been forced to decide if Stalin was a necessary and sufficient condition for that conflict. Removing Stalin from the scene would also have encouraged Gaddis to consider what else about the Soviet Union would have been different. Would foreign policy, for example, still have been subordinate to domestic policy, or subordinate in the same way? Counterfactual thought experiments of this kind could have helped Gaddis to rank order his explanations and the many links among them.

Counterfactual thought experiments are also useful in refuting others' explanations. Because every argument has its related counterfactual, critics have two strategies open to them: they can try to offer a different and more compelling account, or they can try to show that the outcome in question would still have occurred in the absence of the claimed causes. John Mueller's account of the Cold War is a nice example of the second strategy. In contrast to the conventional wisdom that attributed the "long peace" between the superpowers to nuclear deterrence, he argues that Moscow and Washington were restrained by their general satisfaction with the status quo, and secondarily by memories of World War II and the human, economic, and social costs of large-scale, conventional warfare. The unheralded destructiveness of nuclear weapons was redundant, and possibly counterproductive.[37]

Because historians typically study single cases, history confronts what social scientists call the "small-n problem." Single case studies can always be challenged as unrepresentative of the phenomenon in question. Validation is especially difficult when outcomes are attributed to multiple causes. Historians typically attempt to establish causation by process tracing. They try to document the links between a stated cause and an outcome. This works best at the individual level of analysis, but only when there is enough evidence to document the actors' calculations and motives. Even when such evidence is available, it may not permit historians to determine the relative weight of the several causes alleged to be at work, and which, if any, might have produced the outcome in the absence of the others.

Counterfactual analysis introduces variation through thought experiments that add or subtract contextual factors or possible causes, and ask how this would have influenced the outcome. Thought experiments

37. See John Mueller, *Retreat from Doomsday: The Obsolescence of Major War* (New York: Basic Books, 1989), and the debate on this subject between Mueller, "The Essential Irrelevance of Nuclear Weapons: Stability in the Postwar World," and Robert Jervis, "The Political Effects of Nuclear Weapons: A Comment," in *International Security*, Vol. 13, No. 3 (Fall 1988), pp. 55–90.

allow researchers to build in the kinds of controls normally achieved only in a laboratory. They suffer from the obvious drawback: it is generally impossible to know the consequences of variation introduced by the experimenter. This uncertainty increases dramatically when the experimenter considers second- or third-level consequences of the counterfactual. Suppose we postulate that Archduke Franz Ferdinand was not assassinated in June 1914. This counterfactual involves a minimal rewrite of history; if the carriage carrying the archduke and his wife had not made a wrong turn, Prinzip "would not have had an opportunity to shoot the Royals at point-blank range." Deprived of the pretense provided by the assassination, it seems highly unlikely that Austria-Hungary would have presented Serbia with an ultimatum and would have gone to war when it was rejected. World War I would have been averted, at least temporarily. What would have happened next is very hard to say.

The speculative nature of counterfactual thought experiments makes many historians wary of them. But counterfactual analysis does not always have to be as speculative as the longer-term consequences of the survival of Archduke Franz Ferdinand. Deterrence offers a nice counterexample. One of the principal policy "lessons" of the 1930s is that appeasement whets the appetites of dictators whereas military capability and resolve is likely to restrain them. The failure of appeasement in the 1930s is readily apparent, but the putative efficacy of deterrence rests on the counterfactual that Hitler could have been restrained by France and Britain if they had credibly demonstrated their willingness to go to war in defense of the status quo. German documents make this possibility an eminently researchable question, and historians have used these documents to try to determine at what point Hitler was no longer deterrable.[41] Their findings have important implications for any assessment of French and British policy and the broader claims made for deterrence. The Cuban missile crisis is another evidence-rich environment in which to study counterfactuals. Key policy choices—Khrushchev's decision to send and remove missiles from Cuba, and Kennedy's decision to impose a blockade—and subsequent scholarly analyses were both contingent upon hypothetical antecedents. Kennedy believed, incorrectly, that Khrushchev sent missiles because Khrushchev doubted his resolve, and would not have sent them if he had taken a stronger stand at the Bay of Pigs and in Berlin. He reasoned that he had to prevent the installation of the missiles to convince Khrushchev of his resolve and deter a subsequent and more serious challenge to Berlin.

41. Yuen Foong Khong, "Confronting Hitler and Its Consequences," in Tetlock and Belkin, eds., *Counterfactual Thought Experiments in World Politics*, pp. 95–118.

My arguments have borrowed heavily from neopositivist epistemology. I have not spoken of prediction—the holy grail of neopositivism—because I think predictive theories are impossible in international relations and most other domains of social inquiry. I believed that explanation—identification of the causal mechanisms responsible for given outcomes—is a more realistic goal, and one to which many historians aspire. Studies of the Cold War that seek to explain its origins, dynamics, evolution, termination, or relationship to other conflicts indicate this commitment. Many of these studies reflect a "soft" positivist epistemology, and can accordingly be evaluated in terms of neopositivist protocols for hypothesis construction and testing.

Positivism, in both its "hard" and "soft" formulations, assumes that reality has an objective existence that is outside and independent of the language and conceptual categories used to describe and analyze it. This assumption, and positivism more generally, has come under increasing attack in the social sciences. The principal alternative in international relations theory, "constructivism," is very much in the interpretivist tradition. Like other interpretivist approaches, it assumes that reason and irrationality are constitutive of actors and the societies in which they are embedded. Constructivists emphasize the intersubjective understandings actors have of themselves, other actors, and their relationships with these actors. Constructivist research suggests that categories of analysis used by international relations scholars often bear little relationship to the categories actors themselves use to frame problems, evaluate their interests, make policy, and draw lessons.[51]

For interpretivists, empathetic understanding from inside (*verstehen*), not explanation (*erklären*), is the goal of scholarly inquiry.

The ongoing epistemological debate in international relations theory has important implications for the relationship between international relations and history. For most of the Cold War, the international relations literature was realist and neopositivist, while Cold War history ran the gamut from interpretivist to neopositivist. These epistemological differences made dialogue difficult; the only real conversations were between realists in both disciplines, and between diplomatic historians and the small community of interpretivist political scientists who used primary historical sources to reconstruct events from the perspectives of the actors involved. Jack Levy argues that the prospect for dialogue is diminishing because of the "revival of narrative" and the "linguistic turn" in history, and the further spread of quantitative methods and game theory in international relations.

Historical Sociology & "World System" Theory

IMMANUEL WALLERSTEIN

In *Fifty Key Thinkers in International Relations* (M. Griffiths, ed.), 2008

Immanuel Wallerstein began his career as a student of African politics, specializing in Ghana and the Ivory Coast. But his reputation as an international theorist is based on his radical attempts to reconceptualize international relations in the context of his arguments concerning the nature and history of the modern capitalist "world-system". Wallerstein is the pioneer of world-systems theory, which is based in part on radical dependency theories of underdevelopment as well as the French Annales School of historiography. In three pioneering volumes of extraordinary historical detail and theoretical ambition, Wallerstein has attempted to look beneath the epiphenomena of diplomatic and military relations among states to grasp the logic of a single world-system.

It is important to understand at the outset that the term "world-system" does not refer primarily to the geographical scope of capitalism, merely to the fact that the logic of the system operates at a different level than any existing political unit such as the nation-state. His most famous text, *The Modern World-System: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century*, published in 1974, locates the origins of the modern world in what he called "the long sixteenth century", from around 1450 to 1670. Before this period, Western Europe was feudal and economic production was based almost entirely on agriculture. From 1300 onwards, however, agricultural production fell rapidly. It was not until the 1500s that Europe moved towards the establishment of a capitalist world-economy, in which production was oriented towards exchange in the market rather than seasonal consumption and those who produced goods earned less than their value.

Economic growth in the new era entailed the expansion of the geographical scope of the market, the development of different forms of labor control, and the rise of strong states in Europe. The new world-economy that emerged differed from previous empires in that it coexisted with a multiplicity of political jurisdictions and was characterized by a new international division of labor between "core" and "periphery". The core refers to those regions that benefited most from change. In the period of initial expansion, this included most of northwestern Europe (France, England, Holland). The region was characterized by strong central governments and large mercenary armies. The latter enabled the bourgeoisie to control international commerce and extract economic surplus from trade and commerce. The growth of urban manufacturing was fed by movements of landless peasants from the countryside to the cities, while improvements in agricultural technology ensured continuous increases in agricultural productivity. The core is where capital is always concentrated in its most sophisticated forms. Banks, professions, trade activity and skilled manufacturing were all sufficiently widespread to sustain a wage-labor economy.

The periphery, in contrast, refers to regions lacking strong central governments, dependent on coercive rather than wage labor, and the economies of which depend on the export of raw materials to the core. Latin America and Eastern Europe were key peripheral zones in the sixteenth century. In Latin America, the Spanish and the Portuguese conquests destroyed indigenous political leaders and replaced them with weak bureaucracies under European control. Native populations were killed or enslaved, African slaves were imported to work the land and the mines, and the local aristocracy was complicit with a system that kept it in power while it presided over the production of goods primarily for consumption in Europe. In the periphery, extensive cultivation and coercive control of labor achieve low-cost agricultural production.

Wallerstein also refers to semi-peripheries as well as external areas. Semi-peripheries were either regions that could be located geographically in the core but were undergoing a process of relative decline (Spain and Portugal) or rising economies in the periphery. They were exploited by the core, but in turn took advantage of the periphery. Unlike some of the dependency thinkers who posited a polar relationship between two basic categories, Wallerstein argues that the semi-periphery is a crucial buffer between core and periphery:

> The semi-periphery is assigned a specific economic role, but the reason is less economic than political . . . one might make a good case that the world-economy . . . would function every bit as well without a semi-periphery. But it would be far less politically stable, for it would mean a polarized world-system. The existence of the third category means precisely that the upper stratum is not faced with the unified opposition of all the others because the middle stratum is both exploited and exploiter.

Much of Wallerstein's work traces the geographical expansion of the world-system over time. Two stages in particular mark its development from the sixteenth to the late twentieth century. Up to the eighteenth century, the system was characterized by a strengthening of European states, following the failure of the Hapsburg Empire to convert the emerging world-economy into a world-empire. Increasing trade with the Americas and Asia enriched small merchant elites at the expense of wage laborers in Europe, while its monarchs expanded their power to collect taxes, borrow money and expand militias to support the absolute monarchies. In the eighteenth century, industrialization replaced the emphasis on agricultural production, and European states embarked on an aggressive search for new markets to exploit. Over the past 200 years, new regions have been absorbed into the system, such as Asia and Africa, increasing the available surplus. However, it was not until the early years of the twentieth century that the world-system became truly global.

Wallerstein also traces the rise and decline of core hegemons (or dominant powers) in the world-system over time. In 1984, he described "three instances" of hegemony; "the United Provinces in the mid-seventeenth century, the United Kingdom in the mid-nineteenth, and the United States in the mid-twentieth". In his more recent work he has speculated on the future of the world-system in light of the decline of the United States in the world-economy and the end of the Cold War. He suggests that the collapse of the Soviet Union and its peripheral status is not good news for the dominant forces of the capitalist world-system, because it removes the last major politically stabilizing force that helped to legitimate the hegemony of the United States. In *Geopolitics and Geoculture* (1991), he suggests that the period of U.S. hegemony may be over now that Japanese and Western European enterprises are genuinely competitive with American companies, but in the absence of the "Soviet threat", it is unclear whether conflicts between states in the core can be diluted by appealing to any common ideological interest in sustaining co-operation.

For Wallerstein, the capitalist world-system – while it may continue to function as it has for the past 500 years in search of the endless accumulation of capital and goods – is characterized by some fundamental internal contradictions, which will ultimately bring about its demise even as it appears to consolidate its global control. First, there is continuing imbalance between supply and demand. So long as decisions about what and how much to produce are made at the level of the firm, the imbalance will be an unintended consequence of continuous mechanization and commodification. Second, whereas in the short run it is rational for capitalists to make profits by withdrawing the surplus from immediate consumption, in the longer term the further production of surplus requires a mass demand that can be met only by redistributing the surplus. Third, there are limits to the degree to which the state can co-opt workers to maintain the legitimation of the capitalist system. Finally, and most significantly, there is the contradiction between the one and the many, the coexistence of a plural states system within one world-system.

While this facilitates the expansion of the system, it also impedes any attempt to develop greater co-operation to counter systemic crises in the system as a whole.

Wallerstein's approach is characterized by two fundamental epistemological commitments. He is fundamentally opposed to the idea that one can study processes of economic "development" within states without situating them in a much broader spatial and historical context. To study the state as if it were the unit within which problems are both generated and solved is to accept uncritically the dominant liberal ideology of progress. According to this ideology, the way out of economic underdevelopment for poor states is to adopt the political, economic and cultural characteristics of "developed" states: if governments adopt "free market" policies, and promote private enterprise and an entrepreneurial culture, then there is no intrinsic barrier to modernization. Equally, Wallerstein takes issue with those on the left who believe that underdevelopment is promoted by core states in their ability to extract economic surplus from periphery states. Insofar as this implies that Third World states should somehow withdraw from the capitalist world-economy, Wallerstein argues that in a single world-system, peripheral states cannot develop along lines different from those imposed by the core.

Wallerstein is also extremely critical of Western social science, which treats politics, economics, history and sociology as separate "disciplines" in the social sciences. He certainly would not recognize the study of international relations as an autonomous discipline, and his approach is therefore radically at odds with the realist view that its autonomy arises from the special character of relations among states in an anarchical environment. This is only one aspect of the structure of the world-system, and a subordinate one at that. Since the late eighteenth century, the modern era has been dominated by the idea of progress and by the political myth that sovereignty is legitimate, as the power of states is said to derive from "the people". For Wallerstein, the modern ideologies of conservatism, liberalism and socialism are best understood as political programs to manage the social turmoil that constant economic change engenders. At the end of the twentieth century, many people believe that liberalism is now dominant. The threefold political program of universal suffrage, the welfare state and the creation of national identity effectively secured the legitimation of the world-system in Europe, and provides a model for universal aspiration outside it. Most social scientists espouse a liberal ideology, for the whole enterprise of social science is founded on the premise of social progress based on the ability to manipulate social relations, provided this can be done in a "scientific" manner.

Wallerstein's work has – as one might expect, given its radical challenges to orthodox social science – been the subject of intense debate. Traditional Marxists have complained that he misunderstands the nature of capitalism, focusing too much on the logic of exchange in the market rather than on modes of production. Ernesto Laclau, for example, claims that "the fundamental economic relationship of capitalism is constituted by the free laborer's sale of his labor power, whose necessary precondition is the loss by the direct producer of ownership of the means of production". If wage labor is the defining characteristic of capitalism, then Wallerstein's whole model is cast in doubt, as other forms of labor have been dominant in other parts of the world, making it difficult to define them as capitalist.

Indeed, Wallerstein's views have been attacked from across the ideological spectrum. Socialists who believe that radical reform is still possible within the boundaries of the state, or between socialist states, have not taken kindly to the idea that socialism is possible only at a global level. Wallerstein takes the Trotskyist position of dismissing "socialism in one country", defining communist states as merely collective capitalist firms whose very participation in the world-system prevents the transition to socialism at a global level. More orthodox scholars have attacked the extreme structural-functionalism of Wallerstein's theoretical approach. Realists, for example, would argue that if the competitive interstate system is itself derived from the economic logic of the capitalist world-system, how does one account for competitive behavior among political units before the sixteenth century? They argue that there is a distinctly political logic involving the struggle for power among sovereigns, which cannot be reduced to capitalism. As Kal Holsti has pointed out, "to say that war between capitalist states is inevitable is like saying that collisions between Ford automobiles are inevitable; but which is the critical variable? Automobile or Ford? State or economy?"

It might also be argued that the rigidity of the core/semi-periphery/periphery mode fails to account for anomalies such as the rise of some states to "core" status (Japan?) because it presupposes a zero-sum relationship among states in the system. The structure of the system remains constant for Wallerstein, so that if some states appear to rise and move from one category to another, others must fall. Given the generality of the theoretical approach, as well as its historical depth, it is sometimes difficult to place some states within any of the categories. For example, on the basis of its GNP per capita and standard of living, Australia can be categorized as part of the core, even though Wallerstein places it in the semi-periphery.

Finally, one might note a tension between the empirical claims of Wallerstein (which should therefore be amenable to hypothesis-testing) and his contempt for conventional methodologies of theory construction in the social sciences. Is it possible to make deterministic claims about the primacy of global economic forces and defend those claims not on criteria of empirical validity, "but on their heuristic value; i.e. whether they make sense to the people and organizations who are seeking to act in world-historical contexts and need to understand the dynamics of change"? Of course, this is a tension that characterizes a great deal of radical thought that defends the need for change on the basis not of moral criteria but of empirical claims regarding the inherent inequality of the capitalist system.

The Development of a World Economic System

In *Modern History Sourcebook*: A summary of *The Modern World-System: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century* (1974)

In his book, *The Modern World-System*, Immanuel Wallerstein develops a theoretical framework to understand the historical changes involved in the rise of the modern world. The modern world-system, essentially capitalist in nature, followed the crisis of the feudal system and helps explain the rise of western Europe to world supremacy between 1450 and 1670. According to Wallerstein, his theory makes possible a comprehensive understanding of the external and internal manifestations of the modernization process during this period and makes possible analytically sound comparisons between different parts of the world.

MEDIEVAL PRELUDE

Before the sixteenth century, when Western Europe embarked on a path of capitalist development, "feudalism" dominated Western European society. Between 1150 and 1300 both population as well as commerce expanded within the confines of the feudal system. However, from 1300 to 1450 this expansion ceased, creating a severe economic crisis. According to Wallerstein, the feudal crisis was probably precipitated by the interaction of the following factors:

- Agricultural production fell or remained stagnant, so the burden of peasant producers increased as the ruling class expanded.
- The economic cycle of the feudal economy had reached its optimum level; afterwards the economy began to shrink.
- A shift of climatological conditions decreased agricultural productivity and contributed to an increase in epidemics.

THE NEW EUROPEAN DIVISION OF LABOR

Wallerstein argues that Europe moved towards the establishment of a capitalist world economy in order to ensure continued economic growth. This entailed the development of different modes of labor control and the creation of relatively strong state machineries in the states of Western Europe. In response to the feudal crisis, by the late fifteenth and early sixteenth centuries, the world economic system emerged. This was the first time that an economic system encompassed much of the world with links that superseded national or other political boundaries. The new world-economy differed from earlier empire systems because it was not a single political unit. Empires depended upon commercial monopolies combined with extensive bureaucracy and a standing army to direct the flow of economic goods from the periphery to the center. Only the techniques of modern capitalism enabled the modern world economy to extend beyond the political boundaries of any one empire.

The new capitalist world-system was based on an international division of labor that determined relationships between different regions as well as the types of labor conditions within each region. In this model, the type of political system was also directly related to each region's placement within the world economy. As a basis for comparison, Wallerstein proposes four different categories: core, semi-periphery, periphery, and external, into which all states of the world can be placed. The categories describe each region's relative position within the world economy as well as certain internal political and economic characteristics.

CORE

The core regions benefited the most from the capitalist world economy. For the period under discussion, much of northwestern Europe (England, France, Holland) developed as the first core region. Politically, the states within this part of Europe developed strong central governments, extensive bureaucracies, and large mercenary armies. This permitted the local bourgeoisie to obtain control over international commerce and extract capital surpluses from this trade for their own benefit. As the rural population expanded, the small but increasing number of landless wage earners provided labor for farms and manufacturing activities. The switch from feudal obligations to money rents in the aftermath of the feudal crisis encouraged the rise of independent or yeoman farmers but squeezed out many other peasants off the land. These impoverished peasants often moved to the cities, providing cheap labor essential for the growth in urban manufacturing. Agricultural productivity increased with the growing predominance of the commercially oriented independent farmer and improved farm technology.

PERIPHERY

On the other end of the scale lay the peripheral zones. These areas lacked strong central governments or were controlled by other states, exported raw materials to the core, and relied on coercive labor practices. The core expropriated much of the capital surplus generated by the periphery through unequal trade relations. Two areas, Eastern Europe (especially Poland) and Latin America, exhibited characteristics of peripheral regions. In Poland, kings lost power to the nobility as the region became a prime exporter of wheat to the rest of Europe. To gain sufficient cheap and easily controlled labor, landlords forced rural workers into a "second serfdom" on their commercial estates. In Latin America, the Spanish and Portuguese conquests destroyed indigenous authority structures and replaced them with weak bureaucracies under the control of these European states. Powerful local landlords of Hispanic origin became aristocratic capitalist farmers. Enslavement of the native populations, the importation of African slaves, and the coercive labor practices such as the *encomienda* and forced mine labor made possible the export of cheap raw materials to Europe. Labor systems in both peripheral areas differed from earlier forms in medieval Europe in that they were established to produce goods for a capitalist world economy and not merely for internal consumption. Furthermore, the aristocracy both in Eastern Europe and Latin America grew wealthy from their relationship with the world economy and could draw on the strength of a central core region to maintain control.

SEMI-PERIPHERY

Between the two extremes lie the semi-peripheries. These areas represented either core regions in decline or peripheries attempting to improve their relative position in the world economic system. They often also served as buffers between the core and the peripheries. As such, semi-peripheries exhibited tensions between the central government and a strong local landed class. Good examples of declining cores that became semi-peripheries during the period under study are Portugal and Spain. Other semi-peripheries at this time were Italy, southern Germany, and southern France. Economically, these regions retained limited but declining access to international banking and the production of high-cost, high-quality manufactured goods. Unlike the core, however, they failed to predominate in international trade and thus did not benefit to the same extent as the core. According to Wallerstein, the semi-peripheries were exploited by the core but, as in the case of the American empires of Spain and Portugal, often were exploiters of peripheries themselves. Spain, for example, imported silver and gold from its American colonies, obtained largely through coercive labor practices, but most of this went to paying for manufactured goods from core countries such as England and France rather than encouraging the formation of a domestic manufacturing sector.

External Areas

These areas maintained their own economic systems and, for the most part, managed to remain outside the modern world economy. Russia fits this case well. Unlike Poland, Russia's wheat served primarily to supply its internal market. It traded with Asia as well as Europe, but internal commerce remained more important than trade with outside regions. Also, the considerable power of the Russian state helped regulate the economy and limited foreign commercial influence.

STAGES OF GROWTH

The development of the modern world economy lasted centuries, during which time different regions changed their relative position within this system. Wallerstein divides the history of the capitalist world-system into four stages (which for our purposes can be simplified and divided into two basic phases):

Stages 1 and 2:
This period follows the rise of the modern world-system between 1450 and 1670. When the Hapsburg Empire failed to convert the emerging world economy to a world empire, all the existing western European states attempted to strengthen their respective positions within the new world-system. In order to accomplish this move, most of the states consolidated their internal political, economic and social resources by:

a) Bureaucratization. This process aided the limited but growing power of the king. By increasing the state power to collect taxes, the kings eventually increased state power to borrow money and thereby further expand the state bureaucracy. At the end of this stage, the monarch had become the supreme power and instituted what has been called "absolute monarchy."

b) Homogenization of the local population. To underline state involvement in the new capitalist system and encourage the rise of indigenous capitalist groups, many core states expelled minorities. These independent capitalist groups, without deep rooted local ties, were perceived as threats to the development of strong core states. The Jews in England, Spain and France were all expelled with the rise of absolute monarchy. Similarly, Protestants, who were often the merchants in Catholic countries, found they were targets of the Catholic Church. The Catholic Church, a transnational institution, found the development of capitalism and the strengthening of the state threatening.

c) Expansion of the militia to support the centralized monarchy and to protect the new state from invasions.

d) The concept of absolutism introduced at this time related to the relative independence of the monarch from previously established laws. This distinction freed the king from prior feudal laws.

e) Diversification of economic activities to maximize profits and strengthen the position of the local bourgeoisie.

By 1640, northwestern European states secured their position as core states in the emerging economy. Spain and northern Italy declined to semi-peripheral status, while northeastern Europe and Iberian America became peripheral zones. England gained ground steadily toward core status. During this period, workers in Europe experienced a dramatic fall in wages. This wage fall characterized most European centers of capitalism with the exception of cities in north and central Italy and Flanders. The reason for this exception was that these cities were relatively older centers of trade, and the workers formed strong politico-economic groups; their resistance prevented employers from accumulating the large surplus necessary for the advancement of capitalism. Meanwhile, employers in other parts of Europe profited from the wage lag by accumulating large surpluses for investment.

Long-distance trade with the Americas and the East provided enormous profits, in excess of 200%-300%, for a small merchant elite. Smaller merchants could not hope to enter this profiteering without substantial capital and some state help. Eventually, the profits of the trans-Atlantic trade filtered down and strengthened the merchants' hold over European agriculture and industries. Merchants with sufficient power accumulated profits through the purchase of goods prior to their production. By controlling the costs of finished products, merchants could extend their profit margin and control the internal markets. This powerful merchant class provided the capital necessary for the industrialization of European core states.

Stages 3 and 4 (18th century and beyond):
Industrial rather than agricultural capitalism represented this era. With the shifting emphasis on industrial production, the following reactions characterized this period:

a) European states participated in active exploration for the exploitation of new markets.

b) Competitive world-systems such as the Indian Ocean system were absorbed into the expanding European world-system. With the independence from Spain of the Latin American countries, these areas as well as previously isolated zones in the interior of the American continent entered as peripheral zones in the world economy. Asia and Africa entered the system in the nineteenth century as peripheral zones.

c) The inclusion of Africa and the Asian continents as peripheral zones increased the available surplus, allowing other areas such as the U.S. and Germany to enhance their core status.

d) During this phase, the core regions shifted from a combination of agricultural and industrial interests to purely industrial concerns. In 1700, England was Europe's leading industrial producer as well as the leader in agricultural production. By 1900, only 10% of England's population was engaged in agriculture.

e) By the 1900s, with the shift toward manufacturing, core areas encouraged the rise of industries in peripheral and semi-peripheral zones so that they could sell machines to these regions.

THEORETICAL REPRISE

The capitalist world economy, as envisioned by Wallerstein, is a dynamic system that changes over time. However, certain basic features remain in place. Perhaps most important is that when one examines the dynamics of this system, the core regions of northwestern Europe clearly benefited the most from this arrangement. Through extremely high profits gained from international trade and from an exchange of manufactured goods for raw materials from the periphery (and, to a lesser extent, from the semi-peripheries), the core enriched itself at the expense of the peripheral economies. This, of course, did not mean either that everybody in the periphery became poorer or that all citizens of the core regions became wealthier as a result. In the periphery, landlords for example often gained great wealth at the expense of their underpaid coerced laborers, since landowners were able to expropriate most of the surplus of their workers for themselves. In turn in the core regions, many of the rural inhabitants, increasingly landless and forced to work as wage laborers, at least initially saw a relative decline in their standard of living and in the security of their income. Overall, certainly, Wallerstein sees the development of the capitalist world economy as detrimental to a large proportion of the world's population.

Wallerstein's World Capitalist System: A Theoretical and Historical Critique

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Immanuel Wallerstein's *The Modern World-System* aims to achieve a clean conceptual break with theories of "modernization" and thus provide a new theoretical paradigm to guide our investigations of the emergence and development of capitalism, industrialism and national states. This splendid undertaking could hardly be more appropriately timed and aimed. For quite some time, modernization approaches have been subjected to telling critical attacks. They have been called to task for reifying the nation-state as the sole unit of analysis, for assuming that all countries can potentially follow a single path (or parallel and converging paths) of evolutionary development from "tradition" to "modernity," and, concomitantly, for disregarding the world-historical development of transnational structures that constrain and prompt national or local developments along diverse as well as parallel paths. Moreover, modernization theorists have been criticized for the method of explanation they frequently employ: ahistorical ideal types of "tradition" versus "modernity" are elaborated and then applied to national cases; if the evidence seems to fit, one assumes that a particular historical instance is

adequately explained; if not, one looks for the "chance" factors that account for its deviation.
In the opening pages of *The Modern World-System*, Wallerstein unequivocally defines his approach in direct opposition to these features of modernization theory. Thus in his book he concentrates on explaining the structure and functioning of capitalism as a world economic system, viewing sovereign states as but "one kind of organizational structure among others within this single social system". Equally important, he intends to avoid the "intellectual dead-end of ahistorical model-building" by grounding his theorizing in an analysis of the historically specific emergence and development of capitalism since the sixteenth century. He hopes thereby to demonstrate "that to be historically specific is not to fail to be analytically universal," that "the only road to nomothetic propositions is through the historically concrete".

