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One Question, Two Answers

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The Research Question

Many of the major policy challenges facing governments today are in some sense collective problems calling for joint solutions. However, even when effective solutions can be developed and implemented only through joint efforts, voluntary cooperation can be hard to establish and maintain, making it all the more important to understand the conditions for success and the causes of failure. This study addresses the question of why some efforts at developing and implementing joint solutions to international problems succeed while others fail.

Leaving aside sheer luck, at the most general level this question seems to have two possible answers. The first lies in the character of the *problem* itself: some problems are intellectually less complicated or politically more benign than others and hence are easier to solve. The second possibility focuses on *problem-solving capacity*: some efforts are more successful than others because more powerful (institutional) tools are used or because greater skill or energy is used to attack the problem.

This study attempts to explore the contents and merits of these two general propositions. We started out with an equal interest in both, but along the way we became increasingly preoccupied with the latter, for two main reasons. First, it points us toward factors that can—at least in principle—be deliberately manipulated by decision makers and hence used as *tools* for problem-solving. If the study of regime formation and implementation is ever to contribute to the development of praxis itself, it will have to do so primarily by providing insights into the functioning of accessible tools or instruments. Second, we think it is fair to say that—thanks largely to developments in the application of game-theory constructs to the analysis of international cooperation—more is known about what makes a problem politically

malign than about what determines our capacity to solve it. In particular, we became puzzled (and also encouraged) by the fact that some of our malign problems were indeed solved or alleviated rather effectively, and we wanted to understand better what made such successes possible.

After having formulated the research problem, we must define our key concepts. What precisely do we mean by *regime effectiveness*, and how do we go about measuring it? What distinguishes a benign problem from one that is malign? What are the critical components of problem-solving capacity? Once we have constructed this conceptual platform, we can move on to try to translate the two basic propositions formulated above into specific form. The general statement that some problems are harder to solve than others becomes interesting only if we can specify *which* kinds of problems are malign and what makes them so. Likewise, the proposition that the problem-solving capacity of some institutions or systems is greater than that of others becomes interesting only to the extent that we can specify *which* kinds of institutions and systems have the greatest capacity and what determines this capacity. The remainder of this introductory chapter is devoted to these questions. In a final section we introduce our empirical testing ground—fourteen cases, all but one focusing on problems of environmental protection or resource management.

The Dependent Variable: The Concept of Regime Effectiveness

In a common-sense understanding, a regime can be considered effective to the extent that it successfully performs a certain (set of) function(s) or solves the problem(s) that motivated its establishment.[1] Although useful as a point of departure, it soon becomes obvious that this definition of *effectiveness* is not sufficiently precise to be useful as an analytical tool for systematic empirical research. Thus before venturing into comparative research, some conceptual groundwork is needed to clarify *what* precisely our dependent variable is. This is all the more important since analysts studying the impact of international institutions have construed their dependent variables somewhat differently.[2] Even though an encouraging trend toward convergence can be seen over the last decade, it can still be difficult to distinguish substantive differences from those that are merely terminological.

From a methodological perspective, evaluating the effectiveness of a cooperative arrangement means *comparing* something—let us provisionally refer to this object simply as *the regime*—against some standard of success or accomplishment. Any attempt at designing a conceptual framework for the study of regime effectiveness

must, then, cope with at least three (sets of) questions: (1) what precisely constitutes *the object* to be evaluated? (2) against which *standard* is this object to be evaluated? and (3) *how* do we go about comparing the object to this standard—in other words, what kind of measurement operations do we have to perform to attribute a certain score of effectiveness to a certain regime? Let us briefly consider these three questions.

What Constitutes the Object to Be Evaluated?

This may at first glance appear a trivial question; the object clearly must be the cooperative arrangement (regime) in focus. A second look reveals that identifying the object to be evaluated may not be this simple.

