

Guide to the Scientific Study of International Processes



Edited by Sara McLaughlin Mitchell, Paul F. Diehl and James D. Morrow



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COMPENDIUM PROJECT

Guide to the
Scientific Study
of International Processes

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Paul F. Diehl, and James D. Morrow

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Editors' Introduction

Sara McLaughlin Mitchell, Paul F. Diehl, and James D. Morrow

The Scientific Study of International Processes (SSIP) is a robust group of scholars dedicated to its name. One of the oldest sections in the International Studies Association, SSIP focuses on the rigorous analysis of arguments and evidence. The SSIP community broadly falls into two camps: one primarily addresses the collection and statistical analysis of data, while the second uses mathematical models to elaborate theories of international processes. Neither milieu is exclusive; some members of each group do research that crosses into the other, and members of both research groups examine evidence and arguments outside of the other camp. The common project of the SSIP community brings rigor to the logical structure of theories and the assessment of evidence for and against those theories. This rigor aims at making the creation and analysis of data and the elaboration of theory more visible to the scientific community.

Most research in SSIP focuses on questions of security and conflict, such as why wars occur, which conflict management strategies are most successful, and the consequences of conflicts for future interstate interactions. Historically, those questions drove early efforts at data collection and analysis and model building, including the development of the Correlates of War (COW) Project. As both the accumulation of data sets and the range and sophistication of models have increased, new research has addressed questions from other areas of international studies, such as political economy, international cooperation, and human rights. Recent work on conflict has also focused more explicitly on explaining intrastate violence such as civil wars and protests as well as violence conducted by non-state actors such as transnational terrorist attacks.

We organize this edited volume using the two major research approaches in the SSIP tradition. The first set of chapters in Section I examines methodological issues and approaches in SSIP research. These chapters help the reader understand what methods are used and why they were adopted to answer certain questions. In doing so, the chapters provide a gateway into understanding those methods. Kadera and Zinnes provide a historical overview of the creation of the SSIP section, describing its evolution over the past several decades. Zagare and Slantchev focus on the use of game theory to study conflict processes, noting in particular the use of zero-sum and nonzero-sum models and summarizing the major debates that game theory has helped to settle in conflict studies. Hensel summarizes the major data collection efforts that have been undertaken by the SSIP community including the data sets on interstate and intrastate conflict, crises, rivalry, territorial change, regime type, human rights behavior, and contentious issues. Bennett discusses the benefits and challenges to teaching the SSIP approach to undergraduate students describing the importance of generalizability, the quality and depth of evidence for theoretical claims, and the use of different pedagogical tools in the classroom. Finally, Morrow's chapter identifies some challenges that arise when integrating the two broad approaches of the SSIP tradition and discusses recent approaches such as the Empirical Implications of Theoretical Models (EITM) approach that seek to integrate the traditions.

The second set of chapters, in Section II, examines substantive topics of research that have played an important role in the development of the SSIP approach to understanding conflict and cooperation. Rasler and Thompson review several systemic theories of interstate conflict including balance of power theory, power transition theory, long cycle theory, and world systems theory respectively. Tir and Vasquez examine how contiguity and territorial disputes have increased the risks for interstate militarized disputes and wars. They also describe territorial conflicts inside the state, such as partitions and secessionist movements, and how these domestic conflicts relate to interstate disputes. Leeds and Morgan analyze the literature on arms races and alliances as potential forces for conflict or peace through deterrence, starting from the traditional balance of power and power transition approaches. Chan provides an overview of the democratic peace literature from Kant's work on "Perpetual Peace" to modern analyses of the Kantian tripod for peace and potential threats to the democratic peace (e.g., the dangers of democratization).

Moore and Tarar discuss how domestic factors influence interstate conflict processes including the relationship between civil conflict and interstate conflict, the diversionary theory of war, and the way in which domestic institutional characteristics influence interstate bargaining. Salehyan and Thyne review the work on civil war onset, duration, and outcomes, concentrating on such factors as greed, grievance, geography, and international influences. Prorok and Huth focus on the expansion and diffusion of war, the conduct of war such as respect for the laws of warfare or civilian targeting, and factors that influence the duration and outcomes of interstate and civil wars. Hartzell and Yuen appraise recent research on the durability of peace following interstate and civil wars describing how the characteristics of conflicts and belligerents and the nature of third party conflict management efforts influence the prospects for peace.

The final section of the book takes stock of what we have learned with an SSIP approach and identifies avenues for future research. Maoz points to many successes of the SSIP community including the development of sophisticated data sets and numerous formal models to help understand conflict processes, the increasing sophistication of statistical models for analyzing SSIP data sets, and successful attempts to integrate the logic of paradigmatic approaches in the study of international relations, such as realism and liberalism. He also describes several shortcomings of SSIP research such as a disconnect between theory and empirical tests, a heavy emphasis on the dyadic level of analysis, and a failure to explain fully why a small number of countries fight most interstate conflicts in history ("fightaholism").

The chapters in this edited volume provide readers with a very thorough introduction to the SSIP approach and the numerous contributions it has made to the broader understanding of conflict processes. Readers will gain valuable insight into the data sets, methodological advances, formal models, and theoretical arguments advanced by the SSIP research community. The increasing prominence of this research in major social science journals and book publishers attests to the success of the SSIP approach. This volume will help conflict scholars expose this material to a younger generation of SSIP researchers.