Given these very appealing and appropriate intentions of Wallerstein's theoretical program, not to mention the impressive scope of his reading in the works of historians, it is hardly surprising that *The Modern World-System* has met with an uncritically laudatory response from many sociologists, but this assessment is too hasty. As I shall attempt to show, Wallerstein's arguments are too misleading theoretically and historically to be accepted at face value. Like many other important pioneering works, Wallerstein's *Modern World-System* overreaches itself and falls short of its aims. It is therefore incumbent especially upon those of us who are sympathetic to its aims to subject this work to rigorous critical scrutiny. For the true contribution of *The Modern World-System* will lie in the theoretical controversies and advances it can spark.

I

Despite his avowed desire to avoid "abstract model building," Wallerstein in fact deals with historical evidence primarily in terms of a preconceived model of the capitalist world-economy. I shall, therefore, start by describing and discussing this model. Wallerstein insists that any theory of social change must refer to a "largely self-contained" entity whose developmental dynamics are "largely internal". There have been, he says, only two kinds of large-scale social systems: (1) empires, in which a functional economic division of labor (occupationally, not geographically, based) is subsumed under an overarching, tribute-collecting imperial state, and (2) world-economies, in which there are multiple political sovereignties, no one of which can control the entire economic system. A world-economy should be, in Wallerstein's view, more able than a world-empire to experience sustained economic development precisely because economic actors have more freedom to maneuver and to appropriate and reinvest surpluses.

Such a world-economy – of which capitalism from the sixteenth century to the present has been (according to Wallerstein) the only long-lasting historical instance – is based upon a geographically differentiated division of labor (core, semi-periphery, and periphery) tied together by world-market trade in bulk commodities. Each major zone of the world-economy has an economic structure based upon its particular mixture of economic activities (e.g., industry plus differentiated agriculture in the core; mono-culture in the periphery) and its characteristic form of "labor control" (e.g., skilled wage labor and tenantry in the core; sharecropping in the semi-periphery; and slavery or "coerced cash-crop labor" in the periphery). The different zones are differentially rewarded by the world-economy, with surplus flowing disproportionately to the core areas. Moreover, the economic structure of each zone supports a given sort of dominant class oriented toward the world-market, as well as states of a certain strength (strongest in the core and weakest in the periphery) that operate in the interests of that class. Finally, according to Wallerstein, the differential strength of the multiple states within the world capitalist economy is crucial for maintaining the system as a whole, for the strong states reinforce and increase the differential flow of surplus to the core zone. This happens because strong states can provide "extra-economic" assistance to allow their capitalist classes to manipulate and enforce terms of trade in their favor on the world-market.

Let us reflect for a moment upon this model as a whole. Historically, one of the most striking things about capitalism has been its inherent dynamism. From a world-historical perspective, we need to understand how and why capitalism emerged and has developed. Wallerstein clearly appreciates the importance of these issues, yet he does not offer very many insights about them. For one thing, Wallerstein's theory does not put him in a good position to explain the transition from feudalism to capitalism in Europe. The most obvious difficulty is the lack of any theoretical conception of the dynamics of feudalism, which is neither a "world-empire" nor a "world-economy" in Wallerstein's terms. To explain what he holds to be the demise of feudalism around 1450, Wallerstein employs, first, an amalgam of historians' arguments about reasons for the crisis of feudalism (1300-1450) and, then, a series of teleological arguments about how the crisis "had to be solved" if "Europe" or "the system" were to survive. The emergence of the capitalist world system is presented as the solution. Thus in this one instance where Wallerstein actually discusses a supposed transition from one mode of production to another, he uses the language of system survival, even though such language is quite incongruous.

As for how world capitalism develops once it is established, although Wallerstein does assert repeatedly that the system is dynamic, he provides us with no theoretical explanation of why developmental breakthroughs occur. The only definite dynamics of Wallerstein's world capitalist system are market processes: commercial growth, worldwide recessions, and the spread of trade in necessities to new regions of the globe. Apparently the final demise of the system will come after the market has spread to cover the entire globe and transform all workers into wage laborers. But even the all-important dynamic of global expansion itself depends upon the occurrence of technological innovations – themselves unexplained.

In sharp contrast to his awkwardness and sketchiness in explaining dynamics, Wallerstein is very forceful on the subject of the stability of the world capitalist system. In theory, as we have seen, once the system is established, everything reinforces everything else. And Wallerstein consistently employs not only system-maintenance arguments but also direct analogies between the structure of the world capitalist system and the typical structure of political empires to convey a sense of the massive stability of the whole. For he believes that his model points to the essential structures of world capitalism – to patterns of division of labor and of relationships among states in different economic positions that have endured since the sixteenth century even though the system as a whole has expanded geographically and particular countries have changed positions within the system.

II

Taking our cue from his emphases, then, let us take a close critical look at the ideas about determinants of socioeconomic and political structures that are built into Wallerstein's model of the world capitalist system. We can most readily pinpoint the problematic points, I suggest, if we see that the model is based on a two-step reduction: first, a reduction of socio-economic structure to determination by world-market opportunities and technological production possibilities; and second, a reduction of state structures and policies to determination by dominant class interests.

The ways in which Wallerstein tries to make sense of the differences of economic structure among his three major zones of core, semi-periphery, and periphery lead him to make the first reduction. The crux of the differences is the mode of labor control "adopted" in each zone by the dominant classes oriented to the world-market. In his theoretical passages addressed to this issue, Wallerstein repeatedly implies that the dominant classes choose freely among alternative strategies of labor control by assessing rationally the best means for maximizing profits, given the geographical, demographic, technological and labor-skill conditions in which they find themselves, and given the profitable possibilities they face for selling particular kinds of products on the world-market. Now the curious thing here is that, despite the fact that Wallerstein seems to be placing a great deal of stress on the class structures of the major zones of world capitalism, actually (as far as I can see) he is explaining the fundamental economic dynamics of the system in terms of exactly the variables usually stressed by liberal economists, while ignoring the basic Marxist insight that the social relations of production and surplus appropriation are the sociological key to the functioning and development of any economic system. For this Marxist idea demands that one pay attention to institutionalized *relationships* between producing and surplus-appropriating classes and allow for the ever-present potential of collective resistance from below. Instead, Wallerstein treats "labor control" primarily as a market-optimizing strategy of the dominant class alone.

One major theoretical effect of his reliance on liberal economics is a nonexploitative picture of the process of income distribution within the world system. To be sure, he argues that the forces of the marketplace tend to maintain established differences of "occupational" structure among regions. But notice the reason offered: "a capitalist world-economy essentially rewards accumulated capital, including human capital, at a higher rate than 'raw' labor power". Would a liberal economist say anything different, since all that is being argued here is that regions with the scarcer factors of production are differentially rewarded by the market? Yet, of course, Wallerstein does argue theoretically that the structure and functioning of the world capitalist economy are inherently exploitative. He does so by assigning the international hierarchy of dominating and dominated states (especially core vs. periphery) a crucial mediating role in exacerbating and sustaining overall inequalities in the system as a whole. Thus he writes, "Once we get a difference in the strength of the state-machineries, we get the operation of 'unequal exchange' which is enforced by strong states on weak ones, by core states on peripheral areas. Thus capitalism involves not only appropriation of surplus-value by an owner from a laborer, but an appropriation of surplus of the whole world-economy by core areas".

But, then, how are degrees of state strength to be explained? Here we arrive at the second reduction built into Wallerstein's model. For in his theory, differences of state strength in different zones of the world system are explained as the result of differences in regional rates of surplus appropriation and, above all, as the expressions of the world-market interests of the dominant classes within each major zone. Thus the core area ends up with strong states primarily because there are more surpluses to tax and because the dominant capitalist classes want state protection for industry and their control of international trade; on the other hand, the periphery ends up with weak states because it reaps less from world trade and because its dominant capitalist classes are interested in profiting from direct dealings with merchants from the core areas. In short, to explain differences in state strength, Wallerstein relies upon arguments about economic conditions and world-market interests, largely ignoring other important variables such as historically preexisting institutional patterns, threats of rebellion from below, and geopolitical pressures and constraints.

Given that the economic structure and functioning of the world system have already been explained in market-technological rather than class terms, Wallerstein must make this second reduction, of politics to world-market-oriented class interest, in order to be able to assert that the system will be exploitative, and stably so over the long run. For as he points out, if states were equally strong across the major regions, "they would be in the position of blocking the effective operation of transnational economic entities. . . . It would then follow that the world division of labor would be impeded, the world-economy decline, and eventually the world-system fall apart". Without a hierarchy of dominating and dominated states corresponding to the existing pattern of economic differentiation, there is no worldwide "unequal exchange" in this theory. Ironically, then, Wallerstein has managed to create a model that simultaneously gives a decisive role to international political domination (curiously enough for a theory that set out to deemphasize the nation-state!) and deprives politics of any independent efficacy, reducing it to the vulgar expression of market-class interests.

Certainly some quite implausible assumptions have to be made to make the model internally consistent. Since everything is directly or indirectly an expression of capitalist class interests, we are forced to assume that these classes always get what they want, reshaping institutions and their relations to producing classes to suit their current world-market opportunities. At the same time, we must assume that, although all of the variously situated dominant capitalist classes want and are able to maximize their world-market trading advantages, nevertheless *only* the core-area capitalists want, need and get the extra-economic assistance of strong states, while peripheral capitalists do not. Still, the implausibilities would not matter very much if the model itself were genuinely useful for analyzing and explaining actual historical developments. But I believe that each of the two reductions in Wallerstein's model deprives him of crucial explanatory resources for understanding the patterns of history. Let me argue my case by examining in turn two major early modern European developments that Wallerstein himself stresses: (1) the resolution of the "crises of feudalism"; and (2) the emergence of monarchical absolutisms.

III

One of the most striking developments in Europe during the "long sixteenth century" (1450-1640) was the divergence of economic patterns between northwestern Europe and eastern Europe (including Poland, Hungary, Livonia and Germany east of the Elbe River). While in the west serfdom was virtually gone by 1600, and thereafter the commercialization of the social relations of agrarian production and the growth of industries were important trends, in the east the peasants had by 1600 become tied to the land so that labor and dues could be forcibly extracted from them by the landlords, and this so-called "second serfdom" was accompanied by the decline of towns and indigenous industries under bourgeois control. Moreover, during the same general period east and west became more intensively linked through the Baltic trade, in which primary bulk goods, including especially grain, were exported from the east, which in turn imported manufactured goods, primarily from England and the Netherlands. Clearly this pattern corresponds very nicely to Wallerstein's model of relations between core and periphery in the emergent capitalist world-economy. This in itself is not really surprising, though, since Wallerstein's model, as he fully acknowledges, was originally inspired in part by the work of the historian Marian Malowist, who stresses the importance of the Baltic trade as a contributing cause of the eastern versus western divergence. Yet what was for the historian one contributing cause becomes for Wallerstein, given the dictates of his world capitalist system model, *the* theoretically significant explanation: "The reason why these opposite reactions . . . occurred was because . . . the two areas became complementary parts of a more complex single system, the European world-economy, in which eastern Europe played the role of raw-materials producer for the industrializing west."

To be convincing, Wallerstein's explanation should be validated (or at any rate not invalidated) by the sequence of events; that is, if world trade opportunities really were the decisive cause of the "second serfdom," their availability should precede, or at least fully coincide with, the trends toward enserfment. But actually the process of enserfment was under way in virtually all areas by 1400 and by "the end of the fifteenth century [i.e., 1500] . . . from the Elbe to the Volga, most of the peasantry were well on the way to becoming serfs" (Blum, 1957), whereas eastern grain exports to the west began expanding significantly around 1500 and achieved their most sudden and sizable growth only between 1550 and 1600, *after* the foundations of the coerced labor system were fully established.

Second, and more important, markets cannot solely or primarily explain social-structural transformations or economic developments because, depending upon the preexisting institutional patterns of class relations, different classes may be in the best position to take advantage of available trade opportunities and thereby have their particular positions strengthened. Thus Robert Brenner points out that in parts of northwestern Germany in the sixteenth century peasants (rather than enserfing lords) took advantage of the new export opportunities – "and they appear to have done so after a prolonged period of anti-landlord resistance." As for the eastern lords, Brenner concludes, "No doubt, in this instance, the income from grain produced by serf-based agriculture and sold by export . . . enhanced the class power of the Eastern lords, helping them to sustain their seigneurial offensive. But the control of grain production (and thus the grain trade) secured through their successful enserfment of the peasantry was by no means assured by the mere fact of the emergence of the grain markets themselves." Rather, as even Malowist says, "trade developed in a form determined by locally prevalent social and economic circumstances and affected these in turn."

Brenner's carefully crafted comparative historical investigations suggest that to explain the divergences of socioeconomic developments in east versus west we must attend especially to the "*historically specific* patterns of development of the contending agrarian classes and their relative strength in the different European societies: their relative levels of internal solidarity, their self-consciousness and organization, and their general political resources". Thus Brenner makes a case that eastern peasants were more easily and thoroughly dominated by their landlords because – for various specific historical reasons – they enjoyed much less village community solidarity and local political autonomy than did western European peasants. Consequently, when the eastern lords attempted to impose coercive controls, initially under conditions of economic crisis and labor scarcity, they succeeded, whereas lords in the west had failed in the same attempt under similar conditions during the 1300s.

IV

If Wallerstein's world-market theory prevents him from adequately explaining patterns of economic development in early modern Europe, it leaves him even less able to make sense of the patterns of state development. This was, of course, the era of the initial emergence of absolute monarchies – kingly governments that tried, with varying degrees of success, to impose protobureaucratic administrative controls and coercive monopolies over large populations and territories. Wallerstein recounts the phenomena of absolutism and tries to subsume them within his theory by invoking the category of the "strong state." According to the theory, let us recall, strong states necessarily grow up in the core zone of the world capitalist economy. Thus Wallerstein asserts, "In the sixteenth century, some monarchs achieved great strength. . . . Others failed. This is closely related . . . to the role of the area in the division of labor within the world-economy. The different roles led to different class structures which led to different politics". However, Wallerstein's attempt to equate the strong core state and absolute monarchy does not work. The historical evidence simply does not fit the overall pattern implied by the theory, for there were more and stronger absolutisms outside the core than in it.

Economically speaking, both the Netherlands and England were, according to Wallerstein's analysis, core countries. Were they also strong states? The "strong state" is defined theoretically by Wallerstein as strong "vis-à-vis other states within the world-economy including other core states, and strong vis-à-vis local political units within the boundaries of the state . . . also . . . strong vis-à-vis any particular social group within the state." Since the Dutch government was simply a federation of merchant oligarchies, Wallerstein does not even try to convince us that the Netherlands really was a strong state; instead he stresses the economic interdependence of England and the Netherlands and the transitional functions of Dutch economic primacy for the emerging world capitalist system. But he clearly wants us to believe that the English Tudor state was a strong core state – even though the English monarchs had no large standing armies and no bureaucratic administration that penetrated the localities. In fact, the English monarchs could rule only through cooperation with locally powerful notables, the county-Parliamentary gentry and the London merchant oligarchy.

What about the true absolute monarchies of Europe, such as the Spanish, the French, and the Swedish? Wallerstein stresses the bureaucratic weight and military aggressiveness of the Spanish state whenever he is trying to account for European domination of the New World and when he discusses the Hapsburg attempt at empire building within Europe. Then, suddenly, Spain drops out of the picture, even though her monarchy remained thoroughly absolutist and, arguably, just as internationally powerful as the English government throughout the entire historical period under consideration. As for France, the theoretically induced dilemma that Wallerstein faces is how to explain why this country, situated only partly in the core zone, partly in the semi-periphery, actually developed a much stronger state than did either England or the Netherlands. To cope with this dilemma, an alternative ad hoc (and teleological) explanation of state strength is introduced: France "had to" develop a centralized, bureaucratic monarchy in order to hold together her differently oriented capitalist classes. Similarly, when another, even more blatantly deviant case comes up – that of Sweden, with probably the most powerful and dynamic absolutism of the era – Wallerstein introduces still another ad hoc explanation:

> The position of Sweden is worth brief attention, as the evolution of Sweden's state machinery approached the model

> of western Europe rather than that of the periphery, although it was economically very underdeveloped at this time. It was strong, not because its commerce and industry was strong . . . it was paradoxically rather that its agriculture was weak, and its aristocrats wished to take hold of the profits of other lands for want of being able to create them on their own. . . . The nobility hence needed conquest and for that they needed a strong, not a weak, state.

But with this final explanatory maneuver, Wallerstein thoroughly contradicts his original assertion that the strongest absolutisms should emerge in the core and certainly not in the periphery. For Sweden demonstrates (as does Prussia, after 1650) that a very strong state can be built on a peripheral agrarian base, and that, once built, it can reshape the economic future of the area in question.

Clearly, neither the differential appearance of absolutist states in early modern Europe nor their effects upon economic development are adequately accounted for by Wallerstein's world capitalist system theory. Better treatments of patterns of state development are to be found in Benedict Anderson and Charles Tilly. These works suggest that, although no simple or monocausal explanation of state building is possible, two main sets of variables can go a long way toward accounting for the variations. First, internal class structures were important, not because economically dominant classes got automatically what they wanted, but because different patterns of class relationships and alliances – including relationships and alliances involving agrarian feudal classes – created different possibilities for monarchs to extract resources and encouraged them to use available resources in different ways.

Second, an equally, if not more important, transnational structure than the trade and economic interdependence to which Wallerstein points was that constituted by the system of politico-military interactions among emerging European states. This "European states system" set up constraints and opportunities, varying according to the specific geopolitical situation of each country that helped determine the kinds of state policies that developed (or did not develop) in various times and places. In early modern Europe, incessant military competition among monarchies was an important spur to strong state building, for the main use of enlarged royal tax or loan revenues was the building up of standing armies. Not surprisingly, those monarchies that found it possible to extract the resources to build the largest land armies were also the ones that developed the strongest and most bureaucratic administrative machineries.

But these were not the countries that found themselves during this period at the center of the nascent capitalist commercial economy. The Netherlands, as Wallerstein himself points out, was a small country whose survival depended upon military balances among her powerful neighbors. And England could remain somewhat aloof from the continental military system because of her island situation. Because of their prior political histories and relatively sheltered geopolitical circumstances, England and the Netherlands happened during this period to have governments uniquely responsive to commercial-capitalist interests. These were not bureaucratic governments (even by the standards of the time) and for that very reason they were not so strong as to be able to stifle commercial development or protect the lower classes (as the French monarchy did its peasantry) against encroachments by capitalist landlords or bourgeoisies. Indeed, it was probably one necessary condition for continuing capitalist development in early modern Europe that England's would-be absolutisms did not consolidate themselves. Because they did not, agrarian commercialization – which must itself be explained by reference to developments over time of class structure and conflict – could proceed unhindered, and eventually facilitate the Industrial Revolution. Then, once capitalist relations of production and accumulation were firmly established in England, the dynamics of the European states system ensured that capitalist relations would spread over the entire globe as much through military conquests as through market expansion.

If the strongest states are not always in the core and if, in fact, equally strong or stronger states can grow up in the periphery (not to mention the semi-periphery), then according to Wallerstein's own logic the economic division of labor cannot be presumed likely to hold together over time as a "system" and the differential flow of surpluses to the core is likely to be disrupted. Empirically speaking, these disruptive possibilities seem especially likely in later stages of world capitalist development, when strong, noncore states, perhaps created through revolutions from above or below, may be able to initiate rapid industrialization or other programs of economic development.

V

Finally, aside from this substantive critique of Wallerstein's approach, two methodological criticisms need to be made. The first has to do with the way Wallerstein handles historical evidence in relation to his theory-building enterprise. In many of the arguments cited in this essay, we have witnessed the major method of argumentation to which Wallerstein resorts: the teleological assertion. Repeatedly he argues that things at a certain time and place had to be a certain way in order to bring about later developments that accord with what his system model of the world capitalist economy requires. If the actual causal patterns suggested by historical accounts or comparative-historical analyses happen to correspond with the *a posteriori* reasoning, Wallerstein considers them to be adequately explained in terms of his model, which is, in turn, held to be supported historically. But if obvious pieces of historical evidence or typically asserted causal patterns do not fit, either they are not mentioned, or (more frequently) they are explained in ad hoc ways and/or treated as "accidental" in relation to the supposedly more fundamental connections emphasized by the world-system theory. Frankly, I find this aspect of Wallerstein's approach very disturbing because it has the effect of creating an impenetrable abyss between historical findings and social science theorizing. For, through his *a posteriori* style of argument, deviant historical cases do not force one to modify or replace one's theory, while even a very inappropriate model can be illustrated historically without being put to the rigorous test of making real sense of actual patterns and causal processes in history.

Which brings me to my second and final methodological point. I pointed out that Wallerstein hoped to overcome the worst faults of modernization theories by breaking with their overemphasis on national states and their tendency toward ahistorical model building. Ironically, though, he himself ends up reproducing the old difficulties in new ways. Thus strong states and international political domination assume crucial roles in his theory – though, just like the developmentalists, he reduces politics to economic conditions and to the expression of the will of the dominant groups within each national arena. Moreover, Wallerstein creates an opposition between a formalistic theoretical model of universal reference, on the one hand, and the particularities and "accidents" of history, on the other hand – an opposition that uncannily resembles the relationship between theory and history in the ideal type method of the modernization approach.

How could these things happen, given Wallerstein's original intentions? The answer, I suggest, is the "mirror image" trap that plagues any attempt to create a new paradigm through direct, polemic opposition to an old one. Social science may, as is often said, grow through polemics. But it can also stagnate through them, if innovators uncritically carry over outmoded theoretical categories (e.g., "system") and if they define new ones mainly by searching for the seemingly direct opposite of the old ones (e.g., "world system" vs. "national system"). For what seems like a direct opposite may rest on similar assumptions, or may lead one around full circle to the thing originally opposed. The better way to proceed is to ask what new units of analysis – probably not only one, but several, perhaps changing with historical points of reference – can allow one to cut into the evidence in new ways in order to investigate exactly the problems or relationships that the older approaches have neglected.

Theorising the International System: Perspectives from Historical Sociology

Stephen Hobden

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Introduction

Over the past fifteen years there has been an increase in interest by IR theorists in the work of historical sociologists. This is primarily because of the renewed interest in the state shown by writers such as Theda Skocpol, Charles Tilly and Michael Mann. This concern with "bringing the state back in" has been reflected in the substance of the references that have been made to the works of historical sociologists. Contemporary work in historical sociology has developed Weberian notions of the state, seeing it as a set of institutions that claim precedence over a particular territorial area. For IR the state is a central concept, though it is surprisingly under-theorised. Traditionally the state has been viewed as a container that occupies a territorial space. Additionally, a distinction has been drawn between the form of political order within the state compared to that between states. The former is characterised by hierarchy, the latter by anarchy. These different realms, it is argued, require different kinds of theory. By contrast, most historical sociologists do not envisage the state as a territorial and social totality. Instead, the state is viewed as a limited set of institutions that claims the right to wield coercive powers over a particular territory. This set of institutions not only has to compete for resources with other groups within a territorial area, but also with other states in different
territorial areas.

However, despite this interest shown by IR scholars in the analysis of the state by historical sociologists, less attention has been paid to how they view the environment in which states operate. A key advance has been their locating of the analysis of the state within an international environment. Traditional Sociology has been concerned with social relations within one society (usually within the boundaries of a particular country). More recent work has become increasingly interested in examining the impact that multiple societies have on each other. Several historical sociologists refer to an "international system" or equivalent, but so far little attempt has been made to examine exactly what is meant.

The purpose of this article is to investigate how Skocpol, Tilly, Mann and Immanuel Wallerstein incorporate the notion of an international system in their work. These writers were chosen for examination on three grounds. First, they are the historical sociologists that have received the most attention from IR scholars. Second, Wallerstein's work marks a contrast to the other three and provides a way of highlighting particular problems with their approach to the theorising of international systems. Third, in terms of the analysis attempted here, it is possible to trace a development through the works of Skocpol, Tilly and Mann. Skocpol, in her work on revolutions, provides a clear statement of her theoretical view of international systems. Tilly presents a much larger scale picture in his analysis of state formation in Europe, drawing in particular on the link between war and state development. Mann's analysis is the most complex, based around his work on the sources of social power.

The central argument will be that for these writers the concept of international system is vague and under-theorised. In each of their works, the treatment of international systems is inconsistent and is heavily influenced by realism. For these writers, clarity in the concept of the state is bought at the expense of incoherence in the analysis of international systems. The final section of the article proposes an agenda for a historical sociology of international systems or "global structures".

International System Theorising in Skocpol, Tilly, Mann and Wallerstein

Skocpol's first major work examined the causes and outcomes of the French, Russian and Chinese revolutions. An analysis of the state, defined as "a set of administrative, policing, and military organizations headed, and more or less well coordinated by, an executive authority", was central. For Skocpol the state has a dual role, one foot in the domestic situation and one in the international. Hence it is necessary to investigate not only the relations between states and the domestic social classes, but also among states in an international system. She introduced the concept of an international system as an important element in her analysis, as an influence in the occurrence of specific revolutions, and as a considerable determinant of their outcomes. Prior discussions of revolution had concentrated exclusively on conflict within national societies. Skocpol maintained that it was also important to consider transnational relationships that "have contributed to the emergence of all social-revolutionary crises and have invariably helped to shape revolutionary struggles and outcomes."

Two transnational contexts are relevant, Skocpol argues. First, there are two types of structure, a world capitalist economy, and an international system of states. She argues that the important factor about these two structures is that although they are interdependent, neither is reducible to the other. The second context is world historical time. Two aspects are involved here: changes that have occurred between one time period and another; and transmissions, or rather, the availability of information or models to later actors. Skocpol's work provides a sophisticated approach to theorising international systems, combining a number of features: economic, political and temporal. However, despite this cogent theoretical position, when it comes to a discussion of the historical material her approach is to reduce international systems to the presence or absence of warfare. This reductionist position is most noticeable in relation to her discussion of the factors leading up to the French Revolution and the influences on the postrevolutionary regime:

> Warfare was far from extrinsic to the development and fate of the French revolution; rather it was *central and constitutive*, just as one would expect from knowing the nature and dilemmas of the Old Regime from which the revolution sprang.

It is not an international system that influences the developments of revolutions, but rather the interactions between states, and then only in the form of violence: warfare is a determining factor in development rather than an influence.

Skocpol's historical discussion does not match up to the power of her theoretical position. Her coherent account of the state is bought at the expense of a far vaguer notion of international system. Warfare is central to Skocpol's notion of international system. Her explanation for the occurrence of revolution is based to a large extent on the old regime's involvement in international war. Under the new, revolutionary regime, the state becomes rationalised, primarily because of the need to consolidate in the face of international conflict. Hence Skocpol's notion of the state is dependent on the way that, in practice, she has employed the notion of international system.

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A similar concentration on the role of war is apparent in the work of Tilly. He is famous for his statement that "War made the state and the state made war". However, in his most recent work on the formation of nation states and cities in Europe Tilly has built on and superseded the simplicity of this statement. Patterns of state formation are now seen as being determined by the presence of concentrations of coercion (associated with states) and/or concentrations of capital (associated with cities). Different combinations of the concentration of capital and coercion resulted in the emergence of different forms of nation state.

An important factor in this process of state formation is the impact of international systems. Tilly argues that "other states – and eventually the entire system of states – strongly affected the path of change followed by any particular state". Competition is a central feature of international systems: "national states always appear in competition with each other." Tilly also describes international systems as being "made", first in Europe and then through the export of the state form that developed there: "Five hundred years ago, Europeans were busy *creating* a pair of arrangements that were then unique". These remarks are in direct contrast to the view that war and competition are the central constitutive features of international systems. Negotiation, understanding, norms, and rules now define international systems.

Hence there are two accounts of international systems in Tilly's work. On the one hand the role of war is a central factor. As was noted he makes a clear link between war and international systems. He perceives international systems as a product of warfighting. It is also possible to locate a rules-and-norms-based account. As well as relating war to international systems, Tilly, at times, also depicts international systems as webs of rules and norms that act to constrain state actions. Tilly does not analyse how these contrasting views are related. He applies different views of international systems to account for different aspects of state development. When he wants to explain the increase in the scope of state activities he focuses on involvement in fighting wars. When he wants to explain the way in which state activities are constrained, he discusses an international system constituted by norms and rules. As with Skocpol, coherence at the state level is achieved through the employment of a vague and inconsistent view of the international system.