First, we must determine whether we are interested only in the impact of the cooperative arrangement itself or also in the costs incurred and positive side effects generated in the efforts to establish and maintain it. The former is the appropriate basis for evaluating the *regime* itself, while the latter provides a basis for evaluating (also) problem-solving *efforts* or processes. The distinction is not merely one of academic hair splitting. Establishing and operating a regime usually entail various kinds of costs—some of which are significant—and a rational actor presumably makes his choices on the basis of some estimate of *net* rather than gross benefits.[3] Problem-solving efforts can also have significant positive side effects. Thus, international negotiation processes are often large-scale exercises in *learning*, through which at least some parties modify their perceptions of the problem and of alternative policy options and perhaps see their incentives change as well. As a consequence, the process itself may lead governments as well as nongovernmental actors to make *unilateral* adjustments in behavior—even in the absence of any legal obligation to do so (see Underdal 1994). The aggregate impact of such side effects may well be more important than the impact of any formal convention or declaration signed in the end. Since the costs of tracing the various process-generated effects systematically and in depth can be very high, we nonetheless in this project have to confine ourselves mainly to studying the effects of the regimes themselves. But the overall implication of what we have said above should not be missed: problem-solving *efforts* usually generate their own consequences, over and beyond those that can be attributed to the cooperative arrangement they may establish. And some of these process-generated costs and benefits may, indeed, be far from trivial (see, e.g., Miles 1989; Underdal 1994).

Second, as Easton (1965, 351–352) and others remind us, a distinction should be made between the formal *output* of a decision-making or regime-formation process (that is, the norms, principles, and rules constituting the regime itself) and the set

of consequences flowing from the implementation of and adaptation to that regime. In the context of environmental policy, the latter may be further specified by drawing a distinction between consequences in the form of changes in human behavior (here referred to as *outcome*) and consequences that materialize as changes in the state of the biophysical environment itself (*impact*). Environmental regimes are, at least officially, established to protect some environmental values. The ultimate interest thus most often pertains to biophysical impact. In all cases, however, this goal is to be accomplished through changes in human behavior (outcome). In the regime-formation stage and its immediate aftermath, the norms and rules of the regime itself are, however, all we know. Actual change in behavior and environmental impact can be determined only at a later stage (usually after several years of operation). Moreover, there is no straightforward method for inferring outcome or impact from information about output. Targets of regulation sometimes respond by making ingenious adjustments that are hard to predict or by more or less flagrant noncompliance. Such adjustments may render even the most stringent rules ineffective in practice. Similarly, since the role of a specific set of human activities in causing environmental change is sometimes not well understood, the change prescribed in human behavior (such as cutbacks in emissions) will not always in fact lead to the change predicted in the state of the environment. Even perfect compliance with a strong regime is therefore not a *sufficient* condition for achieving policy goals defined in terms of biophysical impact.

In general, then, the effectiveness of a particular regime (E_r) can be seen as a function of the stringency and inclusiveness of its provisions (S_r), the level of compliance on the part of its members (C_r), and the side effects it generates (B_r)—that is,

$$E_r = \mathrm{f}(S_r C_r) + B_r.$$

Analytically, we treat output, outcome, and impact as three distinctive steps in a causal *chain* of events, where one serves as a starting point for analyzing the subsequent stage(s) (see figure 1.1).

Furthermore, we distinguish between the stage of regime *formation* (the end product of which is a new set of rules and regulations—that is, output) and that of regime *implementation* (the first product of which is behavioral change—outcome), leading—if the diagnosis is correct—to some change in the state of the biophysical environment (impact) further down the road. At the output stage, a regime can be assessed on the basis of criteria such as the stringency of its rules and regulations, the extent to which the system of activities targeted is in fact brought under its jurisdiction or domain (its inclusiveness), and the level of collaboration established.

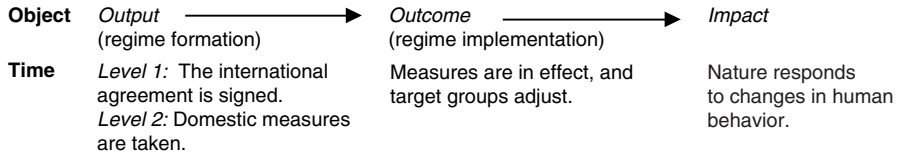

Figure 1.1
Objects of assessment

In this study, we are particularly interested in the relationship between level of collaboration and effectiveness measured in terms of behavioral change. The basic question may be formulated as follows: does more cooperation lead to better substantive results? For the purpose of this part of the analysis, we measure level of collaboration in terms of a six-point ordinal scale:

0 Joint deliberation but no joint action.

1 Coordination of action on the basis of tacit understanding.

2 Coordination of action on the basis of explicitly formulated rules or standards but with implementation fully in the hands of national governments. No centralized appraisal of effectiveness of measures is undertaken.