Section I

The SSIP Approach

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1

The Origins and Evolution of SSIP

How Methods Met Models, with a Short Interlude

Kelly M. Kadera and Dina Zinnes

Introduction

The Scientific Study of International Politics (SSIP) became a section of ISA in 1993, largely out of necessity. Not so long ago, believe it or not, quantitative research in international relations had trouble seeing the light of day. The historically dominated field could not understand the statistical, mathematical, and data-based research, and traditionalists often found the results reported in such studies to be trivial and uninteresting, making both publication and convention panel participation difficult. Initially, SSIP researchers believed that subject-oriented panels were best: mixing traditional and quantitative research on a single panel with a common subject focus, such as deterrence, would encourage cross-fertilization and provide a greater understanding of the problem under study. This proved to be a false assumption and a misunderstanding of the role that public venues play in the development of research. Conventions, in particular, provide a researcher with the opportunity to get feedback on preliminary results from the intellectual community. If panelists, and consequently the audiences they attract, do not understand the methodology behind a piece of research or are hostile to the approach, panels only become wasted opportunities or fora for exchanging useless barbs. Faced with the challenges of obtaining spots on panels, let alone some that provided a productive exchange, Dina Zinnes and Hayward Alker gathered signatures and drafted the charter that led to the establishment of the SSIP section. Because ISA allocates convention panels to each section based on the size of the section and its panel attendance from the previous convention, formal status as a section guaranteed the SSIP community exposure time in potentially productive public environments where panelists and audiences spoke the same language. How did this group of scholars develop a research agenda that was substantial enough to warrant the formation of a new ISA section? Our chapter traces the progress of those who set down the foundations of SSIP.

In sketching the origins and evolution of the SSIP community, it is tempting to propose a linear chronology from one set of studies to another. To do so, however, would distort the realities of what happened. While some approaches did indeed lead to others, the story line contains several subplots that begin independently and only much later merge with the rest of the field in intriguing ways. But we are getting ahead of ourselves. Let us begin by telling the more straightforward, time-ordered tale, explain why it had to be so, and then turn to the parallel but largely distinct narratives. In the end, we show how the story lines have merged and where their shared themes are headed.

The Linear Chronology

The linear part of the story can be traced to the two world wars. International politics as a field of inquiry did not exist prior to World War I. And to the extent questions about international phenomena were entertained at all, they occurred largely in the context of law: the rules that *should* govern the interactions of states with one another. What, for example, was a *just war*, that is, under what conditions was it morally, and therefore legally, acceptable, for one state to attack another? But the horrors of World War I turned attention away from *should*, the ethics of state interaction, to questions about *why* things happen as they do. Hence, realism became the new approach as students of international politics sought to understand the dynamics of state interaction that led to such events as war.

The even greater catastrophe of World War II reinforced this new emphasis on understanding why and how things happen in the international arena. For if you did not understand the hows and whys of two world wars, what would prevent a third from happening? This brought scientists from other disciplines to the study of international conflict and war. Psychologists, sociologists, economists, and even physicists and mathematicians sought to use their skills to study and hopefully prevent future major conflagrations. The flurry of research by these scholars reinforced the realist perspective on understanding *what is* and added to it a demand for observation and the use of rules for measurement. These are the events that set the stage for what has become SSIP, known in its early years by names such as Quantitative International Politics or Interpolometrics to emphasize the observational/measurement component.

As researchers began to tackle the problem of international conflict, it became clear that existing arguments about the whys and wherefores of interactions between states were vague and underdeveloped. These needed to be spelled out explicitly so they could be subjected to empirical appraisals. What affected what? The first steps towards clarification produced “frameworks” and “propositional inventories.”

Frameworks identified and classified the key variables thought to be responsible for inter-nation interactions, providing suggestions as to how they might be linked. One classic in this genre was Kaplan’s *System and Process* (1957). Kaplan argued that it was possible to define different types of international systems based on the characteristics of nation participants and the rules of the game, so to speak, which they used in interacting with one another. Snyder et al. (1962) proposed a very different type of framework. Their goal was to provide a generic outline of the domestic and international variables that structure foreign policy outputs. Rosenau’s “pretheory” (1966) was yet another noteworthy framework. While it was also an attempt to understand the foreign policy decision process, the goal was to demonstrate the differences in foreign policy outputs of nations as a function of key domestic variables. Propositional inventories, on the other hand, were far more specific and focused on particular variables and hypotheses. These inventories were extensive lists of hypotheses about what affected what, largely obtained from a careful reading of the descriptive and historical texts on international politics. Snyder and Robinson (1961) offered one of the most extensive of these inventories, covering all facets of decision making in international politics.

As the arguments about how the international world worked were clarified, it became increasingly obvious that there was a second, more serious, problem. If hypotheses generated by frameworks or inventories were to be subjected to empirical scrutiny one had to have access to *data*. But what constituted data in this field and how would one obtain relevant observations? The methods of observation used in other disciplines – experimentation, participant-observation, interviews – were either not relevant or not feasible. Two solutions emerged: (1) simulation; and (2) what might be called archeology. Let us discuss them in that order, breaking down the archeology investigations into subtypes.