Inconsistency of accounts of international systems is also a feature of Mann's work. Mann is internationally famous for his work on the history of social power, a pathbreaking study of how different combinations of power relations have structured human societies. He uses two terms to describe the organisation of geopolitical space:

multi-power-actor civilisations and empires of domination. The prime characteristic of multi-power-actor civilisations is the degree of shared norms amongst a number of actors. By contrast, in empires of domination, a shared environment based on common norms is replaced by what Mann terms "compulsory cooperation." Mann makes very strong claims regarding the influences of geopolitical organisation. He uses the concept as a causal factor in his account of states, classes and capitalism. However, it is less clear what he means by geopolitical organisation and how this influence comes about. The main way that Mann describes is through "military-fiscal extraction". The demands of warfighting require the extraction by the state of resources from the domestic population. However, these extractions are not made without cost. Mann argues that the tax burden imposed to pay for warfighting activities resulted in domestic class conflict and alterations in domestic political systems.

Alongside this structural account of international systems Mann provides a discussion that stresses the importance of norms. For example, the international norms that affected the European multi-power-actor civilization concerned religion, culture, philosophy, political institutions, economy, monarchical rulers and increasingly through the nineteenth century, racism. All of these acted to increase a normative solidarity amongst the European states. In addition to these holistic accounts, it is also possible to derive two accounts from Mann's work at an individualistic level. First it is possible to view some of Mann's analysis in terms of a constructivist account whereby international systems are a creation of social actors. Alongside this constructivist view it is also possible to find statements where Mann is suggesting a rational actor position for statesmen, forever calculating the best available option. For example, he notes that "multistate diplomacy involves autonomous states with only limited normative ties, continuously recalculating geopolitical options."

Mann's approach to theorising international systems is ill-defined and inconsistent. He attributes a range of causal impacts to international systems but fails to provide a coherent framework within which these can be theorised. Instead, his approach to international systems is as a catch-all area for a variety of factors that cannot be included at a domestic level. He has produced four accounts of international systems: two at an individualistic level and two at holistic level. He employs each of these depending on what he is trying to analyse. As with Tilly, he uses warfighting to explain state growth in Europe, and a norms and rules account to outline the constraints on states. There are also views in his work of an international system as a social construction from the thoughts in the heads of statesmen, and as the result of rational-actor calculations.

Wallerstein's work on the "modern world-system" provides a contrast to the writers considered so far. For Wallerstein the Modern World-System has both spatial and temporal dimensions. The spatial dimension refers to the allotment of different economic roles to specific areas: a core, periphery, and semi-periphery. In terms of the temporal dimensions of the world-economy Wallerstein draws distinctions between cyclical rhythms, secular trends, contradictions and crisis. These are all closely inter-related and each one influences all the others. Cyclical rhythms manifest themselves over the very long period, secular trends over the middle period and contradictions and crisis occupy the level of more immediate events. Wallerstein reserves the term crisis to refer to a particular conjuncture of contradictions from which the system is unable to recover: these arise because of "constraints imposed by systemic structures that make one set of behaviour optimal for actors in the short run and a different, even opposite set of behaviour optimal for the same actors in the middle run". Outside the period of crisis the possibilities for individual action are very restricted. The structures of the world-economy are deeply deterministic. For example, Wallerstein claims that "within a functioning historical system there is no genuine free will. The structures constrain choice and even create choice". For Wallerstein the world-system is the primary element in his analysis, all other features are products of the system itself:

> The development of the capitalist world-economy has involved the creation of all the major institutions of the modern world: classes, ethnic/national groups, households – and the "states". All these structures postdate, not antedate capitalism; all are consequence, not cause.

Wallerstein's systemic approach involves costs and benefits. The prime benefit is the parsimony of his approach. Although it involves several elements Wallerstein's world-system is a clear and simple theory, from which he is able to derive a considerable amount of explanatory and predictive mileage. On the deficit side is a lack of clarity at the sub-unit level. Wallerstein does not have a developed theory of the state: states are either strong or weak depending on their position in the world economic hierarchy.

A Critique of Systems Theorising in Skocpol, Tilly and Mann

In the work of Skocpol, Tilly and Mann the view of international systems has been vague and inconsistent. Their clarity in the analysis of state formations has been achieved only by reducing international systems to warfare (in Skocpol's case), or through using different accounts of international systems depending on what was being explained. The impact of these problems results in an approach to international systems theorising that is not able to provide the form of analysis that these writers are seeking. These specific criticisms are less appropriate for Wallerstein's work. His view of international systems cannot be regarded as inconsistent, incompatible, or heavily influenced by realist approaches. Wallerstein's starting point is a world-wide capitalist system, which is responsible for the other social formations of the contemporary world. His work, with its stress on a systemic analysis, forms a counterpoint to the other authors considered.

Inconsistency

The analysis of international systems presented by Skocpol, Tilly and Mann is inconsistent because they provide multiple accounts and fail to discuss how the factors in each of these accounts are related to each other. This inconsistency is most apparent in the discussion of Mann's *The Sources of Social Power*, where there were four different analyses of international systems: a view of international systems as external structures; a rational-actor game playing version; a view of the system as a set of rules; and a version implying that international systems are a construct of social actors. Mann provides no account of how the relationships between these four different versions are to be understood. This makes it difficult to understand what his perception of international systems is.

There is a failure to analyse the links between his view of international systems and the phenomena that he claims are heavily influenced by it. For example, he argues that states were formed as a response to pressures from the international realm. This is a difficult assertion to accept when it is unclear what is meant by the international realm. There is a similar inconsistency in the works of Skocpol and Tilly. Skocpol provided a theoretical view of international systems that comprised inter-state relations and a world economy, however in the empirical account this systemic position was lost to a view that concentrated on states" involvement in fighting wars with other states. Tilly presents two accounts of international systems: an inter-state system of war and a network of rules and norms. Inconsistencies in the accounts of Skocpol, Tilly and Mann become clearer when their view of international systems are compared to Wallerstein's discussion of the world-system. Wallerstein's view of system is confined to what he calls the modern world-system, essentially an economic system. It is from this one factor that Wallerstein derives his analysis of all social phenomena. There is only one element to his account, and this is responsible for the generation of the other features of the social world.

Incompatibility

The problem of the inconsistency in the accounts of international systems provided by Skocpol, Tilly and Mann is further compounded by the incompatibility between the different elements. Each is trying to produce a single account of international systems from elements that it is problematic to combine. This incompatibility arises because the different accounts of international systems are derived from two distinct approaches to the social sciences. The first stresses a scientific approach to analysing the social world: an explaining account. The second is concerned with comprehending the activities of actors: an understanding account. These two accounts contain different conceptions of structures and agency. In one account it is structures and rational actors that are the object of study. In the other account it is mutual expectations and actors as social agents that are the basis of the analysis.

As an example, the examination of Tilly's work suggested that he had provided two distinct accounts of international systems. These two accounts are founded on different epistemological approaches to analysing the social world. The first account sees an international system as an external given – an anarchic competitive realm. It is competitive warfighting that accounts for the phenomena that Tilly is attempting to explain. The second account is based on rules and common understandings. Here it is the norms and rules of state interactions that act as a constraining factor on state activities. These two accounts have different conceptions of structures: the first views structures as an outside environment of competitive anarchy, while the second depicts structures as a collection of shared understandings. The different conceptions of structure cannot be added together to form one account of an international system. This problem was also highlighted in Mann's approach. As in Tilly's work he is attempting to combine a rules-and-norms account with a structural account. These accounts rely on different epistemological assumptions that make them difficult to include in one analysis.

By contrast, Wallerstein provides a more consistent account of a world-system. Here the underlying epistemological position is that the world-system can be analysed as an external economic structure. He is not attempting to combine this with an account based on alternate epistemological assumptions. Wallerstein's modern world- system provides a top-down structuralist account that is maintained with remarkable uniformity.

Realism

The third general criticism is an over-reliance on realist interpretations of international systems. A textbook realist view of international systems is one that is state-centric, sees the state as a unitary, rational actor, and considers the competition for power and security to be the prime concern of state actors. None of the historical sociologists considered could be described as providing a one-dimensional realist analysis – they provide a much more nuanced account of the state. However, their views of international systems are heavily influenced by realist approaches. This is particularly evident in the use of sources by Tilly and Mann. Tilly cites for his main sources of geopolitical analyses Waltz (and an early work by Rosenau). Mann states that for his analysis of geopolitical power he "draws freely upon" Knorr and Morgenthau.

Although Skocpol is less explicit about the sources for her account of international systems, the influence of realism is seen most clearly in her work. Her theoretical statements on international systems attempt to combine an economic analysis with a discussion of competing states. However, in her empirical analysis this was forsaken for a discussion of the occurrence of war between states. In her discussions of revolution, international systems are reduced to traditional realist concerns with war between states as the central feature. In a similar way both Tilly and Mann consider the occurrence of war in international systems to be the most significant variable in the process of state formation.

This influence of realist-based accounts of international systems is a source of weakness because it makes it more difficult to include other factors. A realist account of international relations, because of its stress on states as the most significant actor, downplays the role of other features and processes occurring internationally. These other factors are highlighted by the writers considered here – particularly the importance of economic factors – hence the overreliance on a realist-type of analysis is puzzling. Mann, Skocpol and Tilly are trying to incorporate the notion of an international system as a variable in their analysis of a range of social phenomena (capitalism, revolution, and class formation), however, their concentration on an international system defined in terms of the occurrence of war restricts its usefulness as an element of their work. Wallerstein's account of the world-system, with its primarily economic base, cannot be considered to be overly reliant on realist formulations. Military force is a less important factor in Wallerstein's work, and states are not the central actors.

Towards a Historical Sociology of Global Structures

Of the historical sociologists considered only Wallerstein provides an account that could be considered as fully systemic. However, the use of Wallerstein's work as a counterpoint to that of Skocpol, Tilly and Mann was not intended as an exercise in extolling the virtues of a world-systems account. Wallerstein's work contains a much more coherent account of an international system, but the price to be paid for this coherence is the over-determination of his systems theory. Compared to Wallerstein, the other writers make weaker claims about the causal power of international systems, however they do include international contexts as an important independent variable in their work, even though they have failed to demonstrate clearly how its influence affects social developments. Two kinds of problem have been encountered during this discussion. On the one hand Wallerstein's deeply deterministic system does not provide a satisfactory analysis, and on the other international systems remain under-theorised in the work of Skocpol, Tilly and Mann.

Wallerstein has provided a much more systemic account; however, this is at the cost of a less developed notion of the state. For Wallerstein, the state is a product of the modern world-system. A state is either weak or strong dependent on its location in the world-economy. Wallerstein achieves a clear notion of a world-system by producing an incomplete account of the state. He has held the state as a primitive factor, which fulfils the requirements that he needs to produce his account of the system. Skocpol, Tilly and Mann have less defined accounts. Compared to Wallerstein the approach to the international system in the work of Skocpol, Tilly and Mann appears more ambiguous. However, in Wallerstein's work the analysis of the (world)-system overwhelms the other elements of the social world. The approach to the international system is the other way round for Skocpol, Tilly and Mann. They have held the system as a primitive concept and have introduced it into their analysis of the state when, and in the form that they have required. Their coherence at the state level is dependent on an inconsistent approach to theorising the international system. Hence there are problems with the way in which the concept of international system has been utilised by historical sociologists. Accounts provided by Skocpol, Tilly and Mann lack coherence because they have used them to bolster their analyses of the state. The international system has fulfilled the role that they have required to keep their account of the state consistent. Wallerstein, whilst having a very strongly developed notion of "system", has not matched this with a coherent account of "units".

Despite these problems there are elements in the general approach used by historical sociologists that might provide the basis for a more historically based analysis of international systems. Prime amongst these is the inclusion of an historical element in the analysis. This has been most successful with the analysis of the state. IR has had a tendency to take the state as a given. Following the work of writers such as Skocpol, Mann and Tilly this is no longer possible. These writers have succeeded in producing a historical account of state development that suggests that the state is historically contingent. Changes in state forms will occur as a result both of relations with other states and different social actors. This historicisation of the state as a social form by historical sociologists has not been matched with an equivalent account of international systems. However, linked to the analysis of the state, there is some conception of ways in which theorising of international systems could be historicised. It is most striking in Wallerstein's work where he has devoted much space to the analysis of the temporal features of the world economy which will, in his view, lead to its eventual collapse. There is also a theory of change of international systems in Mann's work. In volume one of the *Sources of Social Power*, there is a detailed analysis of the process by which the social forces present in an empire of domination lead to its replacement by a multi-power-actor civilisation and *vice versa*.

The challenge for IR theorists is to develop a more coherent analysis of what constitutes an international system. What might such an analysis comprise? A starting point for such an approach would be to build on the multi-logic approach used by Mann. This would envision multiple forces at work internationally, none ultimately determining. The suggestion made by Buzan, Jones and Little (1993) to develop a sectoral approach provides a positive step towards such an aim. They suggest developing an approach based on economic, societal, strategic and political sectors. Each of these sectors would require its own structural analysis. It would also need an analysis of:

- *The form of the units within each sector.* This needs to consider who the actors are within each sector, and how they interrelate. Some actors may be active in more than one sector, or even all.
- *The constitution of units and structures in each sector.* There is also a requirement to consider the units and the structures as historical entities, and to trace the form in which they have developed. The research by historical sociologists on the development of the state would provide a model for this endeavour.
- *How the structures and units change historically.* There is also a requirement for explaining change within sectors. This needs to be followed by a discussion of what factors generate change.
- *How the sectors interact.* Finally, there is a need to understand the relationships between different sectors. This is a particularly difficult exercise, given the constraints

discussed above. However, there are links between the different sectors, for example, political and economic, and including them in the analysis would add depth to the analysis. A multi-logic approach suggests that at any particular time one or a combination of factors might be dominant, but that this would change historically.

Evidently, this is not a minor undertaking. The unravelling of these structures and the analysis of their inter-relations would be a huge task. However, analysis of international systems requires the awareness of historical contingency and the existence of a variety of structures and units. Developing a more historically and socially aware account of international systems would result in an advance in theorising that could have implications for both historical sociology and IR. For IR theorists the approach suggested here would provide a richer account of international events. This would aim to provide an understanding of the links between units and system. Traditional approaches to international relations see units and system as static. By contrast, this approach would attempt to highlight the dynamics of system and unit changes and their inter-relations. Additionally, instead of seeing the state as the dominant actor, this approach would allow the analysis of other actors. Hence the development of social forms, their interactions, and their demise would all be included on the agenda.

The extent of this project and the radical re-thinking of international relations that it implies suggests that the term "international system" may no longer be useful. The approach being proposed here is much more than an analysis of a "system of international relations" with its implicit determinism. Such an account would imply a much wider analysis that would aim to incorporate a broader range of actors within a conception of structures that extended beyond both political and economic. For this reason, the term "global structures" might give a better indication of the analysis intended. This term encompasses the different actors (for example, states, nations, classes, NGOs) included in the analysis. Additionally, it suggests that actors function within the constraints of a number of frameworks. Furthermore, it would suggest the possibility of an account of actors and structures that would be co-constitutive, and that incorporates an element of autonomy. Such an approach to understanding the social world on a global basis could provide an alternate agenda for IR capable of moving beyond the ahistorical analyses of traditional theorising.

Which Historical Sociology? A Response to Stephen Hobden's "Theorising the International System"

Daniel Nexon

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In a recent article in the *Review of International Studies*, Stephen Hobden does a great service by initiating a critical evaluation of the potential for historical sociology in international relations theory. Hobden considers seminal studies by Michael Mann, Theda Skocpol, Charles Tilly and Immanuel Wallerstein, and concludes that each is inadequate for building an historical sociology of the international system. Articles such as Hobden's are particularly important in international relations, where many major theories are dependent upon the assumptions and methods of other disciplines. From time to time, we need to ask, in a comparative manner, just how useful such methods and assumptions really are.

However, I believe that Hobden ultimately does not pay detailed attention to the very assumptions and methods that drive the theorists he examines. While his criticisms of Skocpol and Wallerstein are largely persuasive, he fails to take seriously the sociological relationalist nature of Mann's and Tilly's work. Relationalism – which has emerged as a major paradigm in sociology, and particularly in historical sociology – treats patterns of transactions as the starting point of social theory, as opposed to, for example, the elucidation of relatively autonomous systems. Thus, neither Mann nor Tilly accept as legitimate the distinctions – between structure and norms, rational actors and social agents – upon which Hobden's critiques of inconsistency and incompatibility are premised.

While the fact that relationalists reject Hobden's critical apparatus does not necessarily negate his arguments, a serious engagement with Mann's and Tilly's relational assumptions might have forced him to better justify his own analytical and epistemological positions, which in their present form, are largely unpersuasive. Furthermore, Hobden bases his critiques on works with specific empirical puzzles that are related to, but not identical with, his concern for broader theorisation about the "international system". As a consequence, Hobden passes over what I believe is the most important issue raised by his critical review: the choice between relationalist and neofunctional systems approaches to the study of change in the international system.

Sociological Relationalism

Hobden is correct when he notes that we need to move "beyond the ahistorical analysis" so prevalent in "international theorising." In doing so, he echoes several scholars who have focused upon the need to theorise change within and between "international systems." What we mean by "change", however, is not always clear. Hobden focuses on the need for a "coherent analysis of what constitutes an international system" as a critical step towards analysing change. However, relationalists would caution us to start with the prior question of what constitutes "change" itself.

In general, when international relations scholars analyse change they study processes that alter sociocultural entities or, to varying degrees, the patterns of relations between entities. This means that a major problem for studying change is our tendency to treat processes and relations as substances. For example, we call relative stabilities in patterns of interaction "structures" and treat them as autonomous things, and we simplify international relations by treating agents – such as states, transnational organizations, or even individuals – as coherent, purposive entities. Likewise, functionalists (and many neofunctionalists) begin with systems, whose inner logics are the object of explanation. There are many good reasons for doing so, but when we want to explain alterations in these "things" our own method of treating them as stable substances gets in the way.

Relationalists attempt to solve this problem by taking processes and relations as the building blocks of analysis. Rather than begin with macrophenomena – structures and societies, for example – or microphenomena – agents or individuals – relationalists believe that the proper starting point of analysis is at the *mesolevel*: patterns of transactions themselves. For them, agent-centric theory "builds upon the myth of the person as some pre-existing entity," while "structuralism builds from the myth of society as some pre-existing entity". Rather, relationalists propose that, "the very terms or units involved in a transaction" are constituted by the changing roles "they play within that transaction". Thus, any *a priori* commitment to agents or structures should be avoided.

Hence, relational historical sociologists are not, as Hobden states, focused upon "examining the impact that multiple societies have on each other". Mann rejects such categories, arguing that "societies are not unitary. They are not social systems (closed or open); they are not totalities", and thus "because there is no bounded totality, it is not helpful to divide social change or conflict into 'endogenous' or 'exogenous' varieties". For him "society" is analytically better described as overlapping and intertwining power networks.

Tilly's understanding of coercion and capital also follows this logic. As Hobden notes, coercion and capital are analytic categories representing social networks on the European landscape. In conjunction with processes of military-technical change, different constellations of coercion and capital were more successful than others. At the same time, the very coercion and capital networks within which actors were embedded – and which constituted their interests – were transformed (primarily) by political bargaining associated with warfare.

Thus, processes immanent in social relations are what produce both states and the international system. These transactional relations are analytically prior to *both* international structures and agents, and they are *both* material and cultural in nature. It follows that relationalists are logically required to reject neopositivist variable analysis, where entities remain fundamentally unchanged while their variable attributes "interact, in causal or actual time, to create outcomes, themselves measurable as the attributes of fixed entities". Causal interaction takes place *among* entities and is not generated by the entities themselves. For relationalists, configurations of processes give rise to new configurations: the doings of entities can actually transform entities.

While Mann focuses on power networks, Tilly develops a method focused on "comparative processes". Given a number of similar phenomena – revolutions and wars, for example – social scientists should not attempt to explain their efficient causes. Instead, they trace the processes that lead to different paths *within* those events. Tilly likens historical events to the hydrological understanding of floods: floods occur when water levels exceed a given water table, but there can be no more specific definition of a flood since each of those events is unique, caused by multiple factors, and takes different pathways. If events such as revolutions are thought of in this way, then social scientists should be concerned with the way causal mechanisms generate different pathways of revolution given unique configurational contexts: laws of variation rather than invariant laws.

Relationalists argue that a major reason for historical analysts to adopt their conceptions of causality is the logic of "prospective" path dependency (as opposed to "retrospective" path dependency). In retrospective path dependency models, scholars take a given outcome – such as the decision of a state to go to war – and trace backwards through historical events, looking for critical junctures which produced the pathways leading to that outcome. Such analysis is teleological: the theorist searches backwards for switching points which, once taken, inevitably give rise to the predetermined outcome. The writing of history is structured to give causal efficiency to subsequent events. In contrast, Tilly's conception of path dependency is "prospective" in that previous events shape the possibility of future events. This insight creates serious difficulties for neopositivist methodology and the quest for invariant laws.

In brief, we cannot effectively cut history into independent and dependent variables with the requisite comparability required for neopositivist causal generalizations. Laws of variation allow the incorporation of path dependent logic by integrating past events into the configurational context of subsequent events. Moreover, relational logic involves a more subtle shift in our understanding of causal generalizations. Thus, a theorist looks for recurrent processes, and shows how widely applicable causes concatenate into substantially different outcomes depending on initial conditions, subsequent sequences, and adjacent processes". While not all relationalists adopt Tilly's solution, they do avoid the logic of independent and dependent variable approaches to causality. These approaches, after all, require stable entities with variable attributes, something relationalists call into question. With this overview, I now turn towards Hobden's three criticisms: inconsistency, incompatibility, and realism.

Consistency and Compatibility

Tilly's and Mann's alleged inconsistencies and incompatibilities derive from three purported dichotomies: between "structure" and norms, between structural and agent-centric analysis, and between agents as rational actors and as instantiators of structure. They are inconsistent because they make use of such multiple images and "fail to discuss" how they are "related to each other". Hobden claims these inconsistencies rise to the level of incompatibility because they make use of two "distinct approaches to social science": one which focuses on scientific explanation and one which focuses upon "understanding" the "activities of actors". For many sociologists, and even for constructivists in IR, none of these dichotomies makes a great deal of sense. Structure has both material and cultural manifestations, instrumental rationality and norm-patterned behaviour are two analytical facets of action, and actors can simultaneously respond to and instantiate structure. Even among systems theorists and neofunctionalists who insist upon the relative autonomy of culture, structure and agency, these distinctions are understood as analytical in nature: all are necessary if social outcomes are to be explained in a non-reductionist manner.

As I have argued, relationalists such as Mann and Tilly reject this framework. Mann, for example, *cannot* consistently develop "links between his view of the international system" and other phenomenon because he rejects any *a priori* relative autonomy for systems. For him, "there is no system" and thus social action cannot be explained with ultimate reference to any putative component system of action. Systems, as we experience them, are the historical consequence of transactional configurations and thus it would be nonsensical to develop causal generalizations about their interaction. For Mann, "organization" is a product of the configuration of power networks: thus, "geopolitical organization" and "geopolitical diplomacy" are not equivalent to anarchical structures in the neorealist sense, but are rather spatial consequences of patterns of transaction. While they have causal ramifications, they also should not be understood as independent variables.

For Tilly, the critical processes of state transformation can derive from the interaction of military-technical change and networks of capital and coercion without excluding the roles of rules and norms. War is just as much norm governed as it is material in nature.[37] Furthermore, Tilly's mechanisms of change derive from the transactional bargaining between rulers, subject, other rulers, and potential subjects; in all these, norms and rules are immanent. This is all he means when he writes that "Europeans were busy creating a pair of arrangements that were then unique". Hobden is presumably opposed to relationalism. While he wishes to maintain agent-structure co-

constitution, he also does not wish to sacrifice the autonomy of either agents or structure. Although Hobden's stance is not unique in the social science, it has no more *a priori* validity than relationalism. Thus, if he wishes to critique sociologists such as Mann and Tilly, he needs to defend the utility of the rigid analytical and epistemological dualisms upon which his own arguments are predicated.

Realism

Hobden argues that Mann and Tilly are committed to realism and thus ignore nonrealist (non-bellocentric) sources of change; realism's putative taint is also to be found in Tilly's and Mann's supposed state-centrism. But neither Mann nor Tilly adopt realist assumptions about states; rather, they view them as reified products of social relations. In fact, both launch a profound critique of state-centrism, arguing that the existence of the nation-states has given unwarranted plausibility to the notion that societies are discrete, closed systems that can be analysed with reference to overarching structures. Tilly argues that the comparative method itself has been biased; even if state-centric assumptions about the comparability of units had merit in the past, they do not apply to the contemporary postmodern period where the centrality of the state has greatly diminished.

Even if we put aside the question of relationalism, it is difficult to see how Tilly's work – *whose object is the emergence of the modern state* and which seeks to explain the relative decline of alternative forms of political authority – can be criticized on these grounds. Tilly himself does not believe he is developing general, determinative laws of the dynamics of the international system, but merely an elucidation of the critical processes of state transformation. As I have argued, relational analysis seeks a far lower level of generality than standard causal reasoning: it is explicitly anti-systemic in its historical sociology. If Tilly (or Mann, for that matter – who focuses on four power networks, only one of which is military) has overstated the causal relevance of war, that is an empirical question, not one inextricably tied to their conception of historical sociology. The real question is whether relational techniques are better equipped to explain international processes of stability and change.

Conclusions

What are the implications for Hobden's positive argument? He maintains that the undue focus on state formation in these theorists has led to a neglect of the historical sociology of the international system and calls for an abandonment of the "strict dichotomy between domestic and international". In its place, Hobden wants theorists to turn towards multiple, overlapping sectors of international politics – each with its own structure and corresponding units. In turn, theories should be developed that focus on how these "sectors interact", that is, how they are causally and constitutively linked. Hobden goes so far as to suggest that his arguments could benefit historical sociologists.

What Hobden suggests is, in essence, a careful application of sociological neofunctionalist principles to international relations. Neofunctionalism, which has gained adherents in mainstream constructivist scholarship, reconstructs systems theory by abandoning its functionalist assumptions in favour of causal and constitutive linkages between systems and subsystems. Neofunctionalists echo Hobden in their call for explicit theorising on the interconnection between relatively autonomous systems, be they economic, social, political, cultural or personality in nature. Relationalism stands in direct opposition to such a project, and the historical sociology of Mann and Tilly provide plausible reasons to reject it.

I believe the choice between neofunctionalist or relationalist sensibilities lies at the crux of current debates in constructivist IR and the study of change – and this is my biggest frustration with Hobden's article. An exploration of the contributions of historical sociology to IR needs to explicitly raise the comparative assumptions, techniques and merits of the two approaches. Is a focus on process and relations the best way to build accounts of change? For what reasons might we opt for a neofunctionalist mode of inquiry? Although Hobden lays out a plausible research programme in the latter tradition, his engagement with Mann and Tilly necessitates a more thorough discussion of these questions.

The End of History – Or Its Rebirth?

Philosophy of History at the End of the Cold War

Krishan Kumar

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THE RECOVERY OF THE PHILOSOPHY OF HISTORY

Writing after the Second World War, the German philosopher Theodor Adorno wrote: "After the catastrophes that have happened, and in view of the catastrophes to come, it would be cynical to say that a plan for a better world is manifested in history and unites it". Here he echoed the famous remarks of Walter Benjamin, in the ninth of his "Theses on the Philosophy of History," that history is "one single catastrophe," and "one pile of debris." Both Benjamin and Adorno wished to hold on to the hope of progress that marked the materialist conception of history; both however seemed intent on undermining any comfortable belief that the historical record provided much evidence for such hope. The spirit, perhaps, was that of Kafka's: "there is hope; but not for us."

A belief in progress is not necessary for a philosophy of history. Oswald Spengler's *Decline of the West* (1918–22), proclaimed to a war-devastated Europe its Nietzschean message of the inevitable downfall of their western civilization. But the philosophy of history, whether expressed in spiritualist or materialist terms, seems incapable of an absolute abandonment of hope. Even Spengler thought that by enlightening his fellow countrymen, as well as westerners generally, he was pointing the way to a future recovery. His book, in other words, was a rallying call, not a requiem.

The twentieth century managed to wring hope out of catastrophe – sometimes hope because of catastrophe. This was displayed in the many philosophies of history that, despite the mockery and assaults of the professional historians, continued to appear in abundance in the first half of the century. This serves to remind us that the idea of progress did not, as is frequently claimed, die in the mud of Flanders during the First World War. While certain, mainly literary intellectuals, certainly recoiled at the horror, others – scientists and social scientists especially – continued to find reason for hope. Marxism continued to be the main stimulus to the hopeful philosophies of history in the first half of the twentieth century. The success of the Bolshevik Revolution, and the improbable survival of the Soviet Union in the hostile environment of a capitalist world, heartened those who, whatever their disquiet about actual developments within the Soviet Union itself, thought that capitalism was doomed to self-destruction.