3 Same as level 2 but including centralized appraisal.

4 Coordinated planning combined with national implementation only. Includes centralized appraisal of effectiveness.

5 Coordination through fully integrated planning *and* implementation, with centralized appraisal of effectiveness.

In line with the logic of the flowchart in figure 1.1, we treat level of collaboration as an *intervening* variable. More specifically, we assume that level of collaboration is affected by problem malignancy and the problem-solving capacity of the system that established the regime[4] but also—in turn—that it makes a positive, albeit modest, contribution to effectiveness.

Against Which Standard Is the Regime to Be Evaluated?

Defining an evaluation standard involves at least two main steps. One is to determine the *point of reference* against which actual achievement is to be compared. The other is to decide on a standard *metric of measurement*.

It seems that basically two points of reference merit serious consideration in this context (see figure 1.2). One is the hypothetical state of affairs that would have come about had the regime not existed. This perspective leads us to conceive of

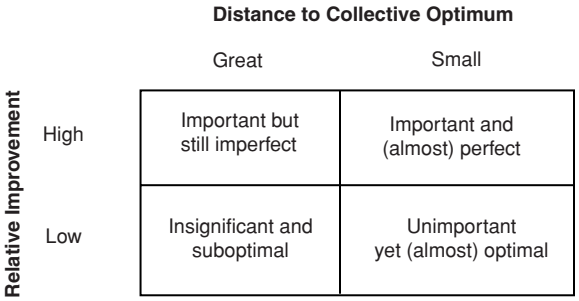

Figure 1.2
Two dimensions of effectiveness

effectiveness in terms of the *relative improvement* caused by the regime.[5] This is clearly the notion we have in mind when considering whether and to what extent a regime matters. The other option is to evaluate a regime against some concept of a good or ideal solution. This is the appropriate perspective if we want to determine to what extent a certain collective problem is in fact solved under present arrangements. The words *good* and *ideal* are chosen to indicate that there are at least two different suboptions that can be adopted—known as *satisficing* and *maximizing*, respectively. Since actors tend to have different standards of satisfaction, we will here refer to the *collective optimum*.[6] We elaborate on this notion below, but at this point we define a collectively optimal solution as one that accomplishes, for the group of members, all that can be accomplished—given the state of knowledge at the time.[7]

These two approaches are clearly complementary. Even a regime leading to a substantial improvement may fall short of being perfect. Conversely, in more fortunate situations a minor adjustment may be sufficient to reach the joint optimum. Moreover, both dimensions are interesting in their own right. International regimes are, it seems, typically evaluated in terms of how well they (can be expected to) perform compared to the state of affairs that would have come about in their absence (the noncooperative solution) *as well as* in terms of their ability to solve the problems they are designed to cope with.[8] This suggests that students of international regimes should be able to play with *both* these notions of effectiveness and perhaps combine them into an integrated measure as suggested by Helm and Sprinz (1999).[9] It is, however, important not to confuse the two.

Each of these approaches calls for further conceptual refinement. Consider, first, the notion of *relative improvement*. Although intuitively meaningful, the provisional

definition given above leaves open at least one critical question: what precisely is the *baseline* from which change or improvement should be measured?

In principle, it seems that we most often have a choice between two main options: one is the hypothetical state of nature that would have obtained if, instead of the present regime, we were left in a no-regime condition (a *fully* noncooperative solution).[10] The alternative option is to take as our baseline the hypothetical situation that would have existed had the previous order or rules of the game been left unchanged. The former measures effectiveness in absolute terms, while the latter measures change from one order to another (effectiveness differentials). The former may ultimately be the more interesting, but the notion itself is elusive and thus virtually impossible to measure. For practical reasons, then, we are left with the previous order as our baseline—the underlying assumption being that it would have continued by default in the absence of the present regime.