Data Generation, Part I: The Simulation

Simulation was an adaptation of the data generation process used in psychology and was spearheaded by Harold Guetzkow, a social psychologist (see Guetzkow 1962 and Guetzkow et al. 1963). Guetzkow argued that international processes could be studied in a mock laboratory in which people played the roles of national decision makers and teams of 3–4 players were stand-ins for nations. The Inter-nation Simulation (INS) project consisted of 5–7 teams with members of each team playing the roles of head of state, domestic affairs advisor and foreign policy advisor. At the onset of a simulation period each team was given a profile describing its resources, decision structure (e.g., parliamentary), and form of government (i.e., its relationship to its citizens). Teams were also given a scenario of the history of the international system up to the point at which the run was to begin; for example, who was allied with whom, whether there had been wars, whether there were existing international organizations, trading partners, and so on. As the simulation proceeded, teams were permitted to (1) interact through messages or prearranged person-to-person visits (state visits or summit meetings), (2) form alliances, (3) create international organizations, (4) declare war, (5) provide aid, and (6) trade resources.

At the conclusion of a run of the simulation, each team filled out a form indicating how it was allocating its original resources: a certain amount for trade, aid, for research and development, for domestic products, and the like. These forms were collected and, while teams took a coffee break, calculations proceeded to determine what happened to each nation as a consequence of its resource allocation decisions. For example, in countries that had begun with minimal resources and a history of population unrest, inadequate allotments to domestic development could lead to a revolution. Alternatively, large allocations to research and development in one round could mean that the nation would have a major breakthrough in weapons technology in the subsequent run and consequently the capacity to develop nuclear weapons. If a war had been declared then the calculations would determine winners and losers.

It is important to note that the calculations of the consequences of the actions and allocations of the players were governed by explicit mathematical formulae, not simply by the judgments of the experimenters. Moreover, these formulae, or rules, were constructed based on the INS team's hypotheses about how the international system worked. In particular, they were based on some of the same hypotheses and arguments that underlay the frameworks and propositional inventories. Hence, the INS researchers made extensive attempts to model the structure of their simulations on what was known about the real-world operation of international politics. This was done in an effort to answer the critics that argued that INS had a serious validity issue: how could teams of high school or college students represent the decisions of national statesmen?

The validity question stalked INS researchers for many years and the attempt to solve or simply answer the charge led these researchers in several directions. One was to attempt to replicate a real world event such as World War I. Structuring the simulation scenario to parallel the principal participants and their relationships prior to 1914, they ran the simulation to see if war would indeed occur. Although the desired result was obtained, questions were raised as to the extent to which the participants (who were not told that the run was an attempt to approximate a real world event) produced war because of their familiarity with the historical context.

Another, very different, approach was to evaluate the underlying propositions that governed the plays and outcomes of the simulations. Thus data on nations were collected and hypotheses about the relationship between national attributes on the one hand and national behavior on the other were tested. Intriguingly, these efforts gave birth to a very different project that soon took on a life of its own: Rudolph Rummel's Dimensionality of Nations (DON). Rummel was a student at Northwestern

when the efforts to validate INS were underway and he was originally in charge of the data collection and hypothesis testing. But Rummel's interest in INS was quickly overtaken by his fascination with data collection and hypothesis testing itself. Rummel's interest in understanding how the attributes of nations translate into behavior led him, much later, to propose how the relationship between national attributes across nations might determine their interactions: thus we have one of the nonlinear spin-offs: field theory. But again, we are ahead of the story. We will return to Rummel's spin-off work in our discussion of archeology.

A third, less direct, INS response to the validity critique was to point out that some questions of great concern to the field could only be studied in the laboratory. While the laboratory was not perfect, perhaps, they argued, it could at least provide insights. One of the more important attempts in this direction was Richard Brody's (1963) study of the Nth country problem: how a widespread change in military technology, the advent of nuclear weapons, could affect the structure of an international system and the interactions among the nations. Brody constructed a bipolar INS system: two major powers (highly resource endowed) with a number of smaller nations in alliance with each major power. The simulation was run for a number of periods under these conditions and the perceptions and actions of the nations recorded. It was found that the smaller nations interacted almost entirely with the superpower in their alliance, having little to do with other smaller nations in their alliance or any of the nations in the opposing alliance. Additionally, all nations within a given alliance saw members of the opposing alliance as threatening. Then nuclear weapons were introduced. Several of the smaller nations discovered at the beginning of a new round that they had struck it rich, so to speak: due to previous investments in research and development, these nations now had nuclear capabilities. The interaction patterns changed dramatically. The bipolar alliance structure crumbled and the perceptions of threat were now ubiquitous. Every nation became fearful of all the others. Whether these runs provided an insight into the breakdown of social interactions between groups or said something of consequence about international politics is, of course, open to question. The results, however, are nevertheless intriguing. And because there is only one system to observe at any point in time, empirical testing is difficult, meaning such simulations may remain scholars' best tool for understanding relatively rare, but significant, phenomena at the aggregate level.