All the more consequential, then, was the impact of the growing disillusionment with the Soviet Union from the mid-century on. Arthur Koestler had already, in his brilliant anti-communist novel, *Darkness at Noon* (1940), announced his defection from the old cause. Even more powerful was the prophetic novel of his friend, George Orwell, whose *Nineteen Eighty-four* (1949) was widely (though mistakenly) read as a denunciation of Soviet Communism. Later, in the post-Stalinist "thaw" in the Soviet Union itself, came the revelations of the grim reality of Stalinist Communism and, by implication, all that had gone wrong with the Soviet Union. In place of glowing images of the proletarian hero of the future came harrowing stories of the *gulag*. The novels and other writings of Aleksandr Solzhenitsyn in particular undermined for most people any lingering faith there might have been in the Soviet Union as the civilization of the future.

The appeal of communism to western intellectuals faded rapidly in the post-war era. For those who kept something of their socialist faith, the future lay with the ameliorism of "social engineering" and the welfare state rather than the revolutionary transformation of society and its culmination in the socialist utopia. The ideology of managerialism, and a general distrust of large-scale schemes and visions, dominated the thinking of many post-war intellectuals. For many, Karl Popper's *The Open Society and Its Enemies* (1961), with its attack on all forms of "historicism" and utopianism as harbouring totalitarian tendencies, was the Bible.

The period of the Cold War was therefore extremely inhospitable to large-scale historical and philosophical speculations such as had provided the material for the earlier philosophies of history. To the still-continuing denunciations of the professional historians and philosophers was now joined a climate of caution and moderation. The Cold War – and the nuclear "balance of terror" that sustained it – might give rise to apocalyptic visions of nuclear war and post-nuclear civilization, but it seemed to dampen thoughts about the future of mankind as seen in long-term historical perspective.

The end of the Cold War, in the anti-communist revolutions of 1989 in central and eastern Europe and the subsequent dissolution of the Soviet Union itself in 1991, broke this impasse. These were events of such magnitude that they seemed to cry out for some sort of sustained reflection, some sort of placing within the larger scheme of things. It cannot have hurt, either, that they occurred – at least in the western calendar – at the end not just of a century but of a millennium. It was inevitable that the fall of communism and the end of the Cold War would be seen as carrying millennial, or at the least, epochal, significance. As Eric Hobsbawm wrote,

> the end of the Cold War proved to be not the end of an international conflict, but the end of an era; not only for the east, but for the entire world. There are historic moments which may be recognized, even by contemporaries, as marking the end of an age. The years around 1990 clearly were such a secular turning point.

It is within this context that we should understand the significance and the impact of the writings of Francis Fukuyama and Samuel Huntington. They chimed with the mood of great hopes, but also of great fears. Neither Fukuyama nor Huntington in truth offered much comfort, though the former in particular was thought to do so. But they did renew a tradition of thought that had fallen into disrepute. They did attempt to place their times in some historical pattern, to read their significance within the wider framework of western and even world history. They did, to that extent, renew the philosophy of history.

THE END OF HISTORY: HEGEL *REDIVIVUS*

In the summer of 1989 there appeared an article in the conservative journal, *The National Interest* entitled "The End of History?" Its author was the Deputy Director of Policy Planning at the U.S. State Department, Francis Fukuyama. The article, written in a high-profile journal much read by policy-makers caused quite a stir, not just among policy-makers and intellectuals in America but in many other countries as well. It was allegedly debated by Margaret Thatcher and Mikhail Gorbachev and their advisers, among other prominent figures. Buoyed by the success of this foray, Fukuyama abandoned his State Department post and devoted himself to a full-scale book treatment of his theme. This duly appeared – with the question mark removed – in 1992 as *The End of History and the Last Man*.

Fukuyama was not just a middle-ranking State Department official. The appearance of an amateur intervening in high-level intellectual debates was deceptive. Fukuyama was a classicist and political scientist who had studied under Roland Barthes and Jacques Derrida in Paris and had gone on to specialize in Middle Eastern and Soviet politics. More relevantly he was a Hegelian, who had been introduced to Georg Wilhelm Friedrich Hegel, and more particularly Alexandre Kojève's persuasive interpretation of Hegel, by his Chicago teacher, the influential philosopher Allan Bloom. Fukuyama applied Hegel's philosophy of history to his own times.

When Fukuyama first wrote his article, the 1989 revolutions in central and eastern Europe were only just getting under way, but change in the region was manifest. *Glasnost* ("openness") and *perestroika* ("restructuring") were in full swing in Gorbachev's Soviet Union. Encouraged – or rather, not discouraged – by Gorbachev, the east Europeans wound up communism in their societies by the end of 1989. Two years later the Soviet Union itself followed suit. By the time Fukuyama's book appeared the 75-year communist experiment in eastern Europe was over. Fukuyama felt justified in removing the question mark that hung over his original article. Many others too felt that the course of events had fully justified his prognostication.

For the substance of Fukuyama's argument was that the defeat and withdrawal of communism as a force in the world represented the "end of history." For those who did not read his writings but knew only their titles, this statement appeared preposterous, and there was a good deal of uninformed denunciation of Fukuyama. A mere glance at his work would have shown that he was using the expression in the specifically Hegelian sense. The history that had ended was a history understood as the conflict of ideologies. Hegel had thought that such a history had ended with the French Revolution, and the victory of the principles of the liberal democratic state. That view, argued Fukuyama, had been premature. Liberal democracy had had to face several challenges to its dominance in the two centuries following the announcement of its principles. Most critically there had been communism and fascism, two ideologies that had been genuinely modern and that had constituted real alternatives to liberal democracy. Fascism had been defeated and now, in the closing years of the twentieth century, communism too had shown that it was incapable of competing with liberal democracy. Hegel was finally vindicated. With the simultaneous collapse of right-wing and militaristic dictatorships throughout the world in the 1970s and 1980s, the contest of ideologies, the substance of history as Hegel understood it, was over. Western-style liberal democracy had triumphed and history was now at an end.

This did not mean that history as a sequence of *events* would or had come to an end. That would indeed be an absurdity, if not an impossibility. Nor did the end of history promise a peaceful and harmonious future. There could be and probably would be ethnic, racial and national conflicts. Poverty and inequality would probably continue, together with conflicts over efforts to eradicate them. Even greater conflicts might be in store in confronting the gathering ecological crisis. There might even be full-scale war, including nuclear war. The point however was that none of this would involve *ideological* conflict. "All of the really big questions had been settled". What the west had achieved over many centuries of struggle – the establishment of liberal democracy and free markets – was now the goal of practically every society in the world. In a remarkable confirmation of this view coming from an unexpected quarter, the British Marxist Perry Anderson wrote:

> For the first time since the Reformation, there are no longer any significant oppositions – that is, systematic rival outlooks – within the thought-world of the west; and scarcely any on a world scale either, if we discount religious doctrines as largely inoperative archaisms. . . . Whatever limitations persist to its practice, neo-liberalism as a set of principles rules undivided across the globe: the most successful ideology in world history.

A note of disquiet about the future emerged more strongly in Fukuyama's book than it had in the 1989 article, where the note of western triumphalism sounded more clearly, to the distaste of many, especially on the left. In the more somber and considered appraisal of the book, the prophet is Nietzsche rather than Hegel. Fukuyama here draws on Nietzsche's portrait of the "last man," the man of modern democratic times who settles for comfort and a kind of animal-like contentment and is unwilling to take risks or encounter dangers in pursuit of higher goals of creativity and spirituality. Even in the 1989 article, the account ended on a wistful note:

> The end of history will be a very sad time. The struggle for recognition, the willingness to risk one's life for a purely abstract goal, the worldwide ideological struggle that called forth daring, courage, imagination, and idealism, will be replaced by economic calculation, the endless solving of technical problems, environmental concerns, and the satisfaction of sophisticated consumer demands. In the post-historical period there will be neither art not philosophy, just the perpetual caretaking of the museum of human history.

Dangerously flirting with a theme that threatens to undermine his whole philosophy of history, Fukuyama speculates, "perhaps this very prospect of centuries of boredom at the end of history will serve to get history started once again". Whether or not such a future would mark a return of history in the old sense is not clear. It is certain however that the end of history *à la* Fukuyama will not necessarily bring the full realization of human potential that Hegel and Marx looked forward to: a world where religion and philosophy, art and culture, become the preoccupations of humans freed from the realm of necessity and material concerns. Fukuyama's announcement of the end of history is tinged with melancholy. This is the clearest difference between him and his nineteenth-century predecessors.

Shorn of the more optimistic hopes of the nineteenth-century philosophy of history, Fukuyama has offered nevertheless a self-conscious renewal of that tradition. "By raising once again the question of whether there is such a thing as a Universal History of mankind, I am resuming a discussion that was begun in the early nineteenth century, but more or less abandoned in our time because of the enormity of events that mankind has experienced since then. History, Fukuyama is convinced, does have a direction. Modern times are driven by the immense and in principle irreversible achievements of modern science. This force is the material basis of the "universal homogeneous state" that has emerged worldwide, and that has brought in its train the imperious demand for equality and democracy. It has become clear to everyone that the only viable form of society in our era is that of liberal capitalist democracy. Thus "it makes sense for us once again to speak of a coherent and directional History of mankind that will eventually lead the greater part of humanity to liberal democracy".

THE CLASH OF CIVILIZATIONS: THE REVENGE OF THE PAST?

For Fukuyama, the two possible rival ideologies to liberal democracy were nationalism and religion. Both showed signs of persistence in the contemporary world; both had been in the past the source of great conflicts. Nationalism, argued Fukuyama, was not really in contradiction to liberalism. It expressed the frustration of people who had been denied freedom and autonomy – in other words, the rights that liberalism itself proclaimed. Therefore nationalism "may constitute a source of conflict for liberal societies, this conflict does not arise from liberalism itself so much as from the fact that the liberalism in question is incomplete". The nationalism that was evident in post-Soviet eastern Europe, Central Asia, and other parts of the world, was a transitional phenomenon. It accompanied democratization. Even though it might occasionally take illiberal forms, once the change had been accomplished nationalism would lose its power, as it had largely done in western Europe and other parts of the developed world.

The religious challenge, especially in its fundamentalist form, was a more serious matter. The most important seemed to be the Islamic revival. It is true, Fukuyama conceded, "that Islam constitutes a systematic and coherent ideology, just like liberalism and communism, with its own code of morality and doctrine of political and social justice." Moreover, "the appeal of Islam is potentially universal, reaching out to all men as men, and not just to members of a particular ethnic or national group. And Islam has indeed defeated liberal democracy in many parts of the world, posing a grave threat to liberal practices even in countries where it has not achieved political power directly".

Nevertheless, argued Fukuyama, in the end Islam was no real competitor to liberal democracy. "Despite the power demonstrated by Islam in its current revival, it remains the case that this religion has virtually no appeal outside those areas that were culturally Islamic to begin with. The days of Islam's cultural conquests, it would seem, are over; it can win back lapsed adherents, but has no resonance for young people in Berlin, Tokyo, or Moscow. And while nearly a billion people are culturally Islamic – one-fifth of the world's population – they cannot challenge liberal democracy on its own territory on the level of ideas." Indeed, it was Islam that was under threat from liberalism: "part of the reason for the current, fundamentalist revival is the strength of the perceived threat from liberal, western values to traditional Islamic societies".

A year after Fukuyama's book appeared, *Foreign Affairs* published "The Clash of Civilizations?" by the well-known Harvard political scientist Samuel Huntington. As with Fukuyama, the article provoked great interest and was widely debated; as with Fukuyama, it was duly converted, also with the original question mark removed, into a book, *The Clash of Civilizations and the Remaking of World Order* (1996). The book continued to be debated into the 1990s; but it was undoubtedly the shattering events of September 11, 2001 – the attack on the World Trade Center in New York and the Pentagon in Washington, DC, by radical Islamists – that gave it a new prominence. Many who had initially expressed skepticism towards its main thesis now found themselves, reluctantly and regretfully, in substantial agreement. The media, in America and elsewhere, seized upon the "clash of civilizations" as the key to September 11.

Though his primary purpose was not to counter Fukuyama, part of the impact of Huntington – especially following September 11 – was undoubtedly his resolute rejection of the optimistic picture of the evolving "universal homogeneous state" and the substitution instead of a view of a world riven by deep-seated and fundamental "civilizational" conflicts. A "one-world, universal civilization," more particularly one based on western ideas of liberal democracy, was a dangerous myth. Such a view

> is rooted in the Cold War perspective that the only alternative to communism is liberal democracy, and that the demise of the first produces the universality of the second. Obviously, however, there are many forms of authoritarianism, nationalism, corporatism, and market communism (as in China) that are alive and well in today's world. More significantly, there are all the religious alternatives that lie outside the world of secular ideologies. In the modern world, religion is a central, perhaps *the* central, force that motivates and mobilizes people. . . . The more fundamental divisions of humanity in terms of ethnicity, religions, and civilizations remain and spawn new conflicts. (Huntington 1996).

Huntington sought to restore the old concept of civilization. The conventional division of the world into nation-states was outdated. Enumerating the civilizations that have existed in world history, Huntington admits, is always difficult – Arnold Toynbee variously offered twenty-one and twenty-three, Spengler identified eight major cultures, William McNeil discusses nine. For Huntington, the major contemporary civilizations are Sinic, Japanese, Hindu, Islamic, Orthodox, western and (possibly) African. Between these civilizations lie the major "fault lines" in the contemporary world. The coming conflicts will not be primarily within civilizations but between them. Even five years before 9/11, Huntington viewed the conflict between Islam and the west as the most serious contemporary conflict, but in the longer term he saw the civilizational conflict between China and the west as likely to be the most profound and far-reaching.

The Middle East specialist Bernard Lewis seems to have provided Huntington with the phrase "the clash of civilizations," as well as identifying its current phase with "the historic reaction of an ancient rival against our Judeo-Christian heritage". But more fundamentally what Huntington was doing, as his references to Spengler, Toynbee and others made clear, was to revive the philosophy of history as practiced by these thinkers. For him, as for them, the basic unit of history is the civilization and there is no single, unilinear, direction for human history. Rather the human story is the Gibbonesque one of rise and fall, of civilizations reaching a period of efflorescence and dominance only to yield to more energetic and creative civilizations on their flanks. The pattern of history, if there is one, is cyclical rather than unilineal. Moreover, far from there being a convergence on a single, unified, global civilization, there persist division and divergence between civilizational units with their own characteristic cultures.

Unlike Fukuyama, who makes his debt to Hegel explicit, Huntington does not link himself directly with any philosopher or philosophy of history. However, it is not difficult to discern a distinct affinity with Arnold Toynbee, and behind him, Oswald Spengler. There is also a striking parallel in the way both Huntington and Toynbee consider the decline of civilizations. Toynbee had argued, in his *A Study of History* (1934-61), that a declining civilization in its final stages throws up a "universal state" and proclaims that history has ended. Civilization achieves a kind of stasis; no further fundamental change is to be expected. The civilization offers itself as an exemplar and a model to the rest of the world. The Roman Empire, in this as in other aspects of civilizational development, was for Toynbee the clearest expression of this tendency, but he found it also in such instances as the Arab, Ottoman and Chinese empires. Huntington does not have such a rigidly schematic approach to the rise and fall of civilizations. But he quotes Toynbee on "the mirage of immortality" that blinds the people of a civilization as they enter the stage of the "universal state." They are convinced that "theirs is the final form of human society," but "societies that assume their history has ended . . . are usually societies whose history is about to decline".

Does that mean that western civilization is in decline? Huntington is not sure. "The development of the west to date has not deviated significantly from the evolutionary patterns common to civilizations throughout history. The Islamic Resurgence and the economic dynamism of Asia demonstrate that other civilizations are alive and well and at least potentially threatening to the west." But, says Huntington, in history nothing is inevitable. If the west builds upon its common legacy – which crucially means that Europe and America work together and reaffirm their western identity against the weakening forces of "multiculturalism" – it can overcome its internal weaknesses. It must however give up its "false," "immoral" and "dangerous" belief in the universality of western culture. We live inescapably in a "multicivilizational, multipolar world." The only prudent attitude, the only one that can stave off "global civilizational war," is one of the mutual acceptances of differences and a mutual commitment to negotiating them by peaceful means. Beyond that, "peoples in all civilizations should search for and attempt to expand the values, institutions, and practices they have in common with peoples of other civilizations."

It has been argued that "the clash of civilizations" is one of the political myths with which contemporary world politics is strewn, a myth especially that aims to demonize Islamic civilization. Since the views of Fukuyama and Huntington stand more or less diametrically opposed to one another, no-one can argue that there is some sort of "hegemonic" western discourse at work here. Yet, for all their differences, Fukuyama and Huntington have re-launched the inquiry into long-term and large-scale historical change that characterized the traditional philosophy of history at the end of the Cold War. With the end of the Cold War, much that had remained bottled up came tumbling out – nationalism, religion, racism and other things that had been thought to have been banished by modernity. One of those things was history – too much of it, in the eyes of many. Ironically, even the proclamation of "the end of history" was a tribute to the idea that, for many people to whom it had been denied, history was once more something to be made, or at least to be part of. It became once more fitting and perhaps even necessary to ask the philosophical questions about our place in history and our destiny.

FRANCIS FUKUYAMA

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Rather like E. H. Carr's *The Twenty Years' Crisis* (1945), Francis Fukuyama's book *The End of History and the Last Man* (1992) provided an interpretation of the significance of the end of the Cold War that captured an enormous amount of public attention. Almost overnight, the phrase "end of history" was used as a synonym for the "post-Cold War era" and Fukuyama, hitherto almost unknown among students of international relations, became an instant intellectual celebrity. In a sense, this was unfortunate. Fukuyama did not say that "history" had come to an end in the sense that politics, war and conflict would no longer take place, nor did he argue that the collapse of communism would guarantee that all states would become liberal democracies. The subtleties of his argument, an ingenious blend of political philosophy, historical analysis and tentative futurology, can only be gleaned from a careful reading of the text, something that too many commentators have neglected to do.

Ironically, however, once one abandons some of the more simplistic interpretations of Fukuyama's argument, it remains unclear why the book did attract so much attention in the last decade of the twentieth century. The most interesting aspects of the book, in my view, were the ones least commented on, having to do with the characteristics of "the last man" rather than the "end of history" *per se*. Again, those who have focused on the first part of the book have downplayed these aspects. Only if one grasps the underlying pessimism of Fukuyama's argument is it possible to avoid the temptation to celebrate or condemn him on the erroneous assumption that his book is merely an exercise in liberal "triumphalism" at the end of the Cold War.

By the phrase "end of history", Fukuyama's argument is that the combination of liberal democracy and capitalism has proved superior to any alternative political/economic system, and the reason lies in its ability to satisfy the basic drives of human nature. The latter is composed of two fundamental desires: one is the desire for material goods and wealth and the other (more fundamental) desire is for recognition of our worth as human beings by those around us. Capitalism is the best economic system for maximizing the production of goods and services and for exploiting scientific technology to generate wealth. However, economic grow is only part of the story. Fukuyama appeals to Hegel's concept of recognition to account for the superiority of liberal democracy over its rivals in the political arena. While economic growth can be promoted under a variety of political regimes, including fascist ones, only liberal democracies can meet the fundamental human need for recognition, political freedom and equality. It was Hegel who contended that the end of history would arrive when humans had achieved the kind of civilization that satisfied their fundamental longings. For Hegel, that end point was the constitutional state. In his version, Hegel appointed Napoleon as the harbinger of the end of history at the beginning of the nineteenth century. Fukuyama argues that we need to recover the philosophical idealism of Hegel and abandon the philosophical materialism of Marx and his followers, who believed that socialism was necessary to overcome the economic inequality of capitalist societies. Fukuyama also finds in Hegel a more profound understanding of human nature than can be gleaned from the ideas of such philosophers as Thomas Hobbes and John Locke, who privileged self-preservation above recognition.

In addition to Hegel, Fukuyama invokes Plato. From Plato, Fukuyama borrows the notion of *thymos*, variously translated as "spiritedness" or "desire". *Megalothymia* is the *thymos* of great men, the great movers of history such as Caesar and Stalin. In contrast, *isothymia* is the humble demand for recognition in the form of equality rather than superiority.

History is a struggle between these thymotic passions. The genius of capitalist liberal democracy is its ability to reconcile the thymotic passions. Shadia Drury sums up Fukuyama's argument as follows:

> Liberalism pacifies and de-politicizes the aristocratic world of mastery by turning politics into economics. Liberalism pacifies the masterful *thymos* of the first man and replaces it with the slavish *thymos* of the last man. Instead of superiority and dominance, society strives for equality. Those who still long for dominance have the capitalist pursuit of wealth as their outlet.

Fukuyama also relies on the interpretation of Hegel by Alexandre Kojève, the Russian exile and political philosopher. In a series of lectures delivered in Paris in the 1940s, Kojève argued that the welfare state had solved the problems of capitalism identified by Marx. Thus, capitalism has managed to suppress its own internal contradictions. Furthermore, it not only provides material prosperity, but also homogenizes ideas and values, thus undermining the clash of ideology between states, thereby reducing the threat of war. Hegel did not believe that the end of war within states could be replicated at the international level. Kojève and Fukuyama argue that while wars will not disappear, the homogenization of values among the great powers will promote peace among the most powerful states, and these are the ones that matter in a long-term historical perspective.

Despite the victory of liberal democracy as a normative model over its rivals, Fukuyama is concerned that the subordination of *megalothymia* to *isothymia* may mean the pursuit of equality at the expense of the pursuit of excellence. If there is too much equality, and no great issues to struggle for, people may revolt at the very system that has brought them peace and security. Unless there are ways to express *megalothymia* in those societies lucky enough to have reached the "end of history" (and according to his own statistics, less than one-third of all states have arrived thus far), liberal democracy may atrophy and die. At one point Fukuyama argues that perhaps Japan may offer an alternative to American liberal democracy and combine a successful economy with social bonds strong enough to withstand the fragmentary forces of liberal democracy. Many Asian societies, he claims, have "paid lip service to Western principles of liberal democracy, accepting the form while modifying the content to accommodate Asian cultural traditions".

It is important to note some of the main criticisms levelled at *The End of History*. First, Fukuyama's appeal to Hegel and Plato has been called into question by some commentators, like Shadia Drury, who points out that it is not possible to "[reconcile] Plato's objectivist views with [an] intersubjective concept of recognition". She argues that Fukuyama's invocation of Plato is designed to avoid the awkward fact that Hegel himself never predicted that history would end, even in the sense that Fukuyama uses the term "end". Nor could Hegel do so, given his commitment to the idea that history is inherently dialectical. John O'Neill makes a similar criticism. According to O'Neill, Hegel argued that "recognition cannot be its own end since it is parasitic on other goods" that provide the appropriate criteria for recognition. This is why Hegel ultimately rejects an individualized market economy as satisfactory as a means of recognition. It is unclear, therefore, how Fukuyama can coherently use Hegel to defend capitalism and liberal democracy when Hegel explicitly denied that such a combination could adequately achieve the goal of recognition. For all his criticisms of Hobbes and Locke, Fukuyama fails to make a sufficient break with their atomistic conceptions of human nature.

Criticism has been levelled at Fukuyama's empirical claims regarding the spread of liberal democracy around the globe. On the one hand, Fukuyama defines liberal democracies in somewhat vague, formal terms: a liberal democracy is one whose constitution respects some basic political rights and requires the government to rule on the basis of explicit consent from its citizens through regular competitive and fair elections. While a broad definition facilitates some rough measurement of the "march of democracy", such a crude indicator is hardly adequate for any firm conclusions to be made about the extent of freedom in the contemporary world. The term itself becomes less clear now that there are, in his view, no alternatives against which to define it. There is simply no analysis of the enormous differences in the way the states that he lumps together manage the tensions between freedom and equality in politics and economics.

Finally, there are problems with Fukuyama's presumption that political and economic liberalism – the twin engines of his unidirectional historical motor – can coexist comfortably within the territorial boundaries of the sovereign state. By contrast, much of the literature on the post-Cold War era is concerned with the contradictory dynamics of "globalization" versus "fragmentation" – of which ethnic nationalism is a prime example. Globalization is a blanket term that conveys the limits to state power arising from the myriad dynamics of a global economy in which the state seems to be relatively powerless to manage its domestic economy. In particular, the integration of global capital tends to subordinate domestic politics to the demand for efficiency and competitiveness on a global playing field that is anything but level. Consequently, as governments become less accountable to those they claim to represent over a broader range of issues, so the spectrum of democratic choice before citizens narrows considerably. To the extent that economic globalization and political fragmentation are operating at different levels of social, political and economic organization, one could plausibly accept much of Fukuyama's philosophical assumptions and reach opposite conclusions to the ones that he draws. On the reasonable assumption that global capitalism is exacerbating economic inequality both within and between states while simultaneously denying them a redistributive capacity to moderate its impact, the "struggle for recognition" may take reactive forms such as ethnic nationalism. It is not clear how this problem can be solved merely by appealing to the virtues of capitalism and liberal democracy, since the main difficulty lies in striking the right balance between them, an issue that Fukuyama does not deal with in his book.

The End of History and the Last Man (Introduction)

Francis Fukuyama

The distant origins of the present volume lie in an article entitled "The End of History?" which I wrote for *The National Interest* in the summer of 1989. In it, I argued that a remarkable consensus concerning the legitimacy of liberal democracy as a system of government had emerged throughout the world over the past few years, as it conquered rival ideologies like hereditary monarchy, fascism, and most recently communism. More than that, however, I argued that liberal democracy may constitute the "end point of mankind's ideological evolution" and the "final form of human government," and as such constituted the "end of history." That is, while earlier forms of government were characterized by grave defects and irrationalities that led to their eventual collapse, liberal democracy was arguably free from such fundamental internal contradictions. This was not to say that today's stable democracies, like the United States, France or Switzerland, were not without injustice or serious social problems. But these problems were ones of incomplete implementation of the twin principles of liberty and equality on which modern democracy is founded, rather than of flaws in the principles themselves. While some present-day countries might fail to achieve stable liberal democracy, and others might lapse back into other, more primitive forms of rule like theocracy or military dictatorship, the *ideal* of liberal democracy could not be improved on.

The original article excited an extraordinary amount of commentary and controversy. Criticism took every conceivable form, some of it based on simple misunderstanding of my original intent, and others penetrating more perceptively to the core of my argument. Many people were confused in the first instance by my use of the word "history." Understanding history in a conventional sense as the occurrence of events, people pointed to the fall of the Berlin Wall or the Chinese communist crackdown in Tiananmen Square as evidence that "history was continuing". And yet what I suggested had come to an end was not the occurrence of events but History: that is, history understood as a single, coherent, evolutionary process, when taking into account the experience of all peoples in all times.

This understanding of History was most closely associated with the German philosopher G. W. F. Hegel. It was made part of our daily intellectual atmosphere by Karl Marx, who borrowed this concept of History from Hegel, and is implicit in our use of words like "primitive" or "advanced," "traditional" or "modern," when referring to different types of human societies. For both of these thinkers, there was a coherent development of human societies from simple tribal ones based on slavery and subsistence agriculture, through various theocracies, monarchies, and feudal aristocracies, up through modern liberal democracy and technologically driven capitalism. This evolutionary process was neither random nor unintelligible, even if it did not proceed in a straight line, and even if it was possible to question whether man was happier or better off as a result of historical "progress." Both Hegel and Marx believed that the evolution of human societies was not open-ended, but would end when mankind had achieved a form of society that satisfied its deepest and most fundamental longings. Both thinkers thus posited an "end of history": for Hegel this was the liberal state, while for Marx it was a communist society. It meant that there would be no further progress in the development of underlying principles and institutions, because all of the really big questions had been settled.

The present book, while informed by recent world events, returns to a very old question: whether, at the end of the twentieth century, it makes sense for us once again to speak of a coherent and directional History of mankind that will eventually lead the greater part of humanity to liberal democracy? The answer I arrive at is yes, for two separate reasons. One has to do with economics, and the other has to do with what is termed the "struggle for recognition." It is of course not sufficient to appeal to the authority of Hegel, Marx or any of their contemporary followers to establish the validity of a directional History. In the century and a half since they wrote, their intellectual legacy has been relentlessly assaulted from all directions. The most profound thinkers of the twentieth century have directly attacked the idea that history is a coherent or intelligible process; indeed, they have denied the possibility that any aspect of human life is philosophically intelligible. We in the West have become thoroughly pessimistic with regard to the possibility of overall progress in democratic institutions. This profound pessimism is not accidental, but born of the truly terrible political events of the first half of the twentieth century – two destructive world wars, the rise of totalitarian ideologies, and the turning of science against man in the form of nuclear weapons and environmental damage. The life experiences of the victims of this past century's political violence – from the survivors of Hitlerism and Stalinism to the victims of Pol Pot – would deny that there has been such a thing as historical progress. Indeed, we have become so accustomed by now to expect that the future will contain bad news with respect to the health and security of decent, liberal, democratic political practices that we have problems recognizing good news when it comes.