Conceiving of effectiveness in terms of the distance between what is *actually* accomplished and what *could have been* accomplished immediately puts before us the intriguing question of what constitutes the *maximum* that a particular group of actors can accomplish. A natural scientist would probably answer by referring to environmental sustainability, assimilative capacity, or some other notion of ecosystem health. An economist would frame the answer in terms of social welfare, probably measured as net economic benefit to the group of regime members. Whenever we are talking about solutions that have to be shaped through political processes, however, such technically perfect solutions will not always be politically feasible. What is politically feasible depends on the institutional setting, particularly the decision rule. Whenever we are dealing with collective decisions that can be made only through consensus, the appropriate notion of the political optimum is the *Pareto frontier*. This frontier is reached when no further increase in benefits to one party can be obtained without leaving one or more prospective partners worse off. In the most favorable circumstances, a solution maximizing the sum of net benefits to the group will also be Pareto optimal, but in the context of international negotiations there is no guarantee that the two will coincide (see Underdal 1992b). In this study we therefore started with an ambition to assess regimes in terms of a political as well a technical notion of the ideal solution. We were soon forced to conclude, however, that we had no reliable method for determining distance from the Pareto frontier with sufficient confidence. As a consequence (and with some reluctance), we had to drop the notion of political optimum from the comparative analysis.

To define a standard of evaluation we need not only to decide on a point of reference against which actual achievement is to be compared; we also need to agree

on some standardized *metric of evaluation*. In most of our cases we seem to face a choice between measuring effectiveness in terms of social welfare (usually translated into some measure of economic costs and benefits) or in terms of ecological sustainability or some other biophysical criterion.[11] For example, the performance of the International Whaling Convention (IWC) may be evaluated in terms of net economic benefits, in terms of sustainable biological yield, or in terms of some preservationist notion of protection. As we demonstrate in chapter 15, the score that we give to the IWC depends critically on which of these values we choose as the basis for our evaluation. The most important lessons to be drawn here seem to be that we should be explicit about the choices we make and should realize that scores obtained by using different evaluation metrics cannot be used interchangeably—at least not without a critical examination of compatibility.

How Do We, in Operational Terms, Attribute a Certain Score to a Regime?

The discussion of the practicalities of operational measurement is left to chapter 2. Here we say just a few words to clarify our ambitions. In this study no attempt is made to go beyond *ordinal*-level measurement. The purpose of the project—to help improve our understanding of why some international problem-solving efforts are more successful than others—does not require a higher level of measurement. Nor do we know how to construct a cardinal scale that would make sense in this context. In fact, in comparing different regimes or components we use scales including only four (in the case of distance to the collective optimum) or five (in the case of relative improvement) levels of effectiveness. Even such a crude coding is by no means a straightforward exercise. The major challenges that we face in scoring cases in terms of effectiveness are (1) to determine empirically our point of reference (whether it is the collective optimum or the hypothetical state of affairs that would have occurred in the absence of the regime) and (2) to distinguish the causal impact of the regime itself from that of other factors affecting human behavior or the environment. We take some comfort in the fact that these challenges are not unique to this particular study. In fact, they are encountered—and somehow apparently solved—by all students and practitioners who dare to assign a certain score of effectiveness to a political institution.

Methodological Challenges to Measuring Regime Effectiveness

It should by now be abundantly clear that scoring cases in terms of effectiveness confronts us with intriguing methodological challenges. This is particularly so when we compare scores *across* different regimes; diachronic comparison *within* one

regime or regime component is, fortunately, somewhat less complicated. The reader should understand that however careful and systematic we try to be in examining available evidence, all assessments of effectiveness will inevitably involve some element of subjective judgment or inference on the part of the analyst. We do believe that many of our conclusions are quite robust, but we realize that others are not. We owe it to our readers to try to distinguish the former from the latter, and we attempt to do so in the presentation of the individual cases.

To summarize, we have taken as our point of departure a common-sense notion of effectiveness, saying simply that a regime is effective to the extent that it successfully performs some generic function or solves the problem that motivated its establishment. For most *environmental* regimes the ultimate test will be to what extent they improve the state of the environment itself. Environmental objectives are to be achieved through changes in the human behavior that causes environmental damage (such as pollution and nonsustainable harvesting). Accordingly, human behavior is the immediate target. The relationship between rules and regulations (output), change in human behavior (outcome), and biophysical change in the environment (impact) is a question to be settled through empirical research. In assessing the effectiveness of a regime we use two basic points of reference—the hypothetical situation that would have existed in its absence (the noncooperative situation) and a notion of what would be the ideal solution (the collective optimum). These distinctions are summarized in figure 1.3.

As we have seen, some other studies have used different notions of effectiveness. Combined with the fact that the scores we end up with will sometimes depend critically on which standard we apply, this observation raises two important questions.