Data Generation, Part II: Archeology of Actors

The other answer to the data question was to turn to the traces of things that had actually happened – just like conducting an archeological dig. Using the historical record, this approach sought to collect, in a systematic and explicit fashion, information about the characteristics of nations, their behavior and their interactions. Those who chose this route believed that these data were far more real than the information gathered from simulation runs. With time, however, it became evident that the validity problem did not disappear. It now simply showed itself in another form: how do you define a *war*, an *alliance*, or even an *interaction* between nations?

Thus began the many data generating projects of the 1960s and 1970s. While all of these projects rely on some written record – whether it be the historians' accounts, yearly statistical compilations by various international agencies, or newspaper reports – they were anything but unified. These enterprises varied in the principal questions that drove them, the sources used to extract the data, the definitions of the variables of interest and the methodologies applied. To take but one example of how these data collections differed even in the definition of a single variable, consider the three different data sets on war. For Quincy Wright (1942), a political scientist, a war existed

and was recorded in his collection only if there had been a legal declaration of war by one state against another. When J. David Singer, another political scientist, initiated the Correlates of War (COW) project, a war was defined in terms of the number of individuals killed on the battlefield; an event was counted as a war only when 1,000 people had been killed (Singer 1979). On the other hand, Lewis Fry Richardson, a meteorologist and Quaker, sought to understand why any disagreement ended up in the death of even a single individual. Thus his data collection focused on “deadly quarrels” and contained murders at one end of the continuum through gangland executions to the world wars at the other (Richardson et al. 1960b).

Despite these important differences, the many data collection efforts can be classified as being (1) attribute oriented, (2) behavior/interaction oriented or (3) a combination of the two. The attribute collectors were principally concerned with recording, over time, the characteristics of nations – population size, GDP, square miles of territory, number and composition of minority groups, regime type, and so on. These projects became the various World Handbooks (Russett and Banks 1968; Russett et al. 1968; Taylor and Jodice 1968; Taylor and Hudson 1975a, 1975b; Taylor et al. 1975) and Ted Gurr’s Polity enterprise (Eckstein and Gurr 1975).

The behavior/interaction projects on the other hand were concerned with tracking the events that transpired between nations. To a large extent these event data efforts shared a common focus on international crises as a potential prelude to war. The oldest of these is WEIS, the World Event Interaction Survey, initially begun by Charles McClelland (1971). McClelland argued that international crises could be predicted by classifying events into hostile, neutral, and cooperative types and monitoring the co-occurrence of event combinations. Crises, he posited, were the culmination of sets of particular kinds of interchanges between nations. Using principally the *New York Times* (and later adding the *London Times*), every action taken by any nation towards any other nation was coded by indicating the day on which it occurred, the nation perpetrating the event, the target of the event, and the event type. Thus WEIS was a massive daily chronology of every action directed by one nation towards another. Using indices from information theory such as Hrel (Miller 1964), McClelland (1972) demonstrated how combinations of hostile/cooperative acts could predict the onset of international crises.

Approximately a decade later, and unaware of the ongoing WEIS effort, Edward Azar began work on COPDAB, the Conflict and Peace Data Bank (Azar 1980). Born in the Middle East with strong family ties to the region, Azar looked at the interactions between nations from a different perspective. Azar’s concern, like McClelland’s, was with international crises, but unlike McClelland, Azar focused on recurring crises between the same participants, the ongoing, seemingly endless, Arab–Israeli conflict that would cyclically heat up, cool down, and heat up again. Azar called these crises “protracted conflict” and his data collection efforts were an attempt to chart the course of long, drawn-out interactions. Like McClelland, Azar developed a classification scheme for these events in terms of hostility and cooperation. However, unlike McClelland, Azar saw this classification as a scale that ran from high to low levels of hostility and from low to high levels of cooperation. Sensitive to scaling issues, Azar utilized methodologies from psychology to assign weights to the conflict–cooperation categories so that the intensity of hostility or cooperation could be meaningfully assessed for a designated time period, permitting the researcher to observe the ebb and flow of conflict or cooperation.

In addition to the differences in their classification schemes, WEIS and COPDAB differed in the sources used to extract the events. Azar argued that Middle Eastern events were inadequately covered in the Western press and that reliance on a single source, like the *New York Times*, would provide a distorted picture of what was happening in that region. Consequently he turned to the use of multiple regional news outlets.

While both WEIS and COPDAB had to struggle with reliability and validity issues in the definition of the various types of events and the training of coders, the use of multiple sources created an additional problem for COPDAB: knowing when an event recorded in one source was the same or different from an event recorded in another source. Unless one could make this distinction, COPDAB would run the risk of over-recording events and thus falsely magnifying the amount of conflict or cooperation occurring on a given day.

Although COPDAB began with a focus on the Middle East, it soon expanded to world coverage, rivaling WEIS. This led to comparisons and evaluations of the relative merits of the two (Howell 1983; Vincent 1983) and, to some extent, arguments over the usability of one versus the other. However, because the classification schemes differed in their definitions of types of interactions and the fact that COPDAB came with scaled weights, the superiority of one over the other was never clear. This, together with the fact that the two projects had very different funding sources, kept both alive. DARPA, the Defense Advanced Research Projects Agency of the Defense Department, had been deeply involved in supporting WEIS and was reluctant to switch gears after putting so much behind the efforts to create a crisis indicator. COPDAB, on the other hand, began with small university backing and then, on and off, received limited support from the National Science Foundation (NSF).