And yet, good news has come. The most remarkable development of the last quarter of the twentieth century has been the revelation of enormous weaknesses at the core of the world's seemingly strong dictatorships, whether they be of the military authoritarian right, or the communist-totalitarian left. From Latin America to Eastern Europe, from the Soviet Union to the Middle East and Asia, strong governments have been failing over the last two decades. And while they have not given way in all cases to stable liberal democracies, liberal democracy remains the only coherent political aspiration that spans different regions and cultures around the globe. In addition, liberal principles in economics – the "free market" – have spread, and have succeeded in producing unprecedented levels of material prosperity, both in industrially developed countries and in countries that had been, at the close of World War II, part of the impoverished Third World. A liberal revolution in economic thinking has sometimes preceded, sometimes followed, the move toward political freedom around the globe.

All of these developments, so much at odds with the terrible history of the first half of the century when totalitarian governments of the right and left were on the march, suggest the need to look again at the question of whether there is some deeper connecting thread underlying them, or whether they are merely accidental instances of good luck. By raising once again the question of whether there is such a thing as a Universal History of mankind, I am resuming a discussion that was begun in the early nineteenth century, but more or less abandoned in our time because of the enormity of events that mankind has experienced since then. While drawing on the ideas of philosophers like Kant and Hegel who have addressed this question before, I hope that the arguments presented here will stand on their own.

After establishing why we need to raise once again the possibility of Universal History, I propose an initial answer by attempting to use modern natural science as a mechanism to explain the directionality and coherence of History. The unfolding of modern natural science has had a uniform effect on all societies that have experienced it. By establishing a uniform horizon of economic production possibilities, technology makes possible the limitless accumulation of wealth, and thus the satisfaction of an ever-expanding set of human desires. This process guarantees an increasing homogenization of all countries undergoing economic modernization. They must unify nationally on the basis of a centralized state, urbanize, replace traditional forms of social organization like tribe, sect and family with economically rational ones based on function and efficiency, and provide for the universal education of their citizens. Such societies have become increasingly linked with one another through global markets and the spread of a universal consumer culture. The logic of modern natural science would seem to dictate a universal evolution in the direction of capitalism. The experiences of the Soviet Union, China and other socialist countries indicate that while highly centralized economies are sufficient to reach the level of industrialization represented by Europe in the 1950s, they are woefully inadequate in creating what have been termed complex "post-industrial" economies in which information and technological innovation play a much larger role.

But while the historical mechanism of modern natural science is sufficient to explain the growing uniformity of modern societies, it is not sufficient to account for the phenomenon of democracy. There is no question but that the world's most developed countries are also its most successful democracies. But there is no economically necessary reason why advanced industrialization should produce political liberty. Stable democracy has at times emerged in pre-industrial societies, as it did in the United States in 1776. On the other hand, there are many historical and contemporary examples of technologically advanced capitalism coexisting with political authoritarianism, from Meiji Japan and Bismarckian Germany to present-day Singapore and Thailand. In many cases, authoritarian states are capable of producing rates of economic growth unachievable in democratic societies. What we have called the logic of modern natural science is in effect an economic interpretation of historical change, but economic interpretations of history are incomplete and unsatisfying, because man is not simply an economic animal. In particular, such interpretations cannot really explain why we are democrats, that is, proponents of the principle of popular sovereignty and the guarantee of basic rights under a rule of law.

To do this, we turn to Hegel's non-materialist account of History, based on the "struggle for recognition." According to Hegel, human beings, like animals, have natural needs such as food, drink, shelter and above all the preservation of their own bodies. Man differs fundamentally from the animals, however, because he wants to be "recognized." In particular, he wants to be recognized as a *human being,* that is, as a being with dignity. According to Hegel, the relationship of lordship and bondage, which took a wide variety of forms in all of the unequal, aristocratic societies that have characterized the greater part of human history, failed ultimately to satisfy the desire for recognition. The "contradiction" inherent in the relationship of lordship and bondage was finally overcome as a result of the French and, one would have to add, American revolutions. These democratic revolutions abolished the distinction between master and slave by making the former slaves their own masters and by establishing the principles of popular sovereignty and the rule of law. The inherently unequal recognition of masters and slaves is replaced by universal and reciprocal recognition, where every citizen recognizes the dignity and humanity of every other citizen, and where that dignity is recognized in turn by the state through the granting of *rights.*

This Hegelian understanding of the meaning of contemporary liberal democracy differs in a significant way from the Anglo-Saxon understanding that was the theoretical basis of liberalism in countries like Britain and the United States. In that tradition, the prideful quest for recognition was to be subordinated to enlightened self-interest. While Hobbes, Locke, and the American Founding Fathers like Jefferson and Madison believed that rights to a large extent existed as a means of preserving a private sphere where men can enrich themselves and satisfy the desiring parts of their souls, Hegel saw rights as ends in themselves, because what truly satisfies human beings is not so much material prosperity as recognition of their status and dignity. With the

American and French revolutions, Hegel asserted that history comes to an end because the longing that had driven the historical process – the struggle for recognition – has now been satisfied in a society characterized by universal and reciprocal recognition. No other arrangement of human social institutions is better able to satisfy this longing, and hence no further progressive historical change is possible.

The desire for recognition, then, can provide the missing link between liberal economics and liberal politics. As standards of living increase, as populations become more cosmopolitan and better educated, and as society as a whole achieves a greater equality of condition, people begin to demand not simply more wealth but recognition of their status. If people were nothing more than desire and reason, they would be content to live in market-oriented authoritarian states like Franco's Spain, or a South Korea or Brazil under military rule, but they also have a pride in their own self-worth, and this leads them to demand democratic governments that treat them like adults rather than children. Communism is being superseded by liberal democracy in our time because of the realization that the former provides a gravely defective form of recognition.

An understanding of the importance of the desire for recognition as the motor of history allows us to reinterpret many phenomena that are otherwise seemingly familiar to us. A religious believer, for example, seeks recognition for his particular gods or sacred practices, while a nationalist demands recognition for his particular linguistic, cultural or ethnic group. Both of these forms of recognition are less rational than the universal recognition of the liberal state, because they are based on arbitrary distinctions between sacred and profane, or between human social groups. For this reason, religion, nationalism and a people's complex of habits and customs ("culture") have traditionally been interpreted as obstacles to the establishment of successful democratic political institutions and free-market economies. But the success of liberal politics and economics frequently rests on irrational forms of recognition that liberalism was supposed to overcome. For democracy to work, citizens need to develop an irrational pride in their own democratic institutions, and must also develop what Tocqueville called the "art of associating," which rests on prideful attachment to small communities. These communities are frequently based on religion, ethnicity or other forms of recognition that fall short of the universal recognition on which the liberal state is based.

The struggle for recognition also provides us with insight into the nature of international politics. The relationship of lordship and bondage on a domestic level is naturally replicated on the level of states, where nations as a whole seek recognition and enter into bloody battles for supremacy. Nationalism, a modern yet not-fully-rational form of recognition, has been the vehicle for the struggle for recognition over the past hundred years, and the source of this century's most intense conflicts. But if war is fundamentally driven by the desire for recognition, it stands to reason that the liberal revolution that abolishes the relationship of lordship and bondage by making former slaves their own masters should have a similar effect on the relationship between states. Liberal democracy replaces the irrational desire to be recognized as greater than others with a rational desire to be recognized as equal. A world made up of liberal democracies, then, should have much less incentive for war, since all nations would reciprocally recognize one another's legitimacy. And indeed, there is substantial empirical evidence that liberal democracies do not behave imperialistically toward one another, even if they are perfectly capable of going to war with states that are not democracies and do not share their fundamental values.

The final part of this argument addresses the question of the "end of history," and the creature who emerges at the end, the "last man." Assuming that liberal democracy is, for the moment, safe from external enemies, could we assume that successful democratic societies could remain that way indefinitely? Or is liberal democracy prey to serious internal contradictions? Hegel's great interpreter, Alexandre Kojève, asserted intransigently that history had ended because what he called the "universal and homogeneous state" – what we can understand as liberal democracy – definitely solved the question of recognition by replacing the relationship of lordship and bondage with universal and equal recognition. Recognition is the central problem of politics because it is the origin of tyranny, imperialism and the desire to dominate. But while it has a dark side, it is simultaneously the psychological ground for political virtues like courage, public spiritedness and justice.

But is the recognition available to citizens of contemporary liberal democracies "completely satisfying?" The long-term future of liberal democracy depends above all on the answer to this question, from the left and the right, respectively. The left would say that universal recognition in liberal democracy is necessarily incomplete because capitalism creates economic inequality and requires a division of labor that *ipso facto* implies unequal recognition. In this respect, a nation's absolute level of prosperity provides no solution, because there will continue to be those who are relatively poor and therefore invisible as human beings to their fellow citizens. Liberal democracy, in other words, continues to recognize equal people unequally. The second, and in my view more powerful, criticism of universal recognition comes from the political right that was profoundly concerned with the leveling effects of the French Revolution's commitment to human equality. This political right found its most brilliant spokesman in the philosopher Friedrich Nietzsche. Nietzsche believed that modern democracy represented not the self-mastery of former slaves, but the unconditional victory of the slave. The typical citizen of a liberal democracy was a "last man" who, schooled by the founders of modern liberalism, gave up prideful belief in his or her own superior worth in favor of comfortable self-preservation, clever only at finding new ways to satisfy a host of petty wants through the calculation of long-term self-interest. The last man had no desire to be recognized as greater than others, and without such desire no excellence was possible. Content with his happiness, the last man ceased to be human.

Following Nietzsche's line of thought, we are compelled to ask: Is not the man who is completely satisfied by nothing more than universal and equal recognition something less than a full human being, a "last man" with neither striving nor aspiration? Is there not a side of the human personality that deliberately seeks out struggle, danger, risk and daring, and will this side not remain unfulfilled by the "peace and prosperity" of contemporary liberal democracy? Does not the satisfaction of certain human beings depend on recognition that is inherently unequal? Indeed, does not the desire for unequal recognition constitute the basis of a livable life, not just for bygone aristocratic societies, but also in modern liberal democracies? Will not their future survival depend, to some extent, on the degree to which their citizens seek to be recognized not just as equal, but as superior to others?

These questions arise naturally once we ask whether there is such a thing as progress, and whether we can construct a coherent and directional Universal History of mankind. Totalitarianisms of the right and left have kept us too busy to consider the question seriously for the better part of this century. But the fading of these totalitarianisms, as the century comes to an end, invites us to raise this old question one more time.

The Clash of Civilizations?

Samuel Huntington

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THE NEXT PATTERN OF CONFLICT

World politics is entering a new phase, and intellectuals have not hesitated to proliferate visions of what it will be – the end of history, the return of traditional rivalries between nation states, and the decline of the nation state from the conflicting pulls of tribalism and globalism, among others. Each of these visions catches aspects of the emerging reality. Yet they all miss a crucial, indeed a central, aspect of what global politics is likely to be in the coming years. It is my hypothesis that the fundamental source of conflict in this new world will not be primarily ideological or primarily economic. The great divisions among humankind and the dominating source of conflict will be cultural. Nation states will remain the most powerful actors in world affairs, but the principal conflicts of global politics will occur between nations and groups of different civilizations. The clash of civilizations will be the battle lines of the future.

Conflict between civilizations will be the latest phase of the evolution of conflict in the modern world. For a century and a half after the emergence of the modern international system of the Peace of Westphalia, the conflicts of the Western world were largely among princes – emperors, absolute monarchs and constitutional monarchs attempting to expand their bureaucracies, their armies, their mercantilist economic strength and, most important, the territory they ruled. In the process they created nation states, and beginning with the French Revolution the principal lines of conflict were between nations rather than princes. In 1793, as R. R. Palmer put it, "The wars of kings were over; the wars of peoples had begun." This nineteenth-century pattern lasted until the end of World War I. Then, as a result of the Russian Revolution and the reaction against it, the conflict of nations yielded to the conflict of ideologies, first among communism, fascism-Nazism and liberal democracy, and then between communism and liberal democracy. During the Cold War, this latter conflict became embodied in the struggle between the two superpowers, neither of which was a nation-state in the classical European sense and each of which defined its identity in terms of ideology.

These conflicts between princes, nation states and ideologies were primarily conflicts within Western civilization, "Western civil wars," as William Lind labeled them. This was as true of the Cold War as it was of the world wars and the earlier wars of the seventeenth, eighteenth and nineteenth centuries. With the end of the Cold War, international politics moves out of its Western phase, and its center-piece becomes the interaction between the West and non-Western civilizations and among non-Western civilizations. In the politics of civilizations, the people and governments of non-Western civilizations no longer remain the objects of history as targets of Western colonialism but join the West as movers and shapers of history.

THE NATURE OF CIVILIZATIONS

During the Cold War the world was divided into the First, Second and Third Worlds. Those divisions are no longer relevant. It is far more meaningful now to group countries not in terms of their political or economic systems or in terms of their level of economic development but rather in terms of their culture and civilization.

What do we mean when we talk of a civilization? A civilization is a cultural entity. Villages, regions, ethnic groups, nationalities, religious groups, all have distinct cultures at different levels of cultural heterogeneity. The culture of a village in southern Italy may be different from that of a village in northern Italy, but both will share in a common Italian culture that distinguishes them from German villages. European communities, in turn, will share cultural features that distinguish them from Arab or Chinese communities. Arabs, Chinese and Westerners, however, are not part of any broader cultural entity. They constitute civilizations. A civilization is thus the highest cultural grouping of people and the broadest level of cultural identity people have short of that which distinguishes humans from other species. It is defined both by common objective elements, such as language, history, religion, customs, institutions, and by the subjective self-identification of people. People have levels of identity: a resident of Rome may define himself with varying degrees of intensity as a Roman, an Italian, a Catholic, a Christian, a European, a Westerner. The civilization to which he belongs is the broadest level of identification with which he intensely identifies. People can and do redefine their identities and, as a result, the composition and boundaries of civilizations change.

Civilizations may involve a large number of people, as with China ("a civilization pretending to be a state," as Lucian Pye put it), or a very small number of people, such as the Anglophone Caribbean. A civilization may include several nation states, as is the case with Western, Latin American and Arab civilizations, or only one, as is the case with Japanese civilization. Civilizations obviously blend and overlap, and may include sub-civilizations. Western civilization has two major variants, European and North American, and Islam has its Arab, Turkic and Malay subdivisions. Civilizations are nonetheless meaningful entities, and while the lines between them are seldom sharp, they are real. Civilizations are dynamic; they rise and fall; they divide and merge. And, as any student of history knows, civilizations disappear and are buried in the sands of time.

Westerners tend to think of nation states as the principal actors in global affairs. They have been that, however, for only a few centuries. The broader reaches of human history have been the history of civilizations. In *A Study of History*, Arnold Toynbee identified 21 major civilizations; only six of them exist in the contemporary world.

WHY CIVILIZATIONS WILL CLASH

Civilization identity will be increasingly important in the future, and the world will be shaped in large measure by the interactions among seven or eight major civilizations. These include Western, Confucian, Japanese, Islamic, Hindu, Slavic-Orthodox, Latin American and possibly African civilization. The most important conflicts of the future will occur along the cultural fault lines separating these civilizations from one another. Why will this be the case?

First, differences among civilizations are not only real; they are basic. Civilizations are differentiated from each other by history, language, culture, tradition and, most important, religion. The people of different civilizations have different views on the relations between God and man, the citizen and the state, husband and wife, as well as differing views of the relative importance of rights and responsibilities, liberty and authority, equality and hierarchy. These differences are the product of centuries. They will not soon disappear. They are far more fundamental than differences among political ideologies and political regimes. Differences do not necessarily mean conflict, and conflict does not necessarily mean violence. Over the centuries, however, differences among civilizations have generated the most prolonged and the most violent conflicts.

Second, the world is becoming a smaller place. The interactions between peoples of different civilizations are increasing; these increasing interactions intensify civilization consciousness and awareness of differences between civilizations and commonalities within civilizations. North African immigration to France generates hostility among Frenchmen and at the same time increased receptivity to immigration by "good" European Catholic Poles. Americans react far more negatively to Japanese investment than to larger investments from Canada and European countries. Similarly, as Donald Horowitz has pointed out, "An Ibo may be . . . an Owerri Ibo or an Onitsha Ibo in what was the Eastern region of Nigeria. In Lagos, he is simply an Ibo. In London, he is a Nigerian. In New York, he is an African." The interactions among peoples of different civilizations enhance the civilization-consciousness of people that, in turn, invigorates differences and animosities stretching or thought to stretch back deep into history.

Third, the processes of economic modernization and social change throughout the world are separating people from longstanding local identities. They also weaken the nation state as a source of identity. In much of the world religion has moved in to fill this gap, often in the form of movements that are labeled "fundamentalist." Such movements are found in Western Christianity, Judaism, Buddhism and Hinduism, as well as in Islam. In most countries and most religions the people active in fundamentalist movements are young, college-educated, middle-class technicians, professionals and business persons. The "unsecularization of the world," George Weigel has remarked, "is one of the dominant social factors of life in the late twentieth century" and provides a basis for identity and commitment that transcends national boundaries and unites civilizations.

Fourth, the growth of civilization-consciousness is enhanced by the dual role of the West. On the one hand, the West is at a peak of power. At the same time, however, and perhaps as a result, a return to the roots phenomenon is occurring among non-Western civilizations. Increasingly one hears references to trends toward a turning inward and "Asianization" in Japan, the end of the Nehru legacy and the "Hinduization" of India, the failure of Western ideas of socialism and nationalism and hence "re-Islamization" of the Middle East, and now a debate over Westernization versus Russianization in Boris Yeltsin's country. A de-Westernization and indigenization of elites is occurring in many non-Western countries. A West at the peak of its power confronts non-Wests that increasingly have the desire, the will and the resources to shape the world in non-Western ways.

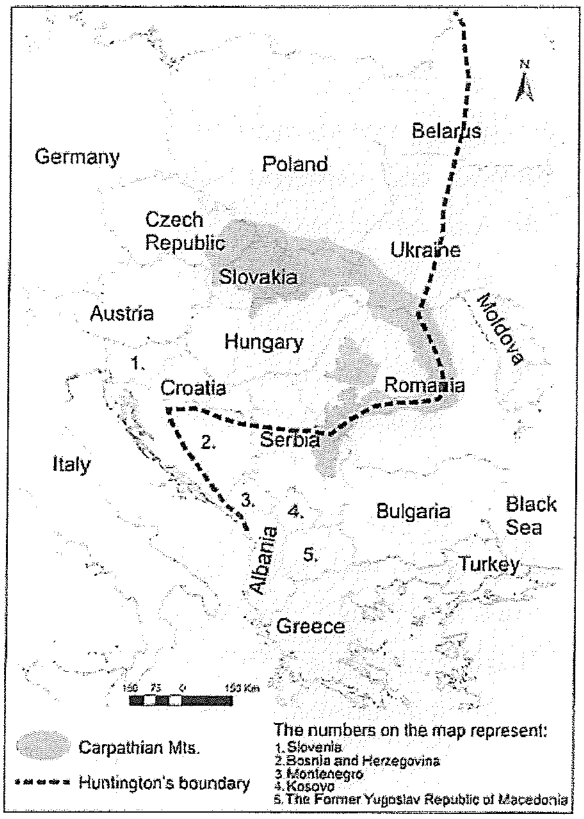
Fifth, cultural characteristics and differences are less mutable and hence less easily compromised and resolved than political and economic ones. In the former Soviet Union, communists can become democrats, the rich can become poor and the poor rich, but Russians cannot become Estonians and Azeris cannot become Armenians. Even more than ethnicity, religion discriminates sharply and exclusively among people. A person can be half-French and half-Arab and simultaneously even a citizen of two countries. It is more difficult to be half-Catholic and half-Muslim.

Finally, economic regionalism is increasing. The proportions of total trade that are intraregional rose between 1980 and 1989. The importance of regional economic blocs is likely to continue to increase in the future. Economic regionalism may succeed only when it is rooted in a common civilization. The European Community rests on the shared foundation of European culture and Western Christianity. The success of the North American Free Trade Area depends on the convergence now underway of Mexican, Canadian and American cultures. Japan, in contrast, faces difficulties in creating a comparable economic entity in East Asia because Japan is a society and civilization unique to itself. However strong the trade and investment links Japan may develop with other East Asian countries, its cultural differences with those countries inhibit and perhaps preclude its promoting regional economic integration like that in Europe and North America. Common culture, in contrast, is clearly facilitating the rapid expansion of the economic relations between the People's Republic of China and Hong Kong, Taiwan, Singapore and the overseas Chinese communities in other Asian countries. With the Cold War over, cultural commonalities increasingly overcome ideological differences, and mainland China and Taiwan move closer together. The principal East Asian economic bloc of the future is likely to be centered on China. This bloc is, in fact, already coming into existence.

The end of ideologically defined states in Eastern Europe and the former Soviet Union permits traditional ethnic identities and animosities to come to the fore. Decreasingly able to mobilize support and form coalitions on the basis of ideology, governments and groups will increasingly attempt to mobilize support by appealing to common religion and civilization identity. The clash of civilizations thus occurs at two levels: at the micro-level, adjacent groups along the fault lines between civilizations struggle, often violently, over the control of territory and each other; at the macro-level, states from different civilizations compete for relative military and economic power, struggle over the control of international institutions and third parties, and competitively promote their particular political and religious values.

THE FAULT LINES BETWEEN CIVILIZATIONS

The fault lines between civilizations are replacing the political and ideological boundaries of the Cold War as the flash points for crisis and bloodshed. As the ideological division of Europe has disappeared, the cultural division of Europe between Western Christianity, on the one hand, and Orthodox Christianity and Islam, on the other, has reemerged. The most significant dividing line in Europe may well be the eastern boundary of Western Christianity in the year 1500. This line runs along what are now the boundaries between Finland and Russia and between the Baltic states and Russia, cuts through Belarus and Ukraine separating the more Catholic western Ukraine from Orthodox eastern Ukraine, swings westward separating Transylvania from the rest of Romania, and then goes through Yugoslavia almost exactly along the line now separating Croatia and



Slovenia from the rest of Yugoslavia. In the Balkans this line, of course, coincides with the historic boundary between the Hapsburg and Ottoman empires. The peoples to the north and west of this line are Protestant or Catholic; they shared the common experiences of European history – feudalism, the Renaissance, the Reformation, the Enlightenment, the French Revolution, the Industrial Revolution; they are generally economically better off than the peoples to the east. The peoples to the east and south of this line are Orthodox or Muslim; they historically belonged to the Ottoman or Tsarist empires and were only lightly touched by the shaping events in the rest of Europe; they are generally less advanced economically; they seem much less likely to develop stable democratic political systems. The Velvet Curtain of culture has replaced the Iron Curtain of ideology as the most significant dividing line in Europe.

Conflict along the fault line between Western and Islamic civilizations has been going on for 1,300 years. After the founding of Islam, the Arab and Moorish surge west and north only ended at Tours in 732. From the eleventh to the thirteenth century the Crusaders attempted with temporary success to bring Christianity and Christian rule to the Holy Land. From the fourteenth to the seventeenth century, the Ottoman Turks reversed the balance, extended their sway over the Middle East and the Balkans, captured Constantinople, and twice laid siege to Vienna. In the nineteenth and early twentieth centuries as Ottoman power declined Britain, France and Italy established Western control over most of North Africa and the Middle East. After World War II, the West, in turn, began to retreat; the colonial empires disappeared; first Arab nationalism and then Islamic fundamentalism manifested themselves. This centuries-old military interaction between the West and Islam is unlikely to decline. Many Arab countries, in addition to the oil exporters, are reaching levels of economic and social development where autocratic forms of government become inappropriate and efforts to introduce democracy become stronger. Some openings in Arab political systems have already occurred. The principal beneficiaries of these openings have been Islamist movements. In the Arab world, in short, Western democracy strengthens anti-Western political forces.

On both sides the interaction between Islam and the West is seen as a clash of civilizations. The West's "next confrontation," observes M. J. Akbar, an Indian Muslim author, "is definitely going to come from the Muslim world. It is in the sweep of the Islamic nations from the Meghreb to Pakistan that the struggle for a new world order will begin." Bernard Lewis comes to a singular conclusion:

> We are facing a need and a movement far transcending the level of issues and policies and the governments that pursue them. This is no less than a clash of civilizations – the perhaps irrational but surely historic reaction of an ancient rival against our Judeo-Christian heritage, our secular present, and the worldwide expansion of both.[1]

Historically, the other great antagonistic interaction of Arab Islamic civilization has been with the animist, and now increasingly Christian black peoples to the south. In the past, this antagonism was epitomized in the image of Arab slave dealers and black slaves. It has been reflected in the on-going civil war in the Sudan between Arabs and blacks, the fighting in Chad between Libyan-supported insurgents and the government, the tensions between Orthodox Christians and Muslims in the Horn of Africa, and the political conflicts, recurring riots and communal violence between Muslims and Christians in Nigeria. The modernization of Africa and the spread of Christianity are likely to enhance the probability of violence along this fault line.

On the northern border of Islam, conflict has increasingly erupted between Orthodox and Muslim peoples, including the carnage of Bosnia and Sarajevo, the simmering violence between Serb and Albanian, the tenuous relation between Bulgarians and their Turkish minority, the unremitting slaughter of each other by Armenians and Azeris, the tense relations between Russians and Muslims in Central Asia, and the deployment of Russian troops to protect Russian interests in the Caucasus and Central Asia. Religion reinforces the revival of ethnic identities and restimulates Russian fears about the security of their southern borders. This concern is well captured by Archie Roosevelt:

> Much of Russian history concerns the struggle between Slavs and the Turkish peoples on their borders, which dates back to the foundation of the Russian state more than a thousand years ago. In the Slavs' millennium-long confrontation with their eastern neighbors lies the key to an understanding not only of Russian history, but Russian character.[2]

The conflict of civilizations is deeply rooted elsewhere in Asia. The historic clash between Muslim and Hindu in the subcontinent manifests itself now not only is the rivalry between Pakistan and India but also in intensifying religious strife within India between increasingly militant Hindu groups and India's substantial Muslim minority. The destruction of the Ayodhya mosque in December 1992 brought to the fore the issue of whether India will remain a secular democratic state or become a Hindu one. In East Asia, China has outstanding territorial disputes with most of its neighbors. It has pursued a ruthless policy toward the Buddhist people of Tibet, and it is pursuing an increasingly ruthless policy toward its Turkic-Muslim minority. With the Cold War over, the underlying differences between China and the United States have reasserted themselves in areas such as human rights, trade and weapons proliferation. These differences are unlikely to moderate. A "new cold war," Deng Xaioping reportedly asserted in 1991, is under way between China and America.

The same phrase has been applied to the increasingly difficult relations between Japan and the United States. Here cultural difference exacerbates economic conflict. People on each side allege racism on the other, but at least on the American side the antipathies are not racial but cultural. The basic values, attitudes, behavioral patterns of the two societies could hardly be more different. The economic issues between the United States and Europe are no less serious than those between the United States and Japan, but they do not have the same political salience and emotional intensity because the differences between American culture and European culture are so much less than those between American civilization and Japanese civilization.

The interactions between civilizations vary greatly in the extent to which they are likely to be characterized by violence. Economic competition clearly predominates between the American and European sub-civilizations of the West and between both of them and Japan. On the Eurasian continent, however, the proliferation of ethnic conflict, epitomized at the extreme in "ethnic cleansing," has not been totally random. It has been most frequent and most violent between groups belonging to different civilizations. In Eurasia the great historic fault lines between civilizations are once more aflame. This is particularly true along the boundaries of the crescent-shaped Islamic bloc of nations from the bulge of Africa to central Asia. Violence also occurs between Muslims, on the one hand, and Orthodox Serbs in the Balkans, Jews in Israel, Hindus in India, Buddhists in Burma and Catholics in the Philippines. Islam has bloody borders.

CIVILIZATION RALLYING: THE KIN-COUNTRY SYNDROME

Groups or states belonging to one civilization that become involved in war with people from a different civilization naturally try to rally support from other members of their own civilization. As the post-Cold War world evolves, civilization commonality, what H. D. S. Greenway has termed the "kin-country" syndrome, is replacing political ideology and traditional balance of power considerations as the principal basis for cooperation and coalitions. It can be seen gradually emerging in the post-Cold War conflicts in the Persian Gulf, the Caucasus and Bosnia. None of these was a full-scale war between civilizations, but each involved some elements of civilization rallying, which seemed to become more important as the conflict continued and which may provide a foretaste of the future.