First, does it make sense to try to develop some *composite* or *aggregate* score to cover all or at least the main options considered above? Our answer can briefly be summarized as follows: (1) If we are talking about using different operational *indicators* for the same theoretical concept, computing some aggregate score basically means constructing an index. This may certainly be a sensible thing to do, provided that the indicators included are believed to capture different aspects of the phenomenon we are trying to get at. (2) Aggregating scores across *different* basic concepts is a straightforward operation only as long as one case strongly dominates another.[12] For example, referring to figure 1.2, we can see no problem in rating cases in the upper right-hand category as "more effective" than those in the lower left-hand cell; in terms of our two criteria the former strongly dominates the latter. (3) To produce a meaningful aggregate score where one case does *not* strongly dominate another, we would have to combine different notions into one integrated

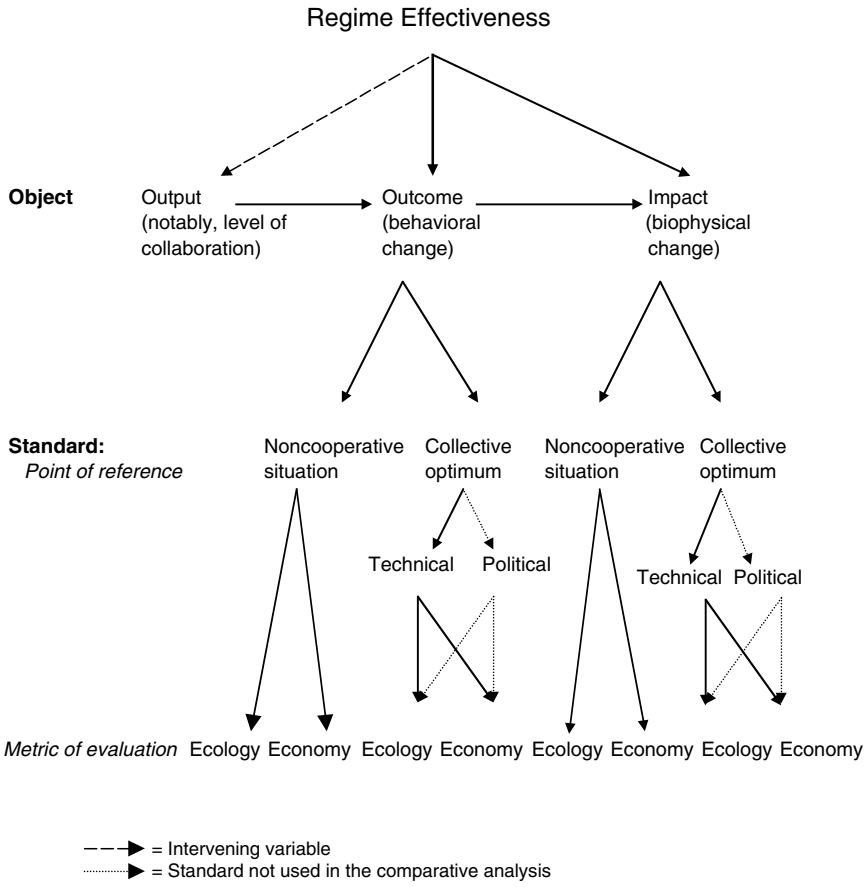

Figure 1.3
Regime effectiveness: Objects and standards of assessment

formula. As we have seen above (note 9), a sensible integrated measure can be created by computing an effectiveness coefficient in which *actual* improvement is expressed as a fraction of *potential* improvement. As far as we can see, this particular kind of combination is the only way to produce aggregate scores in cases where one regime does not strongly dominate another.

As we now move on to define and examine our independent variables, a second question arises: Can the same model, including the same set of independent (and intermediate) variables, be used to account for variations in performance *irrespective* of how we define *effectiveness*? Can, for example, the same model that we

would use to explain variance in what Young (1994) labels "effectiveness as problem solving" equally well account for variance in what he calls "process effectiveness" or "constitutive effectiveness"?[13] This question is much too complex to be answered adequately here. The general rule of thumb must be that the greater the substantive difference between the definitions in question and the better specified the independent variables, the less likely that the answer will be an unqualified yes. Note, for example, that Young's definition of *process effectiveness* and some notions of *regime strength* (e.g., Haggard and Simmons 1987) focus only on the level of implementation and compliance. The relationship between effectiveness as defined here and compliance is by no means straightforward; for malign problems, the situation may very well be that the level of compliance tends to be *inversely* related to the amount of behavioral change required by regime rules (see, e.g., Downs, Rocke, and Barsoom 1996). In another study, Young (1998) divides the process of regime formation into three distinct stages—agenda formation, negotiation, and implementation—and argues that the political dynamics unfolding at each stage are sufficiently different that a hypothesis formulated with reference to one of these stages will not necessarily hold for the other two.