Three other event data collections emerged: (1) Frank Sherman's (1994) *Sherfacs*, which focused on the phases of conflict escalation; (2) Wilkenfeld, et al.'s ICB (International Crisis Behavior) project (see Brecher et al. 1988; Wilkenfeld et al. 1988), which centered on foreign policy behavior and crises as the units of analysis; and (3) Pearson's (1974) foreign interventions data set, which allowed analysis of the outcomes associated with various types of interventions.

What came to be called the 1914 Study represents a very different type of event data collection process. Like WEIS and COPDAB the focus was on international crisis. But unlike these efforts, the 1914 Study was interested in only one particular crisis: World War I. Looking at this single cataclysmic event, researchers attempted to capture the play-by-play sequence of events that led to World War I. This detailed account of an international disaster focused on how the principal decision makers in the European capitals reacted to one another – their perceptions and actions – to eventually produce the disaster known as World War I. Using both the classic histories (Fay 1928; Albertini 1957) of this conflict and original documents that were found in the archives of the Hoover Institution, Stanford researchers, under the direction of Robert North, coded the activities and perceptions of the decision makers as the events unfolded from the assassination of the Austrian Archduke to the declarations of war (see, e.g., Zinnes et al. 1961; Zinnes 1962).

The event data projects surveyed thus far can all be characterized by their principal focus on *actions*. There were, however, two projects which were event based but additionally had an important national attribute component: CREON and DON. CREON, the Comparative Research on the Events of Nations (Hermann et al. 1973), grew out of the Rosenau framework mentioned earlier. Like Wilkenfeld and Brecher's project on comparative foreign policy, CREON was an attempt to understand the formulation and execution of foreign policies. But while Wilkenfeld and Brecher were interested in the foreign policy formulation process, the Hermanns, who spearheaded CREON, wanted to evaluate the Rosenau paradigm that linked types of nations to types of foreign policy decisions. Thus the CREON researchers needed to collect data on both the attributes of nations to permit them to properly place a nation in the Rosenau typology and the actions that these nation-types pursued, that is, their foreign policies. They hoped to show that the foreign policy of a small, underdeveloped nation was very different from the foreign policy of a large developed country. This cross-national perspective had a dramatic effect on CREON's event data collection

procedures. Because WEIS and COPDAB were interested in how crises evolve, their data sets were collected through time. CREON's concern with types of nations and types of foreign policies, however, made time irrelevant. Thus CREON events were extracted from news sources by sampling quarters within the years covered.

The Dimensionality of Nations project, as mentioned earlier, had its origins in the simulation approach to SSIP. As Rummel attempted to provide empirical underpinnings for INS by collecting data on national attributes and events, he became intrigued with an emerging area of statistics known as factor analysis (see Rummel 1963). Factor analysis was being developed in psychology to help researchers identify potential links among large numbers of variables. Given many variables and data sets of considerable size, factor analysis could determine underlying correlations between groups of variables. Thus Rummel applied factor analysis to his data set of national variables and found that variables measuring domestic problems were not related to (did not "load" on the same factors as) variables measuring hostility directed externally at other nations (Rummel 1968). This result was noteworthy because it appeared to debunk one of the old literature arguments: nations experiencing domestic turmoil engage in hostile external behavior to redirect the attention of their population from domestic difficulties.

When Rummel left Northwestern upon completion of his PhD, the DON project retained its factor analytical approach and continued its data collection efforts on both the characteristics of nations and their behaviors. However, the principal argument that drove the project changed. The linkage between domestic problems and international behavior was replaced by a focus on how the relationship between the attributes of nations affected their interactions (see, e.g., Rummel 1969a, 1969b, 1986). Field theory, which interestingly had its origins in both the work of Quincy Wright (1942) of decades earlier and Johan Galtung (1964), the Norwegian sociologist, was an attempt to use the mathematical model inherent in factor analysis to demonstrate how differences or similarities in attributes between two nations was a motivating force that was responsible for how those nations interacted.

Data Generation, Part III: Archeology of the International System

A significant characteristic of an event data set is its actor-level perspective, or its focus on who did what to whom when. In contrast, the Correlates of War, begun by J. David Singer at the University of Michigan, represented a systemic perspective. Singer's initial focus was on what he called "brush-clearing": an empirical examination of major arguments in the international politics literature (1980). Singer took particular aim at propositions linking the distribution of power, alliances, and the onset of war. To examine these hypotheses empirically, the COW project collected data on measures of national power, alliance configurations, and the outbreaks of wars. A number of intriguing indices were formulated to produce systemic measures based on these data. Thus the power distribution of the international system sought to capture the extent to which power was concentrated in a few nations or spread more evenly over a number of nations, alliances were coded in terms of the extent to which the alliance reflected strong or weak commitments on the part of the nations, and wars were measured using indices based on the number of participant nations, number killed, and the duration of the war.