First, in the Gulf War one Arab state invaded another and then fought a coalition of Arab, Western and other states. While only a few Muslim governments overtly supported Saddam Hussein, many Arab elites privately cheered him on, and he was highly popular among large sections of the Arab public. Islamic fundamentalist movements universally supported Iraq rather than the Western-backed governments of Kuwait and Saudi Arabia. Forswearing Arab nationalism, Saddam Hussein explicitly invoked an Islamic appeal. He and his supporters attempted to define the war as a war between civilizations. "It is not the world against Iraq," as Safar Al-Hawali, dean of Islamic Studies at the Umm Al-Qura University in Mecca, put it in a widely circulated tape. "It is the West against Islam." Ignoring the rivalry between Iran and Iraq, the chief Iranian religious leader, Ayatollah Ali Khamenei, called for a holy war against the West. The rallying of substantial sections of Arab elites and publics behind Saddam Hussein called those Arab governments in the anti-Iraq coalition to moderate their activities. Arab governments opposed or distanced themselves from subsequent

Western efforts to apply pressure on Iraq, including enforcement of a no-fly zone in the summer of 1992 and the bombing of Iraq in January 1993.

Second, the kin-country syndrome also appeared in conflicts in the former Soviet Union. Armenian military successes in 1992 and 1993 stimulated Turkey to become increasingly supportive of its religious, ethnic and linguistic brethren in Azerbaijan. "We have a Turkish nation feeling the same sentiments as the Azerbaijanis," said one Turkish official in 1992. Maybe we should show Armenia that there's a big Turkey in the region." President Turgut Ozal agreed, remarking that Turkey should at least "scare the Armenians a little bit." Turkey Air Force jets flew reconnaissance flights along the Armenian border and Iran announced they would not accept dismemberment of Azerbaijan. In the last years of its existence, the Soviet government supported Azerbaijan because its government was dominated by former communists. With the end of the Soviet Union, however, political considerations gave way to religious ones. Russian troops fought on the side of the Armenians, and Azerbaijan accused the "Russian government of turning 180 degrees" toward support for Christian Armenia.

Third, with respect to the fighting in the former Yugoslavia, Western publics manifested sympathy and support for the Bosnian Muslims and the horrors they suffered at the hands of the Serbs. Relatively little concern was expressed, however, over Croatian attacks on Muslims and participation in the dismemberment of Bosnia-Herzegovina. In the early stages of the Yugoslav breakup, Germany, in an unusual display of diplomatic initiative and muscle, induced the other 11 members of the European Community to follow its lead in recognizing Slovenia and Croatia. As a result of the pope's determination to provide strong backing to the two Catholic countries, the Vatican extended recognition even before the Community did. The United States followed the European lead. Thus the leading actors in Western civilization rallied behind its coreligionists. Subsequently Croatia was reported to be receiving substantial quantities of arms from Central European and other Western countries.

Boris Yeltsin's government, on the other hand, attempted to pursue a middle course that would be sympathetic to the Orthodox Serbs but not alienate Russia from the West. Russian conservative and nationalist groups, however, attacked the government for not being more forthcoming in its support for the Serbs. Islamic governments and groups, on the other hand, castigated the West for not coming to the defense of the Bosnians. Iranian leaders urged Muslims from all countries to provide help to Bosnia; in violation of the U.N. arms embargo, Iran supplied weapons and men for the Bosnians; Iranian-supported Lebanese groups sent guerrillas to train and organize the Bosnian forces. In 1993 up to 4,000 Muslims from over two dozen Islamic countries were reported to be fighting in Bosnia. The governments of Saudi Arabia and other countries felt under increasing pressure from fundamentalist groups in their own societies to provide more vigorous support for the Bosnians. By the end of 1992, Saudi Arabia had reportedly supplied substantial funding for weapons and supplies for the Bosnians. In the 1930s the Spanish Civil War provoked intervention from countries that politically were fascist, communist and democratic. In the 1990s the Yugoslav conflict is provoking intervention from countries that are Muslim, Orthodox and Western Christian. The parallel has not gone unnoticed. "The war in Bosnia-Herzegovina has become the emotional equivalent of the fight against fascism in the Spanish Civil War," one Saudi editor observed.

Conflicts and violence will also occur between states and groups within the same civilization. Such conflicts, however, are likely to be less intense and less likely to expand than conflicts between civilizations. Common membership in a civilization reduces the probability of violence in situations where it might otherwise occur. In 1991 and 1992 many people were alarmed by the possibility of violent conflict between Russia and Ukraine over territory, particularly Crimea, the Black Sea fleet, nuclear weapons and economic issues. If civilization is what counts, however, the likelihood of violence between Ukrainians and Russians should be low. They are two Slavic, primarily Orthodox peoples who have had close relationships with each other for centuries. As of early 1993, despite all the reasons for conflict, the leaders of the two countries were effectively negotiating and defusing the issues between the two countries. While there has been serious fighting between Muslims and Christians elsewhere in the former Soviet Union and much tension and some fighting between Western and Orthodox Christians in the Baltic states, there has been virtually no violence between Russians and Ukrainians.

THE WEST VERSUS THE REST

The West is now at an extraordinary peak of power in relation to other civilizations. Its superpower opponent has disappeared from the map. Military conflict among Western states is unthinkable, and Western military power is unrivaled. Apart from Japan, the West faces no economic challenge. It dominates international economic institutions. Global political and security issues are effectively settled by a directorate of the United States, Britain and France, world economic issues by a directorate of the United States, Germany and Japan, all of which maintain extraordinarily close relations with each other to the exclusion of lesser and largely non-Western countries. Decisions made at the U.N. Security Council or in the International Monetary Fund that reflect the interests of the West are presented to the world as reflecting the desires of the world community. The very phrase "the world community" has become the euphemistic collective noun (replacing "the Free World") to give global legitimacy to actions reflecting the interests of the United States and other Western powers. Through the IMF and other international economic institutions, the West promotes its economic interests and imposes on other nations the economic policies it thinks appropriate. The West in effect is using international institutions, military power and economic resources to run the world in ways that will maintain Western predominance, protect Western interests and promote Western political and economic values.

Differences in power and struggles for military, economic and institutional power are thus one source of conflict between the West and other civilizations. Differences in culture, that is basic values and beliefs, are a second source of conflict. V. S. Naipaul has argued that Western civilization is the "universal civilization" that "fits all men." At a superficial level much of Western culture has indeed permeated the rest of the world. At a more basic level, however, Western concepts differ fundamentally from those prevalent in other civilizations. Western ideas of individualism, liberalism, constitutionalism, human rights, equality, liberty, the rule of law, democracy, free markets, the separation of church and state, often have little resonance in Islamic, Confucian, Japanese, Hindu, Buddhist or Orthodox cultures. The very notion that there could be a "universal civilization" is a Western idea, directly at odds with the particularism of most Asian societies. Indeed, the author of a review of 100 comparative studies of values in different societies concluded that "the values that are most important in the West are least important worldwide."[3] The central axis of world politics in the future is likely to be, in Kishore Mahbubani's phrase, the conflict between "the West and the Rest" and the responses of non-Western civilizations to Western power and values.[3]

THE "TORN" COUNTRIES

In the future, as people differentiate themselves by civilization, countries with large numbers of people of different civilizations, such as the Soviet Union and Yugoslavia, are candidates for dismemberment. Some other countries have a fair degree of cultural homogeneity but are divided over whether their society belongs to one civilization or another. These are "torn" countries. Their leaders typically wish to make their countries members of the West, but the history, culture and traditions of their countries are non-Western. The most obvious and prototypical torn country is Turkey. The late twentieth-century leaders of Turkey have followed in the Ataturk tradition and defined Turkey as a modern, secular, Western nation state. They allied Turkey with the West in NATO and in the Gulf War; they applied for membership in the European Community. At the same time, however, elements in Turkish society have supported an Islamic revival and have argued that Turkey is basically a Middle Eastern Muslim society. In addition, while the elite of Turkey has defined Turkey as a Western society, the elite of the West refuses to accept Turkey and such. Turkey will not become a member of the European Community, and the real reason, as President Ozal said, "is that we are Muslim and they are Christian and they don't say that." Having rejected Mecca, and then being rejected by Brussels, where does Turkey look? Tashkent may be the answer. The end of the Soviet Union gives Turkey the opportunity to become the leader of a revived Turkic civilization involving seven countries from the borders of Greece to those of China. Encouraged by the West, Turkey is making strenuous efforts to carve out this new identity for itself.

During the past decade Mexico has assumed a position somewhat similar to that of Turkey. Just as Turkey abandoned its historic opposition to Europe and attempted to join Europe, Mexico has stopped defining itself by its opposition to the United States and is instead attempting to imitate the United States and to join it in the North American Free Trade Area. Mexican leaders are engaged in the great task of redefining Mexican identity and have introduced fundamental economic reforms that eventually will lead to fundamental political change. In Mexico as in Turkey, significant elements in society resist the redefinition of their country's identity. In Turkey, European-oriented leaders have to make gestures to Islam (Ozal's pilgrimage to Mecca); so also Mexico's North American-oriented leaders have to make gestures to those who hold Mexico to be a Latin American country (Salinas' Ibero-American Guadalajara summit).

Globally the most important torn country is Russia. The question of whether Russia is part of the West or the leader of the Slavic-Orthodox civilization has been a recurring one in Russian history. That issue was obscured by the communist victory in Russia, which imported a Western ideology, adapted it to Russian conditions and then challenged the West in the name of that ideology. The dominance of communism shut off the historic debate over Westernization versus Russification. With communism discredited Russians once again face that question. President Yeltsin is adopting Western principles and goals and seeking to make Russia a "normal" country and a part of the West. Yet both the Russian elite and the Russian public are divided on this issue. An opinion survey in European Russia in the spring of 1992 revealed that 40 percent of the public had positive attitudes toward the West and 36 percent had negative attitudes. As it has been for much of its history, Russia in the early 1990s is truly a torn country.

THE CONFUCIAN-ISLAMIC CONNECTION

The obstacles to non-Western countries joining the West vary considerably. They are least for Latin American and East European countries. They are greater for the Orthodox countries of the former Soviet Union. They are still greater for Muslim, Confucian, Hindu and Buddhist societies. Japan has established a unique position for itself as an associate member of the West: it is in the West in some respects but clearly not of the West in important dimensions. Those countries that for reason of culture and power do not wish to, or cannot, join the West compete with the West by developing their own economic, military and political power. They do this by promoting their internal development and by cooperating with other non-Western countries. The most prominent form of this cooperation is the Confucian-Islamic connection that has emerged to challenge Western interests, values and power.

Almost without exception, Western countries are reducing their military power. China, North Korea and several Middle Eastern states, however, are significantly expanding their military capabilities. In the post-Cold War world the primary objective of arms control is to prevent the development by non-Western societies of military capabilities that could threaten Western interests. The West attempts to do this through international agreements, economic pressure and controls on the transfer of arms and weapons technologies. The non-Western nations, on the other hand, assert their right to acquire and to deploy whatever weapons they think necessary for their security. They also have absorbed, to the full, the truth of the response of the Indian defense minister when asked what lesson he learned from the Gulf War: "Don't fight the United States unless you have nuclear weapons." China already has nuclear weapons; Pakistan and India have the capability to deploy them. North Korea, Iran, Iraq, Libya and Algeria appear to be attempting to acquire them.

China is also a major exporter of arms and weapons technology. It has exported materials to Libya and Iraq that could be used to manufacture nuclear weapons and nerve gas. It has helped Algeria build a reactor suitable for nuclear weapons research and production. China has sold to Iran nuclear technology that American officials believe could only be used to create weapons and apparently has shipped components of 300-mile-range missiles to Pakistan. North Korea has had a nuclear weapons program under way for some while and has sold advanced missiles and missile technology to Syria and Iran. A Confucian-Islamic military connection has thus come into being, designed to promote acquisition by its members of the weapons and weapons technologies needed to counter the military powers of the West.

IMPLICATIONS FOR THE WEST

This article does not argue that civilization identities will replace all other identities, that nation states will disappear, that each civilization will become a single coherent political entity, that groups within a civilization will not conflict with and even fight each other. This paper does set forth the hypotheses that differences between civilizations are real and important; civilization-consciousness is increasing; conflict between civilizations will supplant ideological and other forms of conflict as the dominant global form of conflict; international relations, historically a game played out within Western civilization, will increasingly be de-Westernized and become a game in which non-Western civilizations are actors and not simply objects; conflicts between groups in different civilizations will be more frequent, more sustained and more violent than conflicts between groups in the same civilization; violent conflicts between groups in different civilizations are the most likely and most dangerous source of escalation that could lead to global wars; the paramount axis of world politics will be the relations between "the West and the Rest"; a central focus of conflict for the immediate future will be between the West and several Islamic-Confucian states.

This is not to advocate the desirability of conflicts between civilizations. It is to set forth descriptive hypotheses as to what the future may be like. If these are plausible hypotheses, however, it is necessary to consider their implications for Western policy. These implications should be divided between short-term advantage and long-term accommodation. In the short term it is clearly in the interest of the West to promote greater cooperation and unity within its own civilization, particularly between its European and North American components; to incorporate into the West societies in Eastern Europe and Latin America whose cultures are close to those of the West; to promote and maintain cooperative relations with Russia and Japan; to prevent escalation of local inter-civilization conflicts into major inter-civilization wars; to limit the expansion of the military strength of Confucian and Islamic states; to exploit differences and conflicts among Confucian and Islamic states; to support in other civilizations groups sympathetic to Western values and interests; to strengthen international institutions that reflect and legitimate Western interests and values and to promote the involvement of non-Western states in those institutions.

In the longer term other measures would be called for. Western civilization is both Western and modern. Non-Western civilizations have attempted to become modern without becoming Western. To date only Japan has fully succeeded in this quest. Non-Western civilization will continue to attempt to acquire the wealth, technology, skills, machines and weapons that are part of being modern. They will also attempt to reconcile this modernity with their traditional culture and values. Their economic and military strength relative to the West will increase. Hence the West will increasingly have to accommodate these non-Western modern civilizations whose power approaches that of the West but whose values and interests differ significantly from those of the West. This will require the West to maintain the economic and military power necessary to protect its interests in relation to these civilizations. It will also, however, require the West to develop a more profound understanding of the basic religious and philosophical assumptions underlying other civilizations and the ways in which people in those civilizations see their interests. It will require an effort to identify elements of commonality between Western and other civilizations. For the relevant future, there will be no universal civilization, but instead a world of different civilizations, each of which will have to learn to coexist with the others.

Notes

1. Bernard Lewis, "The Roots of Muslim Rage," *The Atlantic Monthly*, 266, September 1990, p. 60; *Time*, June 15, 1992, pp. 24-28.

2. Archie Roosevelt, *For Lust of Knowing*, Boston: Little, Brown, 1988, pp. 332-333.

3. Kishore Mahbubani, "The West and the Rest," *The National Interest*, Summer 1992, pp. 3-13.

A Structural Theory of the 5,000-Year World System

André Gunder Frank

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Our thesis has been articulated in *The World System: Five Hundred Years or Five Thousand?* (1993). Its main theoretical premises are: (1) the existence and development of the world system that stretches back not just for 500 years but for 5,000 years; (2) the (political) world economy is a world system; (3) the process of capital accumulation is the motor force of (world system) history; (4) the center-periphery structure is one of the characteristics of the world system; (5) the world system is depicted by hegemony and rivalry of political power although system-wide hegemony has been rare or nonexistent; (6) long economic cycles of alternating ascending and descending phases underlie economic growth of the world system.

This approach addresses several disciplines and participates in longstanding controversies within and between them by exploring the connections of our thesis with historiography, civilizationism, archaeology, classicism in ancient history, economic history, macro-historical sociology, political geography, international relations, development studies, ecology, anthropology, and so on.

Theoretical Categories and Definitions

The World System

Contra Wallerstein (1974), we believe that the existence and development of the world system in which we live stretches back 5,000 years or more. According to Wallerstein – and unlike our world system (without a hyphen) – "world-systems" (with a hyphen and sometimes plural) are in a "world of their own, which need not be even nearly worldwide". Wallerstein (1982) further stresses the difference between world economy (without a hyphen) and world-economy (system) (with a hyphen): "The world economy is an expression applied to the whole world. . . . A world-economy only concerns a fragment of the world, an economically autonomous section". Our view is that the world system has long been much larger and older than the European-centered "world-economy" or world-system" of Wallerstein. We can identify one single world system already in the Bronze Age and an unbroken historical continuity between the world system from that era to our contemporary "modern capitalist world-system."

A criterion of systemic participation in a single world system is that no part of this system would be as it is if other parts were not as they are. We explicate that surplus extraction and accumulation are shared or interpenetrating across otherwise discrete political boundaries. Thus, elites participate in each others' system of exploitation vis-à-vis the producing classes. This participation may be via economic exchange, political relations (e.g., tribute), or through combination of both. All of these relations characterize the millenarian relationship, for example, between the peoples of China and Inner Asia. This interpenetrating accumulation thus creates an interdependence between structures of accumulation and political entities.

The identification of the geographical extent of near-simultaneity of the up and down phases of cycles may serve as another important operational definition of the extent of the world system. If distant parts of Afro-Eurasia experience economic expansions and contractions nearly simultaneously, that would seem to be evidence that they participate in the same world system. Edens and Kohl (1993) suggest that a major criterion of participation in a single world system is near-simultaneity (or "synchronism") of expansion and contraction. This suggests

> [the] action of an interrelated set of transregional social forces operative over vast regions of western Asia from the mid-third through the mid-second millennium BC. . . . The existence of an ancient world system is postulated by the largely synchronous processes of rise and collapse recorded throughout this area; it is difficult to deny that one here is witnessing historically connected processes . . . (in) the world system.

The World Economy

We propose that a world economy has been in existence for a long period of time and distinguish two related issues from this proposal. One refers to the existence of production for exchange, through the market, followed with capital accumulation. The other is that these economic relations comprised a division of labor with specialization and trade and occurred on a large scale over long distance so as to link distant areas into a single "world" economy. Both propositions are controversial, but we believe that there is ample historical evidence to support these claims.

There is evidence of a market/credit economy existing as far back as Assyria. In our definition of the world system, regular exchange of surplus does affect the "internal" character of each of the parts of the world system as well. Some scholars, such as Wallerstein, for instance, reject our definition because they do not believe that "mere" trade makes a "system." We do. We not only believe that regular and significant trade provides sufficient ground for speaking of a "system" or of a real "world economy", but also that trade integrates social formations into something that should be called the "international division of labor," even in the ancient Eurasian world economy. This takes place because trade and production are not separated. The nature of trade directly affects the character of production, as the history of the early modern world system so clearly illustrates. These effects are a consequence of specialization if nothing else, and we contend that they are intimately related to the system of the regular transfer of surplus as well as to specialization. Wallerstein (1993) sets very specific criteria for the level of integration in his international division of labor that precludes considering the pre-1500 division of labor as being in the same formal category, so as to preserve the distinctiveness of the European "capitalist" world-economy.

A related question is how extensive this division of labor and trade network was. By our aforementioned criteria, as early as the third millennium BC, the world economy/system included Egypt, Mesopotamia, the Near East, Persia, the Indus Valley and parts of Central Asia (all south of the east-west mountain ranges that transverse much of Asia), however, the analysis of Evgenij Chernykh (1992) also leads to the inclusion in this world system of the region north of the mountains involving "a whole chain from the Atlantic to the Pacific". We find that this world economy/system was formed in the third millennium BC or earlier and that since then it has had a continuous (albeit cyclical) development that incorporated more and more areas of the globe, and that still proceeds today. Although the luxury trade did play a significant role in these ancient external trade relations at that time, there were also significant amounts of economically vital trade in bulk necessities: metals, timber, grain, animals and other raw materials and foodstuffs, textiles and ceramics. For instance, southern Mesopotamia lacked metals and timber and was dependent on their import from Anatolia and the Levant, while it exported grains and textiles.

Capital Accumulation

We regard the process of accumulation as the motor force of world system history. Wallerstein and others regard continuous capital accumulation as the *differentia specifica* of the "modern world-system." We have argued elsewhere that in this regard the "modern" world system is not so different and that this same process of capital accumulation has played a central role in the world system for several millennia. Others disagree: they argue that previous world-systems were what Amin and Wolf (1982) call "tributary" or Wallerstein "world empires." In these, politics and ideology were in command, not the economic law of value in the accumulation of capital.

In our view "ceaseless accumulation" is a feature of the world system throughout its development and is not unique to the modern period. Though there can be no real doubt that industrialization of production played a crucial role in bringing about a quantitative change in the rate of “ceaseless accumulation” in the modern period, in our view this change is essentially a matter of degree. Indeed, Wallerstein himself says that the difference between so-called proto-capitalism and supposedly full-blown capitalism is really a matter of degree. This debate turns on the definition of “ceaseless,” since Wallerstein also notes the existence and indeed even perhaps the prevalence of capital before the “modern” period.

There has been a fundamental misconception of the character of the “premodern” economy, particularly of Eurasia, based on the mistaken generalization of the “command economy”. In our view, what Amin and Wolf call the “tributary mode” is, more often than not, merely “taxation” by another name. The fact that all historical states have lived by some form of taxation is hardly a revelation to anyone. However, it is not necessarily incompatible with the idea that more often than not, these premodern states coexisted with a vibrant commercial sector in the economy, primarily directed by private merchants and bankers and conducted on a vast international scale. The sheer volume of evidence from the various parts of Eurasia corroborates the contention of the centrality of this world economic commerce for early modern times (see Abu-Lughod, 1989).

Center-Periphery/Hegemony-Rivalry Structure

This structure is familiar to analysts of dependence in the “modern” world system and especially in Latin America since 1492. It includes, but is not limited to, the transfer of surplus between zones of the world system. However, we now find that this analytical category is also applicable to the early periods of the world system. The structure of this world system does not conform to the “unipolar” model of center-periphery relations, common in most approaches using this concept. We see more “multipolar” center-periphery relations on a world scale. Therefore, the world system is not viewed as having always been composed of a single core and single periphery, but rather of an interlinked set of center-periphery complexes joined together in an overall ensemble.

Thus, the world system, first in Eurasia before 1500 and globally after 1500, has always been multicentric in structure. This includes even the period of supposed unipolar European (or Western) global hegemony in the modem world system. Distinct regional, imperial or market-mediated center-periphery complexes are all part of a single whole with systemic links to one another. Yet this multi-centricity does not mean "equality" among the various centers or between different center-periphery complexes in the world system. This multi-centricity is hierarchically structured. There is a very complex "chain" of "metropole-satellite" relations of extraction and transfer of surplus throughout the whole world system.

The center-periphery structure of the world system is simultaneously an economic hierarchy as well as a political hierarchy, as hegemony embodies both. World system and international relations literature has produced many analyses of alternation between hegemonic leadership and rivalry for hegemony in the world system. However just as the world economy/system never entirely "falls" but only changes, hegemonic ascent and descent are usually quite gradual and do not occur in a unipolar framework, but rather in a multipolar one. We particularly emphasize how economic rhythms common to the entire world economy/system, such as long cycles of expansion and contraction, affect the relative position of all of the "parts" of the system.

Our position is distinguished by the argument that these ascents and declines occur within the same world economy/system. Therefore, we also have serious reservations about widely accepted theories of hegemony, such as Modelski and Thompson's "political" leadership or Wallerstein's "economic" hegemony. To begin with, the claims that Portugal, the Netherlands, England and the United States have successively been hegemonic is based on their hegemony in an essentially European/Western-centered world system. If we recognize that in the sixteenth to eighteenth centuries the world system was much larger than the "European world system," then the claim to hegemony of little Venice, Portugal and the Netherlands within the whole Afro-Eurasian and American world economy immediately becomes doubtful. All of these economies and their participation in the world were too small in scale to exercise any kind of "hegemony" in the world system. Moreover, they certainly were not the centers of world economic accumulation. By comparison, Ming/Qing China and Moghul India, as well as the Ottoman Empire, and perhaps Safavid Persia politically and economically far outranked any of the individual western European economies and states – and probably all the European ones added together.

Furthermore, "hegemony" is a feature of the world system itself, more than of any of its parts. By that criterion also, the small European city-states and even national economies were in no sense hegemonic. On the contrary, their very economic success was entirely derived from their subsidiary participation in an Asian-based world economy in which accumulation was centralized in India and China. Indeed, the Europeans were able to participate in this world economy at all only by virtue of the golden and silver means of payment that they plundered from the Americas, a substantial portion of which they transshipped to the economies of West, South and East Asia, where the real accumulation took place on the basis of their respective manufacturing superiority and competitiveness.

Therefore, we are led to conclude that not only throughout world system history but even during the modern period, world economic/systemic hegemony is rare if not nonexistent, and that hegemony tends itself to generate the conditions and competition that soon undermine one hegemony and replace it with rivalry and an alternative hegemony. The norm of the situation we have called "interlinked hegemonies". This is how one arrives at the formulation that global or world hegemony is always shared hegemony, exercised through a complex network composed of class coalitions, alliances, and other forms of association between states, including competitive ones. Furthermore, the world system is characterized by a number of coexisting core powers (or interlinked hegemonic powers) that become increasingly integrated via both conflictual and cooperative relations.

Long and Short Economic Cycles

We have noted the apparent existence of alternating ascending phases of economic and political expansion and descending phases of political and economic crises. An important characteristic of the "modern" world system is that the process of capital accumulation, center-periphery position, world system hegemony and rivalry are all cyclical and occur in tandem with each other. We find that this same world system cycle and its features also extend back many centuries before 1492.

We now believe that we can identify a cyclical pattern of long ascending and descending phases in the same world system back at least through the third millennium BC. Most revealing of the extent of the world system is the approximate 500-year-long economic cycle, and the interregional near-synchronization of its approximate 250-year-long up (A) and down (B) phases. Our suggested dating of the ascending and descending phases for the entire "Bronze Age" world system is approximately: A: 3000-2700, B: 2700-2600, A: 2600-2400, B: 2400-2000, A: 2000-1750, B: 1750-1500, A: 1500-1200, B: 1200-1000 (which was the Bronze dark age crisis). Tentative "Iron Age" dates are as follows: A: 1000-800, B: 800-550, A: 550-450, B: 450-350, A: 350-200, B: 200-50, A: 100 BC-200 AD. B: 200-500, A: 500-750, B: 750-1000, A: 1000-1250, B: 1250-1450, A: 1450-1600.

Of course, we should not expect to find complete synchronization nor simultaneity of A and B phases across the entire world system, and still less in its Bronze Age beginnings. It seems enough to be able to demonstrate or even suggest substantial synchronization of economic expansion or contraction over very wide areas, which are usually considered to be quite independent of each other. Expansions and contractions seem to begin in one part of the world system, usually in its center core, and then tend to diffuse to other parts, including toward core competitors and the periphery. Dales (1976) observed an apparent eastward displacement of cycle phases through West, Central, and South Asia in the third millennium BC. Today, cyclical expansion, and especially contraction, begins in the United States and spreads out from there. Therefore, cyclical decline also tends to spell the relative or even absolute decline of the principal core power. This decline crisis offers opportunities to some rivals, or often even to some peripheral part of the system. Some of them advance both absolutely and relatively, perhaps to even replace the previous central core.

Implications for Recent World Economic History

Thus, long before the birth of the putative "European world-economy" the real world economy had a far-flung division of labor and intricate trade system, which was preponderantly Asian. This also means of course that, as Abu-Lughod persuasively argues, the city-centered interlinked regions of Asia were dominant in the world economy before

European hegemony. However, this Asian dominance was not limited to her "thirteenth century world system." It also continued long after that in a world economy that (western) Europe did not attain hegemony over until the nineteenth century. In reality, during the period 1450-1750, sometimes regarded as the period of "primitive accumulation" leading to full capitalism, the world system was still very predominantly under Asian hegemonic influences. The Chinese Ming/Qing, Ottoman, Indian Moghul and Persian Safavid empires were economically and politically very powerful and only waned vis-à-vis the Europeans toward the end of this period or even thereafter. Therefore, if anything, the "modern" world economic system was under Asian, not European, hegemony. Likewise, much of the real dynamism of the world economy and its primary centers of production and capital accumulation also lay in Asia throughout this period.

The most important European impact was the injection of new supplies of American silver and gold – and thereby themselves – into the already well established Eurasian economy. The Europeans did not in any sense "create" either the world economic system itself nor "capitalism." What the injection of new liquidity into the world economy actually seems to have done was to make important, though limited, changes in financial flows, trade and production patterns within the world economy, and to permit the Europeans to participate more actively in the same. The Europeans were able to sell very few manufactured goods to the east, and instead profited substantially from inserting themselves into the "country trade" within the Asian economy itself. They specialized in exploiting global differences in resources, production and prices to maximize their profits as middlemen, and they used military force and naval forts to enforce their own participation in this exchange. However, Europe itself was not a first-rank power nor an economic core region during these three centuries. The core regions, especially of industrial production, were in China and India. West Asia and Southeast Asia also remained economically more important than Europe (Braudel estimates that the Asian economy was still five times larger than the European-American one in 1750).