Moreover, any attempt at measuring effectiveness will have to refer to the state of affairs *at one particular point in time*. For several reasons—one being that producing effects usually takes time—scores may vary depending on when the assessment is made. Everything else being equal, we would expect the effectiveness of a regime to increase when it has had the time to mature and penetrate the system of activities in question. This is not to suggest that we should expect a linear increase in effectiveness over time; rather, we would expect the typical pattern to be *curvilinear*—increasing as the regime matures but diminishing as it ages into obsolescence. The general point is simply that scores may vary over the lifetime of a regime. One very important implication of these observations is that comparing the outcomes or impacts of two or more regimes is a straightforward exercise only if these regimes are measured at similar stages in their life cycles. Whenever we are unable to synchronize observations in this particular sense, caution is required in comparing scores *across* regimes.

The Independent Variables: What *Determines* Regime Effectiveness?

As indicated above, we see regime effectiveness as a function of two main (sets of) independent variables—namely, the character of the problem and what we have called problem-solving capacity. Before defining more precisely what we mean by

these notions and formulating our hypotheses in more specific terms, however, a few words are needed to clarify the relationship between our two (sets of) independent variables. More specifically, at least two questions deserve to be addressed: one is that of relative importance, and the other is that of interplay.

In a survey of empirical cases, we may very well find that one of our independent variables—and in this case, problem type would be our candidate—accounts for most of the variance in effectiveness that we can actually observe. Such a finding would warrant the conclusion that one determinant is basically more important than another if and only if both independent variables are measured by the same yardstick. In this study we will definitely not be in a position to claim that one unit of *benignity* equals one unit of *capacity* (except, perhaps, in the derived statistical terms of standard deviation). When the requirement of standardized measurement cannot be met, the finding that one independent variable has a greater impact than another will be open to diverging interpretations. The amount of variance accounted for by any given independent variable can be seen as a function of its relative basic weight and its range of variation. In the absence of a standardized unit of measurement we have no firm basis for distinguishing the impact of the former from that of the latter.

Even when their effects cannot be separated empirically, basic weight and range of variation should be distinguished *conceptually*. By itself, a constant accounts for *none* of the variance observed. Yet it may be an important determinant of the phenomenon we want to understand. As an illustration, most intergovernmental decisions are made by consensus. In a particular survey of international decision-making processes, we may therefore be justified in considering the decision rule of consensus as basically a constant. If the decision rule remains the same throughout a given sample of cases, it can obviously not by itself help us explain why outcomes *differ*. But this hardly justifies the jump to the conclusion that the decision rule is therefore irrelevant to understanding negotiation behavior or outcomes. Only by keeping the distinction between basic weight and range of variation clearly in mind can we avoid such misinterpretations.

Finally, problem structure and problem-solving capacity cannot be seen as mutually independent factors. Capacity is always the capacity to do *something*. Beyond a certain level of generality, what constitutes problem-solving capacity can therefore be determined only with reference to a certain category of problems or tasks. Thus, it is by now conventional wisdom that the problem-solving skills and the institutional tools required to solve benign problems are somewhat different from those required to solve problems that are malign in character. For example, the

relative importance of technical versus political skills tends to differ; the more malign the problem, the higher tends to be the premium on, inter alia, integration and mediation skills. Similarly, malign problems tend to require higher levels and perhaps also more complex arrangements of cooperation—including more attention paid to procedures for monitoring and enforcing compliance. In what game theorists refer to as *pure coordination games*, common norms or ideas may serve as focal points or clues enabling the parties to achieve effective tacit coordination without formal rules and regulations.

The basic implication for our analysis is that *notions of capacity will have to be matched with notions of problem type and task*. This is potentially a substantial complication, indicating that we should ideally be able to play with a complex and differentiated *set* of capacity constructs. We still have far to go before we can claim to have such a set of analytic tools. To make our task manageable, we confine ourselves in this study to one rather crude three-dimensional concept. The fact that our main interest is to better understand what contributes to the successful handling of *malign* problems serves to narrow the scope of the analysis and thereby also to make our simplifications somewhat less distorting than they otherwise might have been.