One of the more important consequences of COW's initial efforts was the creation of the war data set. As more time and resources were poured into the effort to refine the collection and coding of the war data, the brush-clearing emphasis was replaced by a concentrated effort to understand the war phenomenon itself and *why war* became the central theme. This new focus led to four further ventures. First, because the war

data set developed in the COW project compiled data only for wars between major powers since 1815, Jack Levy subsequently extended the data to include minor powers and extended the data set back to the 1600s. Second, wars involving nonstate entities, such as colonial and civil wars, were gathered (Singer and Small 1972). A third important offshoot was the MID (Militarized Interstate Dispute) data set, which sought to provide an empirical basis for answering the question: why do some international disputes end in war while others do not (Jones et al. 1996)? While every war is at one point an MID, not every MID becomes a war. Thus the goal was to ascertain what characteristics distinguished between these two scenarios. The fourth extension was the BCOW project (Leng and Singer 1988). It sought to understand the buildup of events that led to the outbreaks of wars. Taking a sample of wars from the initial war data set, the events preceding each war were coded and classified. Reminiscent of the 1914 Study (though with less detail), or the WEIS project (though backwards in time), the goal was to determine whether pre-crisis patterns might be uncovered.

An Interlude

Before continuing we need to pause for a few comments. The reader may have wondered why the story thus far has been told principally in terms of data sets rather than the results of the studies that motivated the data sets. The answer is simple. Although every data set came into existence as a consequence of a particular set of questions, very few of the analyses that sought to answer these questions yielded findings that have stood the test of time. A principal reason for this was the lack of statistical training and understanding of research design during the early years of the collection efforts, the late 1960s and 1970s. As late as 1972, *International Studies Quarterly* (*ISQ*) devoted space to an article explaining how to use computer programs to generate such simple descriptive techniques as pie charts, time series plots, and frequency histograms (Dow et al. 1972). And even by 1980, training in research methodologies and statistics was not part of the normal political science graduate program, meaning graduate students had to go to departments like psychology, sociology, economics, and mathematics to learn how to apply statistical models. Unfortunately, the statistics and methodologies learned in these contexts typically did not address the types of problems found in international politics and, if applied, could be inappropriate because of the assumptions that underlay the statistical models. Psychological statistics, for example, assumed that observations were random samples from almost infinite populations in which the individual units were all comparable. In what sense could the wars of the nineteenth century be considered a random sample, much less even a sample?

But while the findings of these early studies have not stood the test of time, the data collection efforts have persevered and data sets like WEIS, COPDAB, COW, and so on continue to be refined, extended, and mined, making them a crucial landmark in SSIP. The reasons for this are not hard to find. Despite the many critiques that have been thrown at these efforts regarding the adequacy of the sources used, the validity of the definitions used to code variables, or the reliability of the coding practices (e.g., Burgess and Lawton 1972; Howell 1983; Vincent 1983), one fact has always been paramount: data to evaluate hypotheses concerning international phenomenon are not easy to come by. The international politics researcher is not like other social scientists who can use laboratories, participant observation, and interview schedules to obtain data to test their ideas. Obtaining data for the study of hypotheses about international politics is extraordinarily expensive and time consuming. So the mere existence of these data collections has become an invaluable resource for many researchers to gain empirical assessments of arguments about how nations conduct their business. In short, these collections have turned out to be useful to researchers other than their original developers.

The existence of data sets, however, has been a two-edged sword. By making research somewhat easier they have also shaped those efforts. Together with the enhanced computing power that came available as massive mainframes were replaced by desktop and laptop computers and ever more sophisticated software, the ease of running even fancy statistical analyses became irresistible. Thus if the 1960s and 1970s were devoted to collecting data, the subsequent decades of the 1980s and 1990s became periods of extensive hypothesis testing: searching for critical independent, dependent, and intervening variables with increasing attention to statistical designs that permitted causal conclusions. The search for recurrent patterns in the large data collections invariably produced the hypothesis testing mind-set: the *if X then Y* perspective with its heavy emphasis on statistics. Only recently has SSIP research begun to consider more carefully the stories behind the hypotheses that were being tested, to attempt to construct the theoretical underpinnings. To understand this development we need to look at what we called earlier the nonlinear historical pieces.

Nonlinear Pieces

The above thread provides a reasonable time line of the principal origins of SSIP. However the complete story must take note of several independent research avenues that were not part of this straightforward chronological development. One of these was the work of Quincy Wright. His two classic volumes on war and international politics were written before the quantitative approach took root, yet reflected the themes later found in the SSIP movement. *On War* brought together information from a wide variety of fields that Wright believed might be of value for understanding the war phenomenon. Thus he surveyed psychology, sociology, economics, and the like to see what was known in these fields that might help one understand international conflict. The appendices of this volume are of special interest. One contains a data set of all legally declared wars, going back to the fifteenth century and noting dates and participants. In another, even more surprising, appendix Wright proposes a very simple mathematical model of conflict.

Of even greater consequence for the SSIP movement was the work of Lewis Fry Richardson. Like Quincy Wright, Richardson completed his work long before today's major data collection efforts were even considered. Working as an ambulance driver in World War I, Richardson saw the horror of war up close and personal. These experiences, together with his Quaker convictions, left him with a profound commitment to apply his mathematical and scientific skills to the study of human violence. Richardson was a meteorologist with considerable mathematical training. In the hours not devoted to the study of weather patterns he patiently began his inquiry into the hows and whys of people killing people.