The introduction of American silver (and to a lesser extent gold) into this Afro-Eurasian economy only increased and accelerated quantitative economic growth in an otherwise qualitatively ongoing system. The major exporters of silver were Latin America and Japan, and of gold were Latin America and Africa. The major importer and re-exporter of both silver and gold was western and southern Europe, to cover its own perpetual massive structural deficit with all other regions. India had a massive surplus with Europe and some with West Asia, based mostly on its more efficient low-cost cotton textile production. These went westward to Africa, West Asia, Europe and from there on across the Atlantic to the Caribbean and the Americas. In return, India received massive amounts of silver and some gold from the west. India also exported cotton textiles to Southeast Asia, and exchanged cotton textiles for silk and porcelain from China.

China had a surplus with everybody, based on its unrivalled manufacturing production and export of silks and porcelain and other ceramics. Therefore, China, which like India had a perpetual silver shortage, was the major net importer of silver and met much of its need for coinage out of imports of American silver, which arrived via Europe, West Asia, India, Southeast Asia, and with the Manila galleons directly from Acapulco. China also received massive amounts of silver and copper from Japan and some through the overland caravan trade across Central Asia. The complexity of the international division of labor and the network of world trade should suffice to indicate that all of these world regions were integral parts of a single world economic system between about 1400 and 1800 AD.

(DIS)AGREEMENT WITH OTHER WORLD SYSTEM THEORISTS

The debate between 500 or 5,000 years of world system history is primarily about continuity versus discontinuity in world history. There are two main positions in this debate. One position is that political/ideological determination of the mode of production before about 1500 AD makes for a sharp break between the pre-1500 and post-1500 periods. This position is dominant among most historians; among world-system theorists it is shared by Wallerstein and his followers. The other position is that capital accumulation did not begin or become "ceaseless" only after 1500 AD, but has been the motor force of the historical process throughout world-system history; thus, there was no such sharp break between different "world-systems" or even modes of production around 1500.

We believe that modes of production are not the key to understanding the "transitions" in the history of world development. Others are also critical of these mode-of-production categories, yet they still maintain that 1500 represents a sharp break with the past. We believe that the continuity and developmental dynamic of the world system as a whole is far more important. Furthermore, real "transitions" seem to be more a matter of the role and position a particular entity fills in the world accumulation process than of changes in modes of production. From this perspective, "hegemonic transitions" have occurred throughout world history and entail not only a shift in the locus of capital accumulation, but necessarily entail profound changes in social, political, economic and cultural aspects of the world system. Of course, these changes raise the question of how alike or different the early world system was from the modern one. The "continuationists," such as Wilkinson and ourselves, and increasingly Chase-Dunn and Hall (who like we, eschew modes of production and prefer modes of accumulation) and Modelski and Thompson, emphasize the commonalities; and the "transformationists," especially Wallerstein and Amin, focus on the differences, especially the "mode of production."

Yet both continuationists and transformationists lack a systematic theory of social or historical evolution. Chase-Dunn and Hall observe that "all world-systems pulsate in the sense that the spatial scale of integration, especially by trade, gets larger and then smaller again" and that "all systems experience the rise and fall of hierarchies". We agree and have found large regions that seem to "drop out" of the world system for long periods of time (India apparently from nearly 1900 to 900 BC and western Europe from 500 to 1000 AD), in that we do not find evidence of their continued participation in especially the system's cyclical upswings. Therefore, the extent of the world system cannot be interpreted in terms of the amount or degree of interaction within it at any one particular time, since the cyclical rhythm or pulsation of the system itself generates greater or lesser scales of integration, especially by trade.

Modelski and Thompson's (1988) temporal and spatial expansion of their cyclical purview overlaps with ours in several respects, but while they now claim to analyze the world system, in fact they remain essentially Eurocentric. We could grant them that the "lead economies are the sparkplugs of the world economy" and that the lead economy in the eleventh and twelfth centuries was in China. But in that case, when begins "the history of European expansion as the core of the world economy"? In their schema, the shift occurs beginning in 1190, centered on the Champagne fairs and the Black Sea trade after 1300 for Venice, from 1540 on for the Baltic and Atlantic trade, and from 1580 for Asian trade. However, it is clear that crucial for Modelski and Thompson is only European *trade*:

> The principal structural change experienced by the global economy in the fifteenth to eighteenth centuries was the construction of an oceanic trading system . . . (and) innovations in long-distance trade after 1500 . . . centered around the pioneering of new trading routes . . . (in) new phases of European imperialism.

Yes, indeed, for Europe – but only for Europe and its new American colonies. For Asians, these same trade routes were age old. It is quite incorrect – and a Eurocentric perspective – to claim that an oceanic trading system was *constructed* only from the fifteenth to eighteenth centuries, just as it is incorrect to claim that "Indian textiles became important about the same time as American plantation crops". Perhaps they did so for Europeans, but in Asia the importance of its own textile production and trade was much earlier and remained much greater in the world economy. Just how, and through what mechanism does the Modelski-Thompson center of gravity in the world economy shift from Song China westward allegedly all the way over to little (one million population) Portugal, bypassing virtually everything and everybody in between? How was this possible? Simply in that another place gets a new technology (not to mention what that new Portuguese technology was)? We need more explanation of this crucial process of transition, if it took place at all, (which we doubt).

Chase-Dunn and Hall come out for comparative analysis, which is exactly why they insist on studying world-systems. Indeed, they are so anxious to do comparative work that they categorize not only all or parts of Eurasia, but also the Wintu Indians in California or "indigenous" Hawaii as "world-systems." We agree that the more comparison we can manage, the better; but we prefer to use the term "world system" and to reserve it for as much of Afro-Eurasia and later the "New World" as can legitimately be viewed as sufficiently interconnected to have been parts of a single world system. Supposedly, Chase-Dunn and Hall are "splitters," whereas we are "lumpers". The point of being a splitter is to undertake comparative analysis, where the units of analysis being compared are "world-systems," including even the putative "mini-systems." This worthy and potentially fruitful project could generate useful abstractions about similar (and different) large-scale, long-term processes of social change and especially about the transformational logic in world system evolution, particularly if the comparisons were among long-lasting large-scale historical world systems, for example, in Mesoamerica, the Andes and Afro-Eurasia.

But we see three possible problems. First, the reliance of Chase-Dunn and Hall, like Marx, on the concept of a "tributary mode of production" makes any such structural, let alone transformational, comparisons problematic, especially if the same "mode" was supposed to have been qualitatively unchanged all around the world for over 4,000 years. Second is the vast amount of historical data that must first be gathered and analyzed before meaningful comparisons become possible. Finally, is the temptation to simplify the processes too much, particularly if this takes the form of some kind of economic reductionism.

In summary, our world system approach is based on the rejection of three conventional dichotomies: (1) between the "premodern" and the "modern" economies; (2) between the premodern and modern political cycles, that is, between a premodern "cycle of empires" versus a uniquely modern cycle of (single) hegemonies; and (3) between a "precapitalist" world composed of several distinct world-economies and a unitary "capitalist" world system post-1500. From our perspective humanity truly is one, having a true common heritage and sharing a common destiny. But we do not propose to return to the cause of universalism(s), and especially not of the Western-based universalism of "development" or "modernization," now being sold in the guise of the equation of "democracy" equals the "free market".

Coming Full Circle

World-System History: From Traditional International Politics to the Study of Global Relations

Robert Denemark

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Any realistic evaluation of the body of knowledge created by students of traditional international politics must come to the unhappy conclusion that the endeavor has largely been a failure. After years of work by thousands of scholars, the questions facing the field are not those of how to build collective insights, but continue to focus on the introductory concerns of how to legitimately *begin*. The discipline does not lack intelligent scholars, plentiful and interesting data or ingenious theories. Yet its triumphs in the areas of understanding, explanation and prediction have been few, its vision shallow, and its range narrow. Four problems have turned the traditional study of international politics into an intellectual cul-de-sac: 1) the post-Renaissance emphasis on the state; 2) the myth of an autonomous international "politics"; 3) an emphasis on the near term; and 4) a bias toward problems of European origin.

THE PROBLEMS WITH TRADITIONAL INTERNATIONAL POLITICS

State-Centrism

The contemporary field of "international politics" answered implicitly the complex question, to whom does one owe loyalty? After the Treaty of Westphalia, with territorial rulers gaining power relative to religious authority, with the codification of the right to make independent foreign policies, and with the maturing of projects to create "nations" of those within given boundaries, states could lay claim to being the fundamental units of both local and global politics. Students of the global social order were not foolish to turn their attentions toward the state. After Westphalia, state power grew, pressing its advantage in the areas of religion, culture, finance and war. As the European state system imposed itself on the rest of the world, social development was thought to have found its naturally dominant actor. "Developing" areas needed to be structurally differentiated and secular to become "modern." Their task was to catch up with the social, political and economic forms of the West.

But three problems rest with the turn to state-centrism. First, state power was hardly as complete as states pretended. Official histories and protestations aside, states have never had the power that the term "sovereignty" pretends to bestow, to the point that Michael Mann's study of power covering the period extending more than a century beyond the Treaty of Westphalia denies states played anything like a central role. Yale Ferguson and Richard Mansbach argue that the forces that drive and divide social groups are basically as they have always been, and that "politics" and "polities," but not "states," should rest at the core of our analyses. A second problem with the state-centric focus is that the growth of state power is not consistent. States or state-like entities have been dominant in some times and places, but state dominance was not nearly as smooth or consistent as generally suggested. There is no reason to assume that states are always to be as central to global social processes as they have been recently. A third problem concerns our vision. As states became focal points of the discipline, our ability to recognize or seriously consider non-state-centric phenomena was impinged. It is as foolish to ignore the various alternative sources of social power as it would be to ignore the state. Yet the state continues to be the focal point of our disciplinary vision, and other areas of concern are viewed as important only when they intersect with it.

Politics as Autonomous

Another major difficulty with which we must contend is a conceptualization of politics as an autonomous activity. From this perspective, international "politics," usually defined in terms of power or authority, is a realm to be studied separately from other international phenomena: social, cultural or economic. This tendency toward disciplinarity is not without value: any attempt to cut into a problem as vast as global social relations would founder without some ability to narrow its scope and produce more specialized knowledge. But as we divided the various social sciences, we also created different vocabularies, methodologies and theoretical strains. What began as an attempt to make a large problem more amenable by creating specialized knowledge ended with our collective inability to draw those insights back to the questions they were created to answer.

A second powerful force stood in the way of escaping from conceptualizations of politics as autonomous. The Cold War made the study of any phenomena from a materialist perspective suspect: it was the realist Hans Morgenthau who galvanized the field with his explicit identification of politics as an autonomous field of inquiry, while the realist E. H. Carr – who included economic and ideological elements in what constituted world politics – was more often ignored. For Morgenthau, students of politics must study the power of states, while issues of wealth or ideas of justice had to be left to economists and philosophers. Given Cold War concerns, strategy and power politics quickly consumed the field. With the demise of this structuring animosity, the study of "international politics" lapsed into a morass of issues for which grand strategy proves an inadequate lens. Political science (the study of authority) is no more autonomous than economics (are there no sources of authority involved in the creation of markets?), sociology (the study of both), anthropology (the study of both historically), or history (the study of all of the above).

Temporal Scope

A third problem with the study of international politics is its temporal scope. How far back need one go to discover what is relevant? Once again, the Cold War intervenes with a chillingly shallow response. It would be most reasonable for scholars to respond by suggesting that they work backward historically to the point of origin of the problems that concern them. But since the Cold War consumed the field, what was "relevant" began in 1945, maybe 1940, or perhaps stretched back all the way to 1917, but no further. With the end of the Cold War, we face a dilemma. If we inquire about the time frame for the problems of our era, and find no agreement on the nature of those problems, we can offer no united response. Traditionalists may continue to start with the Cold War.

Students of radical nationalism and ethnic fratricide in the Balkans, however, may not be satisfied with the unhelpful suggestion that the rise of ethnic nationalism emerges with the decline of the Soviet Empire. When did these divisions first arise, why, to what end, and what are the possible ameliorative elements? Do we look to World War I and the validation of nationalism, or to the rise of nationalism in Europe over the course of the last three centuries, or to eras before European dominance when Ottoman control brought a mixing of religious populations?

What of the questions of inequality and underdevelopment? Lack of fidelity to the market constitutes a typical Cold War-era response, but state-driven Asian development models suggest that the study of underdevelopment must be pushed back another two or three centuries to the rise of western mercantilism. Historical patterns of differential development, as between eastern and western Europe, find explanations in the sixteenth (Immanuel Wallerstein), fourteenth (Giovanni Arrighi), or even eleventh (Peter Gunst) centuries. How old is trade competition? McNeill traces the rise and decline of trading empires and market systems back one thousand years, whereas Frank argues for a far earlier starting point. The problems of our era are not unique to our era. If we wish to discern relevant parameters, origins, trends or patterns, we must must extend our historical reach.

Eurocentrism

The final problems with contemporary international politics concern how we anticipate new problems or seek solutions to old ones. For the last two centuries, the center of attention in international politics has been Europe. Even when this dominance ended following World War II, the winners (the United States and the Soviet Union) traced their own histories and ideologies in a European light. This Eurocentric bias, with its emphasis on the clash of European social, political and economic systems, consumed international politics. It is ironic that European norms remained the focus of international politics after European society had ceased to dominate, without much apparent concern for how or why the European era was ending. Had other regions risen and declined? For what reason, in response to what forces, and with what warnings? These questions could be addressed only with reference to other histories of large-scale rise and decline.

Non-European examples are clearly evident. Scholars know that the glory of Rome was presaged by the Greeks, and followed by a dismal age. China had become a great empire, huge cities had been built and abandoned in India, and Egypt contained architectural wonders as evidence of a glorious past. Great southern and western African states, like those of South America, had waxed and waned. Yet Europe and its progeny looked only inward. "Development" and "modernization" were the sole right of the European system, which "developed" because of culturally generated superior economic organization (John Landes), founded on unique religious beliefs (Max Weber), or more efficient political systems born of many small, feuding states (Charles Tilly; Paul Kennedy). Visions of Europe as exceptional, and of European developments as pristine, bias our understandings and explanations and threaten our ability to explain even the fate of Europe itself.

World-System History

These criticisms are not new and have been addressed by various scholars. One school of thought whose proponents make a concerted effort to deal with these issues is that of world-system history. Some of the main themes of recent work in world-system history are 1) systemic-level interaction; 2) a transcending of disciplinarity; 3) a real concern with the long historical term; and 4) a non-Eurocentric analysis. These different starting points and assumptions facilitate a broader range of explanation and a potentially greater facility for prediction.

1) The Systemic Level

World-system history begins with the thesis that the whole system, the interaction of what we see as its units, and not the constitution and/or functioning of the units themselves, is the proper focus of attention. It is "systemic" in the sense that the whole is greater than the sum of its parts, and that the "system" provides the architecture and the incentives that structure the action of the various component units.

Drawing on a wealth of historical literature, Janet Abu-Lughod's *Before European Hegemony* provides an example of this position. Abu-Lughod seeks to understand the rise and decline of various parts of the thirteenth and fourteenth-century world. No set of consistent explanations based on the internal attributes of the various areas is capable of providing insight into the disparate questions of why China withdrew from global interaction at the height of its power, why the Champagne trade fairs of central France declined while other parts of western Europe grew, and why the great cities of western Asia (Basra, Baghdad, Cairo and Alexandria) shrunk in size and lost their status as global entrepôts. Abu-Lughod offers an explanation based not on differences between these various areas, but on relations among them. She traces the existence of a global trading system stretching from the silk-producing areas of China to the woolens-producing cities of Flanders. Goods are developed, financed, produced, transported, protected and exchanged in a set of interlocking regions that span the entire system, all vulnerable to interruptions in the global flow. With the decline of that flow, the individual units involved declined as well. The relative ability of certain areas to regain what was lost was based not so much on unique internal structures, but on the degree to which they and their immediate trading partners were affected. Under the circumstances, prior centrality became a curse, prior peripherality an advantage. It is not the constitution of a given social system that determines rise or decline, but relative position in a global system and the overall health of that system.

In the same light, André Gunder Frank's *ReOrient: Global Economy in the Asian Age* traces the rise of Europe in the fifteenth through the nineteenth century to the acquisition of American silver with which to trade with economically superior Asia. The tons of silver mined in central and South America never stayed in western Europe. Silver traveled both east to Spain and Portugal (then quickly to Italy to service debts or pay for eastern imports), and west to Asia as direct payment for Europe's imports. Asia's earlier and successful development, based in part on cheap labor and efficient water transport, led to increasing wealth. Over time, and exactly because of the availability of American silver, we find increasing inequality, a lack of incentive to adopt technology, declining effective domestic demand, and economic stagnation.

Political disarray followed and opened the door to Europeans who desperately wanted and hence paid dearly for Asian goods. European colonialism could provide for unending streams of American silver (and other slave-based colonial profits) to Asia. Europeans profited from these goods, developed long-distance transport to facilitate this trade, and began to take over the lucrative inter-Asian country trade. Europe's own technological revolution followed, but not for any uniquely European reasons. Europe's relative poverty and its colonial policies rendered labor scarce by global standards, raised wage levels, and decreased the cost of investment capital. Industrial innovations followed, reducing the cost of European goods substantially, and Asian dominance succumbed to these newly industrializing western countries.

To understand Europe's rise, Asia's decline, and the relative timing of each, one needs to look not at Europe or Asia, but at the interaction of the two. Analyses that ignore this interaction cannot provide an adequate explanation of the nature or the timing of these events. Such analyses will be shackled to explanations that rest on alleged differences between these areas. Frank faults both Marx and Weber (and their intellectual progeny) for failing to avoid this trap.

2) Transdisciplinarity

World-system history has shown a significant tendency toward transdisciplinarity. The complexity of such analyses are well illustrated in the work of Arrighi, and Christopher Chase-Dunn and Thomas Hall. Arrighi, for example, is concerned with the renewed power and global reach of finance capital as a recurrent historical theme. Similar eras of financial dominance (and breakdown) may be noted during the eras of Dutch and Italian prominence, but they rose and prospered for different reasons. The Dutch overcame the dominance of northern Italian social, political and economic organization for two reasons. The trade of Genoa and Venice was oriented more toward the east, which was initially a significant advantage, but as Asian trade routes collapsed in the mid-fourteenth century, Italian power declined as well. The Dutch also internalized protection costs, depending on their own resources (northern Italian cities had depended upon Iberian arms in the west). Arrighi notes a consistent increase in the internalization of costs across each subsequent hegemon, allowing for greater control and predictability. Subsequent system leaders turned aside challenges from competitors that gained power primarily through control of territory (e.g., Spain, France and Germany). Once a hegemon is in place, eras of stable expansion and turbulence, driven by the limits of accumulation, form upswings and periods of stagnation that are dealt with by social, political and economic policies until the limits of the policy process finally replace the old dominant power. Arrighi's careful tracing of the complex interrelationships of these processes requires concern for strategic political, economic and social processes not as separate realms but as fully integrated processes. Financial crises, hegemonic decline, and the rise of new leadership are easier to apprehend as a function of the unified reactions of all three than as a result of any one, or even all three separate but overlapping analytics.

Chase-Dunn and Hall also reject any artificial differentiation of social, political or economic spheres. In *Rise and Demise*, their goal is to compare "world-systems," or interaction networks. They identify four major sets of interaction networks: those for bulk goods, prestige goods, political and military goods, and information. These networks fail to fit nicely into the traditional subject matter of any single field. Using a long-term comparative method, they focus their attention on several phenomena. First, they seek to understand the tendency of world-systems to expand their interaction networks, and then see those networks stagnate or regress. This is world-system "pulsation." Second, they want to consider the manner in which world-systems undergo changes in the deep structural logic of social reproduction. Neither of these questions may be addressed from any single traditional disciplinary perspective. The model of pulsations crosses several disciplines as does their model of world-system transitions. Far from positing autonomous social realms, Arrighi, and Chase-Dunn and Hall favor a unified analysis. They consider the large-scale problems of crisis and systemic change without regard for traditional disciplinary boundaries. Elements of culture, material life, and political authority do not just "interact" but are mutually constitutive in ways that merit understanding on their own terms.

3) Inclusive Temporal Scope

Without a more complete sense of global phenomena, little is likely to be learned about the kind of large-scale structuring process that creates the environment within which global actors contend. It is for that reason that students of world-system history search the historical record. The search is not for independent data bits, but for examples of processes and evidence of changing structures. The suggestion that one identify a phenomenon, and then trace it back through time as far as one can go, belies a difficult methodological problem. How do we recognize equivalent phenomena? Does “war” or “trade” or “politics” among ancient pastoral peoples emerge in the same way or mean the same thing today? If not, what do we learn by tracing these phenomena back through the millennia? Could we not simply lose ourselves in the corridors of history to no end?

There are reasons to plumb the deep historical record. Certain long-term processes, like hegemonic rise and decline, are difficult to grasp in part because of their duration and complexity and in part because of the small number of cases we have to consider. More cases would afford scholars a better chance to consider a variety of issues: Are hegemonic cycles decreasing in duration, what prompts decline, what are the attributes of likely challengers, does war always occur as a result of decline? Earlier bouts of hegemony may well lack attributes considered usual today, and the lack of those attributes may help explain trends or differences that appear in the record. Together with more instances of similar processes, more instances of different processes in the historical record will also help us understand the nature of the global system. The number of wars, the amount of trade, and the duration of periods of growth or decline may well change as a result of greater levels of interaction evidenced today. The incidence of these and other attributes may also vary systematically relative to other general variables. We have no way to engage in meaningful comparison of phenomena over time unless we track the historical record.

The need for examples to feed our search for phenomena that are alike and those that are different does not answer the question of how far back we ought to search before the question of what constitutes equivalent or relevant examples emerges. I contend that tracking social phenomena back through time provides the only legitimate way of deciding what are and are not relevant examples. We cannot conclude that only phenomena that emerged over the last five hundred years are suitable for study simply because those any older sound ancient to us or go back beyond one of our myriad and largely arbitrary historical markers. Comparability is partly an empirical question that only description and further analysis can address. Our ability to understand global relations outside the European context also requires some historical digging. European-centered concerns dominate our study of global history from about the sixteenth century onward. To find independent, non-European systems that have different generative elements and possibly different developmental logics requires analyses of several centuries. Any later, and European goods, ideas and arms came to dominate either the areas or the historians whom we consider today. World-system history is a necessary part of non-Eurocentric analysis.

Abu-Lughod's *Before European Hegemony* once again provides an example of the value of historical reach. One of her major concerns is the argument over whether a hegemon is necessary for the maintenance of stability in a global system. Although China appears larger, richer and stronger than any other power in the system at the time, its military reach did not extend across the entire system. Large zones of peace and a general stability existed as a result of several large powers facilitating pacific linkages with other regions. The subsequent increase in wealth allowed for the maintenance of order, or facilitated a regional demilitarization, as in the Indian Ocean. This stability did not last. Although a hegemonic power was not necessary, the dominance of the various regional powers was. The downturn of the mid-fourteenth century brought several kinds of conflicts. Trading empires like the Chola on India's coast were weakened and then attacked by inland groups more accustomed to acquiring wealth by territorial expansion. The heretofore peripheral Europeans also found their way around Africa and into the still relatively rich and peaceful Indian Ocean, which they plundered with ease. Hegemons were not as necessary to the maintenance of stability in times of trade-based prosperity as they were once that prosperity began to wane.

Frank makes another interesting temporal assessment in *The World-System: Five Hundred Years or Five Thousand?* Where he questions the concept of "capitalism" and trace the oft-alleged "capitalist" elements of the modern world-system back well beyond its traditionally accepted sixteenth-century genesis. Present as well are long cycles of increasing global interaction, growing wealth and enhanced stability on the one hand, and of reversion to local-level interaction, declining wealth, increased rivalry and war on the other. If the onset of capitalism is not a break point in global development, then the entire idea of "modes of production" and even of "class conflict" lose their centrality. The stability or instability of the system does not rest on contradictions that mount within the mode of production. Frank takes a first step at outlining the long upswings and down-swings that may be found in the historical record and hypothesize that we ought to see this dynamic all the way back to the onset of regularized, large-scale, long-term transregional trade some 5,000 years ago. Instead of "modes of production" Frank asks us to consider far older and more fundamental "modes of accumulation".

4) Globocentrism

Eurocentrism does not simply affect how we address questions, it defines which questions are and are not worthy of being asked in the first place. In *The Wealth and Poverty of Nations*, Landes offers an explanation of the rise of the West. The predominant wealth and power of the region are suggested to be the result of a unique culture that fostered innovation and protected private property. The rest of the world did not rise because it lacked those traits. The eminent global historian William McNeill refers to the Landes argument regarding non-European cultures as "unabashedly triumphalist" and warns of "dubious assertions". But Landes has produced more than just a bad history, he has produced a dangerous one. It leads us away from a methodologically sound comparative study of trends and processes, and toward a conceptualization of an allegedly inherently superior culture that must face a random and endless array of ad hoc challenges to the maintenance of its global dominance.

What questions emerge from this culturalist view of western dominance? At a minimum, we can identify those questions that cannot emerge. If intrinsic cultural traits allow for the currently dominant global region to have enjoyed its rise, what are we to make of the existence of prior dominant powers? Were these societies with good, but not perfect, cultural attributes? Where was the effect of the superior culture of the West during earlier periods when other regions were dominant? What of the claims of representatives of previous dominant social orders that they constituted the ultimate form of social organization? Is it not all too easy to ignore the similarity of such claims by Ottoman or Chinese representatives of earlier eras? From such a perspective, the period before the rise of the West must be ignored or studied only for evidence of select differences by those wishing to explain the present. Processes relevant to the dominant culture are assumed to be unique. The emphasis of any historical or comparative scholarship must be on dissimilarity, not continuity. The questions that appear relevant from such a Eurocentric perspective will be different from those that would be otherwise identified as important. Upheaval may be blamed on a "clash of civilizations" (Huntington) in a popular argument that is nonetheless contrary to the empirical evidence.

CONCLUSION

Contemporary international politics continues to be plagued by state-centrism, conceptions of politics as an essentially autonomous process, an overemphasis on the near term, and Eurocentrism. As a result, the ability of traditional international politics to deal with important issues like globalization, political-economic crises, and systemic instability, as well as how we might view these and other challenges is much compromised. An alternative school of thought – world-system history – and the manner in which its proponents avoid the shortcomings of traditional international politics and address important areas of contemporary concern is not without its methodological challenges as well. Traditional social science approaches are unhelpful. Criteria like parsimony, methods centering on hypothesis testing, and predictive modeling are inappropriate given the complexity of the phenomena under study, and the age and heterogeneity of the perspectives in question. Statistical and heuristic modeling, and historical narrative – all designed to be sensitive to non-linear, cyclical, and evolutionary processes – are appropriate tools in this regard. In the end, the level at which explanations based on the propositions of world-system history are successful rests with the relative levels of completeness they achieve. The question we must ultimately ask of all perspectives remains that of how much of the complex social universe they allow us successfully to apprehend.

Theoretical Insights from the Study of World History

Stuart Kaufman, Richard Little and William Wohlforth

In *The Balance of Power in World History*, 2007

The goal of this volume is to assess the workings of a variety of international systems across thousands of years, in order to learn how the concepts of balance of power and its opposite, hegemony – the most central issues in international politics and IR theory – actually worked. We begin by addressing competing theoretical traditions' assumptions about the frequency of balance and hegemony and find support neither for the neorealist assertion of the universality of balance of power nor for the English School claim about the normality of hegemony. We find instead that balanced and unbalanced distributions of power seem roughly equally common. What is universal in international systems is a mix of hierarchy and anarchy within them: systems vary in the degree to which they are hierarchically rather than anarchically organized.

To the degree systems are anarchical, we next consider the degree to which the logic of anarchy may vary from system to system, and relatedly, the degree to which international norms or international society modify the behavior of states in the system. The last systemic-level factor we consider is geography, finding that while the topography of a system does not have consistently important effects, its size does: the opportunity for a system to expand in size seems almost a necessary condition for it to remain balanced. We then turn to examining unit-level variables, which turn out not only to drive most state behavior, but also to a large degree the evolution of the systems themselves. Our cases show that not only is military expansion practically a universal behavior, but that such expansion is frequently characterized by myopic advantage-seeking, rather than aimed at long-term system maintenance (balancing), even among rivals to potential hegemons. The pattern of advantage-seeking is a major reason why balanced systems routinely break down, and why systemic hegemons frequently squander their advantages.