Problems: Benign and Malign

A policy problem may be difficult to solve in at least two respects. At the *intellectual* level some problems are substantively more intricate or complicated than others, implying that more intellectual capital and energy are needed to arrive at an accurate description and diagnosis and to develop good solutions. It is, for example, by no means obvious what level and rate of resource exploitation can be expected to maximize the long-term global social welfare that can be harvested from Antarctica. Nor is it easy to determine what constitutes a sustainable pattern of energy consumption and what mix and programming of policy instruments can achieve such a pattern at minimum social cost. To answer these and many other technical questions we need powerful theory, large amounts of accurate data, and the creative imagination and perseverance of skilled people. But collective-action problems are also *political* issues, and as such they can vary in their degree of malignancy. The political malignancy of a problem will here be conceived of primarily as a function of the configuration of actor interests and preferences that it generates. According to this conceptualization, a perfectly benign problem would be one characterized by identical preferences. The further we get from that state of harmony, the more malign the problem becomes.

In this study we are concerned primarily with the *political* aspects of policy problems. The intellectual dimension will be considered only as it interacts with political characteristics. This is by no means a trivial aspect; intellectual complexity and political malignancy do in fact often interact—most often with the consequence of making a problem more intractable, but sometimes with the benign consequence of facilitating agreement.[14] For example, descriptive uncertainty about the state of a recipient or stock—and particularly theoretical uncertainty about the causal impact of particular human activities on the environment—may fuel political controversy. In turn, political conflict may contaminate processes of knowledge production and dissemination (see, e.g., Miles 1989) and thereby serve to obstruct the development of *consensual knowledge*. As students of politics, we thus examine the interplay between knowledge and politics from the perspective of policy making rather than knowledge making. Moreover, we conceive of intellectual complexity in terms of the amount of descriptive and theoretical uncertainty pertaining to the knowledge base rather than in terms of some objective measure of the inherent intricacy of a problem. We do recognize that the two will not necessarily correlate perfectly, but as our goal is to understand actor behavior and outcomes of collective decision-making processes, the former is the appropriate focus in this study.[15]

Before we move on to specify what distinguishes politically malign problems from those that are benign, a few words seem in order to position our approach in relation to the extant literature that examines the impact of problem characteristics on outcomes of international cooperation. Grossly simplified, it seems meaningful to distinguish between two main paths of research in this area. One distinguishes *problem structures* according to properties of the issue or issue area in focus. The most well-known case in point is a typology developed and applied by a research team based at the University of Tübingen (see, e.g., Rittberger and Zürn 1990; Efinger and Zürn 1990; Hasenclever, Mayer, and Rittberger 1997).[16] Their typology distinguishes four main *objects of contention* and is used to develop a set of hypotheses about the prospects for regime formation (see figure 1.4).

The other main approach categorizes problems according to the structure of the strategic games they generate. Students pursuing this path typically distinguish between broad categories of games (such as coordination games and collaboration games) (Stein 1982) or analyze situations in terms of a set of specific game models (see, e.g., Zürn 1992). As their bases for classification are different, there is no straightforward method for translating from one of these typologies to the other. Yet the hypotheses derived by the two schools seem at least largely compatible and based on similar lines of reasoning.