This inquiry had two central paths. On the one hand, Richardson, like the data collectors of several decades later, believed he needed information, or data, to fuel his ideas. His focus, however, was not just on wars. As a Quaker, Richardson was concerned with the broader issue of why people kill one another. He saw wars as being at one end of the continuum of inter-human violence that begins when one person kills another through gang wars, civil wars, and on to small and then large wars. As we saw earlier, Richardson defined these events as deadly quarrels. He then classified them by size, which was given by the number of people killed in the incident: two when the murderer is executed by the state, on up to millions when we reach the world wars (Richardson 1960b). However, because Richardson believed that every death by a human hand was important, he rescaled the magnitude of these events using a log transformation. Thus, the event in which a murderer is executed, a deadly quarrel of size 2, is less dwarfed by a world war in which millions lost their lives.

The data set that emerged from these efforts is amazing in many respects. Richardson's goal, somewhat like Singer's decades later, was to brush clear, to evaluate the ideas that the literature had put forth for why people kill one another. Consequently he needed to understand as much of the context of these events as could be extracted from records. Thus dates, participants, or number killed were insufficient; Richardson also needed information on the reasons for the conflict and the outcomes. Was it differences in religions, economic disparities, old grievances, territorial contiguity, or something else? The data set that emerged consists of massive tables that record numerous characteristics of each collected event of inter-human violence. To appreciate this extraordinary data one must remember that it was compiled by one dedicated researcher searching through historical archives in his spare time and recording events on scraps of paper. There were no graduate assistants, and of even greater significance, no DARPA or NSF funding and no computing support.

Richardson (1960b) sought to evaluate some of the simple hypotheses using known statistical techniques (e.g., correlations). However, his work is more impressive for the creative ways in which he developed miniature mathematical models to explore questions. Consider for example his exploration of an empirical fact that emerged from his data collection, now recorded in the monograph, *Statistics of Deadly Quarrels* (1960b). Analyzing only those events in which a large number of individuals are killed, instances that he conceptualized as wars, Richardson observed that most wars only involved two participants. Moreover, a histogram in which the number of participants is plotted against the frequency of wars having a given number of participants produces a decelerating curve. Using the mathematics of permutations and combinations and considering the participation in a war as a toss of a coin, he develops a mathematical model that produces the curve found in the data. The analysis is less important for its result than for the style of thinking that it exemplifies. The assumptions underlying the mathematical model (embedded in the use of permutations and combinations) represent a theory of national war participation. The resulting curve is thus a deduction from the theory. The fact that the curve fits the data indicates that this rather simple model is at least one possible theory of war participation. In this way, *Statistics of Deadly Quarrels* is a wonderful blend of the inductive and deductive approaches.

Richardson's second path of inquiry is also a powerful blend of the deductive and inductive. While *Statistics* has a stronger emphasis on the inductive, *Arms and Insecurity* (1960a), the second posthumously published volume, emphasizes the deductive. In *Arms* the reader is again treated to a wonderful excursion into Richardson's thinking processes. The author dialogues with himself as he puts together a story characterizing the decision makers of two nations who wish only for peace but are driven by fear into a potentially devastating arms race. Using historical quotes from statesmen in the European capitals prior to World War I, Richardson develops a differential equations model to capture how each nation's fear of its rival pushes its decision makers to develop an armament program, further fueled by long-term historical grievances, and tempered only by the drag that the production of armaments puts on the country's domestic economy. As Richardson describes the conversation between the two nations' leaders, he demonstrates how verbal statements can be translated into mathematics, making this one of the finest examples of what Lave and March, years later, call the development of theory through story telling (1975). And so we have the now famous Richardson arms race model.

While every student of SSIP should read these first chapters of *Arms and Insecurity* simply for the illustration of how to develop a mathematical model, the value of these pages goes far beyond the simple construction of the model. Two important things happen in the pages that follow the construction. First, Richardson shows how the analysis of this simple two-variable, linear, differential equations model can actually provide answers to the burning questions about *why war?* Using standard phase

portrait methods, Richardson demonstrates how it is the relative significance of the key parameters that actually sets the conditions for the outbreak of war. When two nations fear each other to the extent that they do not care about the impact that the armament programs have on their respective domestic economies, then the arms race will spiral off into an infinite arms level for both sides. Infinity is what Richardson equates with war. Only when the economic drain of an armament program outweighs fear will the two nations consider armament reductions and thereby avoid war.

Richardson's consideration of the model does not end with his mathematical analyses (as, sadly, too many modeling efforts often do). His third contribution to SSIP is his consideration of how one might empirically evaluate the model. Using armament data prior to World War I, he demonstrates that the solution to the differential equations indeed looks very much like the time line of armament buildup.

Again, the model is simple and of less import for what it says than it is for its demonstration of how one can theorize about international conflict. Richardson showed how to move from a verbal story (what we often call a theory) to a mathematical model, to a set of analyses, to deductions and then to an empirical analysis. The presentation, analysis, and evaluation of the arms race model, made almost a half century earlier, could have been the poster child for the NSF's Empirical Implications of Theoretical Models (EITM) program, launched in 2001, that sought to bridge the gap between mathematical modeling and the empirical testing of hypotheses derived from models.