In the face of the frequent failure of balancing, the key variable limiting the expansion of great powers seems to be administrative capacity – the ability to administer territory efficiently, including newly conquered territory, in order to extract resources and make power cumulative. What matters is the outcome of a governance arms race – who can overcome domestic political obstacles to develop efficient processes of government, assert a plausible legitimating ideology of rule, and avoid the internal disunity that can destroy states, either by itself or in the context of outside pressure. The outcome of this governance arms race, in turn, depends in large part on principles of unit identity. Peoples that cling to their local identity often suffer a sad fate: they are inclined to balance, but not to trust their neighbors, so balancing tends to fail. They then tend to rebel frequently against their hegemonic master, and so face the hegemon's wrath – but not before weakening the hegemon itself, sometimes significantly. Each of these patterns is explored below.

The Balance of Power: Universal, Normal or Rare?

The first and simplest issue this volume addresses is the neorealist assertion that "hegemony leads to balance . . . through all of the centuries we can contemplate" (Waltz, 1993). The evidence we provide is sufficient to reject that hypothesis as a serious assertion about international relations. Every one of the cases studied here ended in the establishment of lasting hegemony by a single power over what had previously been a multipolar balance-of-power system. Assyria destroyed and conquered its rivals Babylonia, Urartu and others. It was eventually replaced by Persia's two century-long dominance of an even larger area, creating an empire whose expansion the Greek city-states could limit, but whose overall power they could not begin to match. In similar fashion, Rome destroyed all of its Mediterranean rivals, Qin conquered the Warring States system, Magadha united the Indian system into the Mauryan Empire, and the Aztecs and Incas asserted their hegemony over all their regional rivals. Modern China retained its hegemony over East Asia for many centuries. The balance of power is not a universal constant. On the contrary, the ubiquity of hegemonic outcomes would seem rather to support the contention of the English School that some form of hegemony rather than balance is the norm in international history. If this is right, then not only is the balancing proposition wrong; so is the first assumption of all major American schools of IR theory – realism, liberal institutionalism and constructivism – that the international system is axiomatically anarchic. This raises a methodological issue: do we have a problem of selection bias? It could be argued that the systems discussed here are not typical, and that the modern European norm of multipolarity is really more common than our study might suggest. To address this question, we have compiled a data base of 7,500 system-years of international history to try to measure the frequency of different distributions of power in international systems. In this data base, we code polarity in international systems decade by decade from different parts of Eurasia: the Near East from 1500 BCE to CE 390; the classical-era Mediterranean from 400 BCE to CE 390; East Asia from 1025 BCE to CE 1850; India from 400 BCE to CE 1800; and modern Europe and its global successor system from 1500 to 2000. This database assesses a majority of all international history and provides a reasonable basis for comparison. The table below summarizes the polarity of international systems for 750 decades (7,500 years).

Frequency in Decades of Different System Polarities

System	Nonpolar	Multipolar	Bipolar	Unipolar	Hegemonic
Ancient Near East 1500-400 BCE		60	6	38	6
East Asia 1025 BCE-CE 1875	5	107.5	35	85	57.5
South Asia 400 BCE-CE 1810	17	26	68	73	37
Classical-era Mediter. 400 BCE-CE 390		13	1	18	47
Modern Europe CE 1500-2000		41	8	1	
Total Frequencies	**22**	**247.5**	**118**	**215**	**147.5**

The data summarized above are not supportive of the most common thinking about the balance of power. First, balanced international systems are not the norm: "balanced" multipolar and bipolar systems are almost exactly as common (365.5 decades) as are "unbalanced" unipolar and hegemonic systems (362.5 decades). If anything, this conclusion is probably overly generous to multipolarity: stricter coding rules would certainly decrease the number of multipolar decades in the early periods. A second generalization is that, the modern system aside, there is little variation across space in balanced versus unbalanced systems. The East Asian system is exactly equally divided between balanced and unbalanced periods (142.5 decades of each). The South Asian system is also roughly equally divided between balanced (94 decades) and unbalanced (110 decades) periods. And if our classical-era Mediterranean data is folded into the ancient Near East data to form a single two-millennia-long series for what Wilkinson calls the "Central" international system, the same pattern emerges – relative equality between balanced (80 decades) and unbalanced (109 decades) periods.

A third generalization is that there is over the very long haul a trend toward system consolidation or imbalance over time – or, more precisely, a tendency for the last centuries of any system to be unbalanced rather than balanced. In the ancient Near East, sustained unipolarity was very rare in the 15th through 10th centuries BCE, then shifted decisively toward unipolarity or hegemony under the leadership of Assyria (9th through 7th centuries BCE), Persia (6th through 4th centuries BCE), and Rome (2nd century BCE through 4th century CE). In East Asia, almost half of the unbalanced periods come in the last seven centuries during the Yuan (13th-14th centuries), Ming (14th-17th centuries), and Qing (17th-19th centuries) dynasties, which included 62.5 unbalanced and only 7.5 balanced decades. South Asia follows the pattern less strongly: its most concentrated period is near the beginning under the Mauryan Empire (3rd century BCE); but still the last five centuries are more often unbalanced (31 decades) than balanced (20 decades) under the leadership of the Delhi Sultanate (12th-14th centuries) and the Mughal Empire (16th-18th centuries).

The implication of these data is that the neorealist assertion that "balances form and reform" is only spottily true. Balances frequently form, but they always break down. Sometimes they break down into fragmented or nonpolar systems, but more often in the last two millennia they break down into unipolar or hegemonic systems. The longevity of these systems is widely variable. Sometimes balances of power last for many centuries, as they did in Warring States China and modern Europe. In other cases, like the multipolar Near Eastern system of the 6th century BCE, or the Mediterranean balance of the 3rd century BCE, balanced systems are relatively brief interludes between relatively longer-lasting periods of unipolarity or hegemony. Similarly, some hegemonies (such as Alexander the Great's) are extremely brief, while others (such as Rome's or Han China's) last for centuries. With regard to polarity, therefore, what international relations theory must explain is why balanced and imbalanced systems are roughly equally frequent, and why shifts occur from one to the other.

Variations in Anarchy and its Logic

The finding that balanced and unbalanced systems are roughly equally common disproves the English School assumptions that anarchy is a fragile structure and that some form of hegemony is the most common state of international systems. However, a related insight *is* firmly supported by our evidence: there is always hierarchy within anarchy, and it is very common for more international relations in a system to be hierarchical than anarchical. Watson (1992) asserts a conception of international relations in which there is no sharp line between politics within and outside the empire, but rather gradations of control. This insight is reinforced when we consider that Watson's definition of hegemony – a situation where units are nominally independent, but where the foreign policy of one state is severely constrained by the other – fits the realist understanding of unipolarity. In sum, a realist's unipolarity within anarchy is the English School's hegemony (a weak sort of hierarchy). Realism's rejection of this notion is based on its odd reification of the legal notion of state sovereignty – odd, that is, for a school of thought that otherwise rejects on principle any role for international law and asserts its exclusive focus on the realities of power rather than the abstractions of law. The realities of power are that hierarchical relations between nominally independent actors are entirely normal.

For example, while the degree of Assyrian control over Babylonia was often arguable and varied significantly over time, it remains true that for decades Assyria had some degree of control over Babylonia, but that at the same time this critical relationship has to be understood as a part of the international politics of the system, not merely of the internal politics of the Assyrian Empire (which made a clear distinction between "the Land of Assur and the Yoke of Assur"). Before Rome was hegemonic, the Seleucids and the Ptolemies had to calculate carefully how far they could go in their conflicts with each other without provoking intervention by the superpower. Even the Aztecs, in spite of their well-earned reputation for bloodthirstiness, established a system that was more suzerain than imperial, allowing more autonomy to its subjects than one might expect. Early modern China's relations with its satellites were functionally similar to Rome's with the Ptolemies and Seleucids in the 2nd century BCE (the emperor expected little more from his neighbors than restraint and an exchange of gifts) even though they were nominally more hierarchical (since he expected that their envoys would kowtow to him and verbally acknowledge his primacy). These relationships cannot be understood without the English School's recognition that anarchy and hierarchy are not mutually exclusive categories in international relations, but rather form a continuum.

A separate question is the "logic of anarchy" that anarchic systems display. Realists claim to assume that international politics is invariably a dog-eat-dog affair of unlimited violence and unending war. English School theorists argue that in some international systems, states may share enough intersubjectively understood norms to constitute an international society. Constructivists define these as simply different possible logics of anarchy: the realist version is "Hobbesean," while an international society's logic is "Lockean." Though our evidence is not adequate to distinguish Hobbesean from Lockean international systems in every detail, there is one pivotal distinguishing feature that we can measure: the degree to which states in the system accept each other's existence so that state "death" becomes improbable or rare, especially among major powers, and international relations are to that extent restrained.

Realists would argue that our evidence supports the Hobbesean understanding of anarchy as the transhistorical norm. Assyria, Rome, Qin, Magadha and the Inca annihilated most of their adversaries, and even the Athenians within Greek international society annihilated defiant Melos late in the Peloponnesian War. Persia, early modern China and the Aztecs were more restrained, more often settling for acknowledgment of suzerainty instead of direct rule, but even this could be understood as reflecting sober judgments of limits on their material power: the Persians surely knew they were overstretched, for example, and the Chinese apparently recognized that they probably would be should they attempt to conquer Korea or Vietnam. While it is clear that most of the systems we discuss were Hobbesean in nature, English School or constructivist theorists would nevertheless argue that some of the systems did contain international societies (i.e., were Lockean in logic). The clearest example is early modern China, which evolved a widely accepted set of norms and practices that kept the international system remarkably stable, substantially limited the frequency of war, and ensured the continuing survival and autonomy of the smaller state-units. The Aztecs, too led an international society, building a system that was more suzerain than imperial in nature. This meant that, like the modern Chinese, they allowed many of their subject peoples continued autonomous existence; and like modern Europe, had rules that limited the ferocity of war (though not of the treatment of prisoners of war).

A case can also be made for two separate international societies in the Greek-Persian case. Persia, for its part, carefully negotiated the terms of incorporation into its empire of the Ionian Greek city-states, the Egyptians and the Jews, allowing considerable autonomy to these and presumably other subject peoples. Among the Greeks, Thucydides seems to find Athens's treatment of the Melians remarkable precisely because it violated a longstanding and long-observed norm of treatment of Greek prisoners. Finally, it is worth noting that these cases are not the earliest known international societies: though the case is not included in this volume, the Amarna period system including Egypt, the Hittites, Babylonia and others c. 1500-1200 BCE also had a well-documented system of international norms and generally moderate relations among great powers. While Hobbesean anarchies seem to be the most common sort, then, Lockean anarchies – international societies – are not terribly rare in international history.

Additionally, the evidence seems to indicate a stabilizing effect for international norms: when there exist international norms supportive of the international order, that order appears frequently to be more peaceful and longer-lasting than those that lack such supportive norms. For example, the Assyrians lacked legitimacy among subject and neighboring peoples and hence endured as hegemon for less than a century. Their Persian successors, in contrast, gained legitimacy by showing greater toleration, and in partial consequence lasted more than twice as long. The strongest case of legitimized hegemony, of course, is the early modern Chinese example, since there a relatively peaceful and stable Chinese hegemony continued, albeit with interruptions, for some five or six centuries. The modern European case illustrates an alternative role for international norms: there, as English School theorists and traditional realists have long argued, it was the balance of power that was normatively favored, and that norm contributed to the maintenance of the balance over the centuries of European multipolarity.

Geography and System Size

Ever since the geopolitical speculations of Mackinder (1904), the popularity of geography as an explanatory variable in international relations has fluctuated dramatically. Attention to this variable has modestly increased recently in wake of Mearsheimer's (2001) assertion about "the stopping power of water". Our cases, however, reveal no consistent support for any specific geographical hypothesis. For those cases in which the stopping power of water is potentially relevant, only one case supports the contention (Greece's resistance to Persia), while four do not. By Mearsheimer's logic, for example, the Mediterranean should have obstructed Rome's conquest of Carthage and later Egypt; instead, Rome turned "Our Sea" into a highway of control. Similarly, Kang finds that the Sea of Japan did not prevent Chinese hegemony over Japan; and oceans certainly failed to prevent the Spanish conquest of the Aztecs and Incas. At least since the days of Minoan Crete, empires have often found that water provides transportation power rather than stopping power.

Other specific hypotheses about geography similarly fail to find support. One can develop logical arguments suggesting either that marchland powers or centrally-located powers have the better chance of becoming system hegemons, and it turns out that both propositions find roughly equal support. Assyria, Rome and the Aztec and Inca heartlands were all centrally located. In these cases, geographical centrality divided the growing empires' enemies, an effect that proved more important than the "two-front problem". On the other hand, Persia, Qin and Magadha were all marchland powers, exploiting their positions at the edge of their international systems to defeat their rivals in detail. Mountains have no invariable effect, either. While tribal peoples are often limited either to mountains or to lowlands, hegemonic empires have succeeded in controlling both at least since Assyria surmounted the Taurus Mountains to defeat Urartu. Similarly, Rome crossed the Apennines, Alps and Pyrenees, the Aztecs controlled mountainous central Mexico, and the Incas conquered the Andes. Neither does the original base matter: Magadha began in the lowlands of Bengal, Assyria in the plains of Mesopotamia, and the Aztecs in the Valley of Mexico; but Rome emerged from the foothills, and the Incas in the Andes Mountains.

The only important generalization about geography, then, is that successful hegemons develop or find the communications technology to overcome its obstacles. The Romans began as land dwellers but conquered the sea to conquer Carthage, and Caesar crossed the Alps to conquer Gaul. The Incas developed an astonishing road network enabling them to tie together their imperial possessions across hundreds of miles of mountainous terrain. And a succession of Mesopotamian empires achieved similar results in the Fertile Crescent centuries before the Romans came along to take credit for the idea. What turns out to matter more than topography for explaining international systems is the flexibility of system borders. It may be that a necessary condition for maintaining balance in an international system is flexibility of the borders of that system. For example, Assyria's brief period of hegemony ended only when the Medes organized the uplands of Iran into a new empire outside the previous boundaries of the system: the Medes first balanced and then destroyed the Assyrian Empire. Persia then ran the tables of the old international system, to be checked not only by the Hellespont, but by the Hellenes beyond it in what had been a different international system. And Chinese emperors' hegemony was repeatedly checked not from within the system, but by tribal peoples (such as Mongols and Manchus) from outside it.

System expansion is not, of course sufficient to maintain balance: Rome's world expanded to include the Hellenistic empires of the eastern Mediterranean after it defeated Carthage, but the Romans quickly came to dominate even that larger system. Still, system expansion may be necessary for maintaining balance over the long haul: even the modern European balance was maintained only by the repeated introduction of new powers on the flanks, most importantly Russia and the United States. We assess that the rigidity of the borders of the international system contributed importantly to the hegemony of Persia and Magadha, and was a necessary condition for hegemony in every other case we examine.

State Behavior: Expansion, Balancing, Buckpassing, Bandwagoning and Advantage-Seeking

Realist theorists hypothesize three typical behaviors for states in the international system. Offensive (Mearsheimer, 2001) and hegemonic stability (Gilpin, 1981) realists assert that military expansion is the norm. In Mearsheimer's typically stark assertion, "great powers have aggressive intentions. . . . [S]tates do not become *status quo* powers until they completely dominate the system". Defensive realists (Waltz, 1979; Walt, 1987) argue, in contrast, that the most common state behavior is balancing against great-power expansion. Realist critics of the balancing proposition (Powell, 1999; Rosecrance, 2003) argue that collective action problems systematically interfere with efforts at balancing, asserting that in many cases states may find buckpassing or bandwagoning with the expanding power to be the safer course – or the more profitable one (Schweller, 1994). Our findings show that all of these courses of action are commonly followed, often to the point where they become foolish advantage-seekers.

First, almost every one of our cases involves, as Mearsheimer and Gilpin hypothesize, an expansionist state with limitless ambitions that defeats and subordinates lesser breeds of expansionists. Ruthless expansionism was so common that it became characteristic of the system itself. This assessment is no less true of the systems that came to be led by Assyria, Persia, Magadha, the Aztecs and the Incas (and their successors, the Spanish). Interestingly, though, in most of these cases hubris eventually brought forth rivals, as expansionism was pursued to self-defeating lengths. Both Assyria and Persia were certainly suffering from imperial overstretch by the time they reached (but never fully pacified) Egypt. Qin's

A related finding is that while Mearsheimer and Gilpin are right that states tend to expand where they can, expansionist moves are frequently unwise because they either disrupt a balancing coalition or represent overexpansion by the dominant power. "Bandwagoning for profit" is one subtype of advantage-seeking. In the Assyrian case, both Israel and Judaea sometimes indulged in this, seeking gains at each other's expense while their northern neighbors were trying to balance Assyria; the result was that Israel became the Ten Lost Tribes. Qin's neighbors repeatedly pursued advantage-seeking, scrapping for each other's territories while Qin grew and ultimately destroyed them all. A stark example is Wu, which captured its rival Chu's capital in 506 BCE, but overexpanded and was in its turn conquered by its neighbor Yue three decades later. Thebes's decision to bandwagon with Persia was also an example of advantage seeking, even though Persia lost. This is a characteristic feature of advantage-seeking: it is unwise regardless of the fate of the larger partner. If the larger partner loses, the advantage-seeker is exposed to retribution; if the larger partner wins, it is likely to swallow the advantage-seeker.

Advantage-seeking by hegemons have the same quality: low benefit in case of success (usually due to overexpansion), but high cost in case of failure. Assyria's conquest of Egypt was advantage-seeking, fortunately (for both sides) settled by the rise of an Egyptian regime willing to bandwagon with the superpower after the withdrawal of Assyrian troops. The Assyrian annihilation of Elam turned out to be more costly, as it helped open the way for the rise of the Medes, who eventually destroyed Assyria. Xerxes's invasion of Greece was similarly a case of advantage-seeking for an already-overextended empire, though its consequence were less dire. Athens's Sicilian expedition during the Peloponnesian war may be the most famous such case of classical history, as Athens did not only lose its army; it gained a new enemy that played an important role in its ultimate defeat. Roman history is also replete with advantage-seeking, such as the invasion of Parthia by Crassus leading to the defeat at Carrhae in 53 BCE, and the invasion of Germany leading to the loss of three legions at Teutoberger Wald in 9 CE. The classic examples of "successful" hegemonic advantage-seeking are also Roman: the campaigns of the Emperor Trajan, who annexed Dacia and Mesopotamia early in the second century CE, only to have his successors abandon both places as too costly to hold. All of these versions of realism, therefore, share the same qualities of partial descriptive accuracy but general prescriptive failure. States typically expand, but continuing to do so leads to advantage-seeking. Other states often try to balance but are hampered by would-be allies' advantage-seeking (bandwagoning for profit) or buckpassing. Restraint and cooperation work better, but are less frequently found.

Self-Strengthening Reforms: Administration and Legitimation

If balancing is not a dominant strategy, frequently failing due to collective action problems and difficulty identifying the hegemonic threat, why are balanced international systems even as common as they are? Why is the English School not correct that hegemonic systems are the norm? The studies suggest that the critical overstretch is demonstrated by the fact of its immediate collapse after the death of the first Qin emperor. The hegemons that endured, in contrast, were the ones that knew when to stop. Most starkly, early modern China consistently resisted any temptation to invade its neighbors, and therefore maintained its preeminence for centuries. Similarly, the greatest Mauryan emperor, Ashoka, announced after a great military victory his conversion to Buddhism and his dedication to virtue rather than expanded power: as clear a statement as is possible that enough is enough. The first Roman emperor, Augustus, made the same judgment, and while his advice was not always followed, it is notable that most of the later Roman expansionist efforts (e.g., central Scotland, Dacia, Mesopotamia) were eventually quickly abandoned.

Countering these rising hegemons were rival powers that usually tried a balancing strategy and uniformly failed. The obstacles they faced, due to the collective action problem, were formidable: it is always tempting to pass the buck to other potential balancers, or to "bandwagon for profit" by joining the expansionist power. Clever rising powers exploit the collective action problem facing its rivals to counter the balancing strategy with divide-and-conquer tactics. Sometimes it is unclear which of several expansionist great powers is the greater threat – a problem further complicated if the greater threat to a particular balancer is not the same as the greater *systemic* threat. All of these phenomena played important roles in the failure of balancing efforts, with the free-riding problems particularly common. The Aramaean city-states facing Assyria were never able to stand together for long against the superpower, and indeed some called for Assyrian help against their local rivals, providing Assyria with the chance to divide and conquer. Many of the Greek city-states bandwagoned with Persia (as did Thebes) or passed the buck (as did Argos). Qin made divide-and-conquer an explicit strategy against neighbors that frequently bandwagoned with it against Qin's victims, or squabbled with each other while Qin expanded.

The problem of identifying the rising hegemon was slightly less common in our cases, but very important when it occurred. By the time the medium powers of the Greek east no longer needed Rome to balance more immediate threats, they found that no one left could balance against Rome; a crucial stage in Qin's rise came when it joined a balancing effort against then-hegemonic Qi. Only after the defeat of Qi was the Qin threat plain, and by then it was too late. In other cases, this problem of threat identification played a lesser but still important role: Assyria's resurgence came so quickly that southern and central Syrian states could be excused for failing to perceive its gravity until after the defeat of the Arpad-Urartu coalition – by which time it was too late for balancing. The Aztecs and Incas also rose in the context of the collapse of previous hegemons, so the gravity of their rise, too, was probably not fully appreciated in time. The key finding here is that while balancing is a very common strategy, it is not a dominant one: it often fails, in part because some potential balancers typically choose to do something else.

variable determining the rise of a hegemon is administrative capacity. As Gilpin noted, conquests tend to generate diminishing marginal returns over time due to a "rising cost of expansion". It is this dynamic – states' inability to digest their conquests – that tends to limit most states' expansion. Only when great powers develop the ability to administer new conquests and extract resources from them to fuel further expansion – when, that is, expansion becomes cumulative – does hegemony become possible, and indeed likely. Developing this capability requires something akin to a social or political revolution, but is a necessary condition for hegemony. Assyria was unable to hold onto its conquests in the Near East until internal reforms made that possible. Rome rose because it combined the strengths of traditional Republican institutions with innovations that gave it a unique capacity for inclusion of foreigners, enabling it to continue to expand its resource base. Similarly, Magadha was the most administratively durable of the ancient Indian states; and Qin, with the self-strengthening reforms of Shang Yang – economic reforms and military conscription as well as bureaucratic innovations – developed the most penetrating and brutally effective state structure in its international system. The Incas were also remarkable for the sophistication of their system of rule, as was early modern China for its elaborate bureaucracy and extensive political structure.

Realist theory hypothesizes that rival states should emulate such self-strengthening reforms, engaging in "internal balancing" to match any advances made by a potential hegemon. The various new institutionalist theories, however, posit a host of reasons why this is not likely to be so easy. Logrolled political coalitions are likely to block reforms that might increase state power at the expense of powerful domestic interest groups. Path dependencies may mean that the rival state's institutional structure is so different from the system leader that copying the leader's institutions is effectively impossible. Political and institutional cultures may be an obstacle. And so on. The evidence generally supports the institutionalist objections, rather than the internal balancing hypothesis. Rome provides the most extreme example: Hellenistic empires and Greek city-states were such fundamentally different beasts that it is meaningless to suggest they "should" have copied Roman institutions. Babylonia might have had a chance to emulate Assyria's administrative structure, but it was unable to resolve the conflict between old Babylonians and Chaldean newcomers until the very end of the period of Assyrian hegemony. And while Qin's neighbors did try to emulate its innovations, Qin simply did better, successfully implementing the most comprehensive military, economic, and administrative reforms known in ancient Chinese history.

A related factor that may promote either international hegemony or diversity is the political unity of the states in the system. On the one hand, divide-and-conquer strategies can work not only among states in the system but also within them. The histories of most of the hegemons discussed here are replete with examples in which prohegemonic factions successfully appealed for assistance from the hegemon against their local rivals. Regime change is a common hegemonic tool, but it works best only where significant political factions are willing to collaborate with the hegemon. The most vivid examples in these cases include Assyria's repeated efforts to place pro-Assyrian leaders on the Babylonian throne, and Qin's bribery of key officials in rival states, but Romans, Persians and others commonly resorted to the same tactics. On the other hand, political division can also occur within the system leader, weakening the leader and moving the system toward diversity. Qin's rivals repeatedly suffered from this problem, as did Assyria and Persia. Later Roman history is of course largely the history of one civil war after another as generals competing for the imperial throne, and civil war in China was one of the few factors that could shake early modern China's hegemonic grip on its world. The Inca Empire also had this problem: Atahualpa had just been involved in fighting a civil war against his brother when he marched to face Pizarro. Whether we conceptualize political unity as a function of administrative capacity or as a separate factor, its role is clearly critical.

Another key factor that interferes with the effectiveness of both internal and external balancing is the diversity of units. Systems frequently include different types of units based on different principles of legitimacy and unit identity. The effect is differences in size and methods of rule that may make both internal and external balancing unworkable. The most obvious problem is size: city-states are necessarily smaller and weaker than are empires. And city-states were what several of our rising hegemons faced: Assyria against the Aramaeans of modern Syria, and Persia and Rome against the Greek city-states. Such smaller units must necessarily form larger coalitions to generate enough military power to balance an empire, and as the number of units in the coalition rises, so do problems of coordination and collective action. Most importantly, though, city-states cannot expand – they cannot emulate empires by increasing in size and scale to increase their capabilities without ceasing to be city-states. Fundamentally, they are not like units.

The main obstacle to city-state expansion is not administrative but ideological – the principles legitimating unit identity. The Greek case is the most famous here: the Greeks simply considered the *polis* to be the most advanced form of social life, and a key marker distinguishing them from barbarians (such as the Macedonians). The idea of joining an empire was what their foreign policies aimed at preventing. A similar dynamic operated among the Aramaeans. Furthermore, for those whose governing ideology insisted on a democratic or republican form of government, such self-rule was generally understood to be possible only in small units such as city-states. Republican city-states had the opportunity to form larger leagues in a process called co-binding, but these formations also had limited success: the Vajjian Confederacy was an early victim of Magadha's expansion, and its Hellenic cousins such as the Achaean League were virtually defenseless against the might of Rome. Thus in at least four of our cases, empires' rise to power was facilitated by some units' commitment to political forms that required small size.

The significance of local identity principles is not, however, ended by the conquest of such units by empires. Though easy to conquer, such units frequently prove difficult to hold because the local population refuses to accept the legitimacy of the empire. In short, peoples with strong local identity principles tend to rebel against their conquerors. Babylonia, with its distinctive identity, repeatedly rebelled against Assyrian rule, a problem Assyria never solved. Persia, too, faced multiple rebellions from those of their subjects with strong local identities – Egyptians and Greeks as well as Babylonians. Rome had to crush Greeks, Jews and other particularistic groups ruthlessly and repeatedly before they finally became docile. The Mauryan Empire never fully controlled the forest polities it encompassed. And the Aztecs finally paid for their oppression of other city-states when Tlaxcallan provided Cortes with a base from which he could operate to destroy their empire. The conclusion here is a melancholy one: though local identities are an important fact characterizing many international systems, small groups' insistence on political expression of those identities is highly costly to them (as well as to their conquerors) when they face systemic hegemons.

Final Words

The fundamental finding of this volume is that international life cannot be understood either as typically hegemonic or as reliably anarchic within a balance of power. The basic starting point for any theory of international relations must be Watson's image of time's pendulum swinging between balanced and unbalanced distributions of power (though Watson was wrong about the center point of the pendulum's swing). If we want a theory of the international system that explains the shape and behavior of that system, the central problem of that theory is to explain the motor that drives the pendulum's swing: what is it that makes the pendulum swing from diversity to hegemony and back again?

We think the evidence in this book suggests the outlines of an answer. We find no one dominant factor driving the pendulum, but rather a number of factors pushing in each direction. The location and velocity of the pendulum at any one time seems to be the result of the sum of the forces operating on it, the pushes and pulls in each direction. The most important forces pushing the system toward hegemony are the tendency of states to expand their power, and innovations in administrative capacity that enable states to absorb their conquests, making power cumulative. The most important forces pushing the system toward diversity are expansion of the system to include new players, and local identities that motivate some units fiercely to resist hegemonic control. A factor that can push in either direction is norms of an international society, which can work to stabilize either hegemonic or diverse systems.

The modal behavior of states in the international system is not balancing, buckpassing or bandwagoning, but rather expansion continued to the point of advantage-seeking. This means, on the one hand, that hegemonic stability theorists