Why, one might wonder, were Richardson's efforts long ignored; why did no one pick up where he left off? The historical answer is straightforward: Richardson's work was largely unknown. He published a few of his studies in the magazine *Nature*, but this was a scientific journal not read in the social sciences. When he died in 1953, most of Richardson's work had not been published and only existed in the form of scattered notes. Had his son not worked steadfastly to convince Quincy Wright to champion the publication process, none of us would have known of Richardson's historic contributions. It took a number of years for Wright and his collaborator C. C. Lienau to comb through the notes and put them together into two meaningful volumes (Richardson 1960a, 1960b). Because the various pieces of research were done at different times, there were frequent changes in notation and missing pieces in the mathematical analyses. Thus reconstruction of the arguments was difficult and progress toward publication slow.

As the volumes were reaching publication stage, several historical coincidences occurred. (1) J. David Singer joined the Political Science Department at the University of Michigan; (2) Anatol Rapoport, a mathematical biologist also at the University of Michigan, learned of the unfolding manuscript; and (3) through the joint efforts of Herb Kehlman (a social psychologist), Robert Angel (a sociologist), and then Singer – all at Michigan – the *Journal of Conflict Resolution (JCR)* was born. The birth of this journal was another cross-disciplinary effort to spearhead a dialogue among social scientists about war specifically and human conflict more generally. Once again, international events – this time the Cuban missile crisis – reminded everyone that World War II might not be the end of worldwide conflict. Following the publication of several issues of *JCR*, it was suggested that Rapoport write a synopsis/overview of Richardson's work that would introduce Richardson to the social science community and pave the way for the newly published books. Rapoport was ideally suited to this task because of his mathematical background and the ease with which he could present difficult mathematical concepts to nonmathematical audiences, as was clearly the case among most social scientists. Thus did Lewis Fry Richardson meet the social science, and more particularly the political science, community (Rapoport 1957).

Rapoport's presentation of the arms race model and explanation of the mathematical analyses was masterful. This issue of *JCR* is certainly a classic. But while it did provide

the needed introductions, the absence of mathematical training, as had been true in the statistical realm, made it difficult for fledgling SSIP researchers to grab hold and run with the ideas. Instead, given the data and statistical orientation that the field had begun to assume, the Richardson equations were initially seen as regression equations to be estimated. The power of the mathematical model and the extent to which its analyses provided testable deductions were obscured by the rush to statistically evaluate the equations in ever new data sets, add new variables, and apply the latest econometric techniques. It was decades before the more exciting and significant aspect of Richardson's arms race model would be understood and his theoretical ideas extended – decades during which SSIP researchers slowly began to distinguish between statistics and mathematical modeling and then gain sufficient technical sophistication to make it possible to move Richardson's ideas to another level.

During this period two other trends emerged that would eventually help support the move towards a more theoretically based SSIP. One of these can also be traced back to Rapoport. In yet a second important contribution to SSIP foundations, Anatol Rapoport introduced conflict resolution researchers to ideas in game theory. In *Fights, Games and Debates* (1960), he sought to distinguish between types of conflicts based on their inherent goals. The goal of a fight was to mash the opponent, the goal of a game was to outdo the opponent, while the goal of a debate was to convince and win over the opponent. The SSIP community became intrigued by game theory as a way to characterize and understand international conflict. Unlike the more complicated analyses involved in solving the differential equations of the arms race model, game theory was both intuitively appealing and accessible with little mathematical training. It was easy to think of international confrontations, like the Cuban missile crisis, in terms of a Kennedy and Khrushchev trying to outdo one another in a game of Chicken. Moreover, concepts like dominant strategies or saddle points, which could provide solutions for these models, were relatively simple to grasp. So the application of game theory to international conflicts became an important part of the SSIP landscape.

Initially, however, the application of game theory to international politics was descriptive (or even normative) rather than theoretical. It provided a neat way to characterize types of situations; for example, a zero-sum conflict was one in which the winner took all. Brams's (1980) retelling of biblical stories used game theory as a means of drawing out the critical ingredients of certain types of conflicts to demonstrate why events unfolded as they did. The stumbling block in using game theory as a theoretical tool lies in the construction of the game's payoff matrices. For game theory to be a mathematical model like the arms race model and yield deductions (i.e., predictions) that can be empirically evaluated, the numerical values representing the values that players have for outcomes must be determined independently of the solution. If you know the outcome of the Cuban missile crisis, then it is too easy to configure a game matrix so that it reflects the outcome that occurred. Thus the application of game theory to this scenario might simply illustrate, for example, that it is a game of Chicken. One of the major moves toward using game theory as a theoretical model came in Bueno de Mesquita's *The War Trap* (1981), which used expected utility theory, the foundation of game theory, as an explanatory and predictive tool. Brams's *Theory of Moves* (1994) represents a modification of game theory that also recasts game theory in a theoretical, that is, potentially falsifiable, mold.

The second move towards making SSIP research theoretical harkens back to the days of the Inter-Nation Simulation. As Guetzkow constructed and ran simulations, collected data, and tested hypotheses, he was careful to call his laboratory a man-computer operation. People played the roles of the decision makers, but the outcomes of their decisions were determined by mathematical rules that required computers for calculation. A young graduate student at Michigan State was intrigued by the computer component of INS and approached Guetzkow about the possibility of making INS an