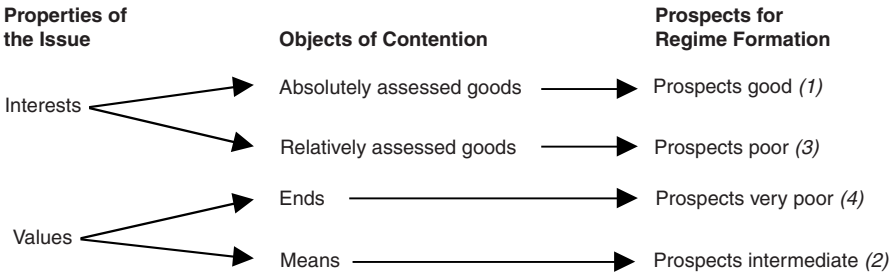

Figure 1.4
Objects of contention: Tübingen team distinctions

Like the Tübingen group, we focus not on specific game structures but on generic *mechanisms* that generate the kinds of problems that international regimes are established to address. Nevertheless, in terms of its basic premises, the scheme that we use here is closer to the *situation-structure* approach. We take as our point of departure the assumption that the official purpose of international regimes is to coordinate behavior in situations where the absence or failure of coordination will or can lead to *suboptimal* outcomes. At least two broad categories of situations fit this description. In one, the cost-benefit calculus of individual actors includes a nonproportional or biased sample (representation) of the actual *universe* of costs and benefits produced by his decisions and actions, so that "the pursuit of self-interest by each leads to a poor outcome for all" (Axelrod 1984, 7). We refer to these situations as problems of *incongruity*. In the other category of situations, (1) the overall result depends on the matching of actions taken by individual parties, (2) more than one route can lead to the collective optimum, and (3) the choice between or among these routes is *nontrivial*, meaning that compatibility cannot be taken for granted—even in the absence of conflict of interests or values. We refer to the latter category as problems of *coordination*. In political terms, the former can be considered more or less malign, while the latter are basically benign. Let us explain why.

Problems of Incongruity To repeat, the defining characteristic of this category of problems is that the cost-benefit calculus of an individual actor is systematically biased in favor of either the costs or the benefits of a particular course of action. Such a bias may be due to the objective distribution of material consequences, the perspective applied in assigning value to these consequences, or both. Transfrontier pollution and common-pool resources are prominent examples of situations where

the former mechanism is at work. To the extent that pollution generated in one country causes damage in other countries, the polluting country typically harvests all the benefits of the activities causing pollution and suffers only a certain fraction of the damage. Similarly, to the extent that the fishing fleet of one country depletes a common-pool resource, it would share the loss in terms of future fishing opportunities with others while reaping all the short-term benefits of higher catches itself. Now, such objective physical impacts do not speak for themselves; they serve as decision premises only as interpreted and evaluated by the actors involved. One important aspect of this evaluation process concerns the criteria used in assigning value to perceived consequences. For a moderately altruistic actor—being equally concerned with his own welfare and the welfare of others—the physical distribution of impact would not matter; his own cost-benefit calculus would positively incorporate also what happens to others. By contrast, a competitively motivated actor would be concerned about relative gains and losses and hence assign negative value to the welfare of others. Finally, an individualistically motivated actor would be concerned only with his own payoff and assign zero value to the welfare of others. The bottom line is that what comes out as problems of incongruity depends on both the objective distribution of material consequences and the perspectives and criteria used by the actors in assigning subjective value or utility to perceived consequences. In the absence of evidence pointing in another direction, we assume that individualistic motivations dominate.

Incongruity can be caused by at least two different mechanisms—externalities and competition.[17] The term *externalities* here denotes external leaks—those effects of an actor's behavior that hit others and (therefore) disappear from the actor's own cost-benefit calculations. Assume that the government of country A is considering the level of pollution abatement to demand from its industries. Half the total damage costs are suffered within A's own borders, and half are suffered by neighboring countries. Assume, furthermore, that all goods produced by A's industries are consumed domestically and are subject to no competition from abroad. If A's government seeks to maximize national economic welfare, and no issue linkages are constructed, it will balance 50 percent of the benefits derived from abatement against 100 percent of abatement costs.

Let us now change one of these assumptions so that A's industries face strong competition in foreign as well as domestic markets. In this case, the real costs of pollution abatement would include not only the direct costs of installing and operating new filters or production equipment, but also—at least if the "polluter-

pays" principle is observed—indirect costs in terms of loss of market shares. The latter can be attributed to competition rather than leaks and will quite often be the major cause of concern.

In general, a relationship of *competition* exists whenever one actor's (subjective) welfare depends on how well he performs compared to others. Competition is found in many spheres of social life—from markets to sports and arms races—and, to use the terminology of the Tübingen team, whenever we are dealing with some "relatively assessed good." The difference between externalities and competition as defined here can be described as one between effects that just leak and those that boomerang. In the case of externalities, the worst that can happen to an actor contributing unilaterally to the provision of some collective good is that the benefits he thereby produces are harvested by others (acting as free riders). In a relationship of competition the worst that can happen is virtual extinction from the game in question. Similarly, if effects just leak, an actor causing a net reduction in social welfare will be able to shift some of the costs onto others. In the context of competition, noncooperative behavior may conceivably be rewarded up to the point where the defector's share of overall group benefits from the activities in question approaches 100 percent. This implies that competition can distort actor incentives by *amplifying* the costs of cooperative behavior or by positively *rewarding* defection in ways in which pure leaks can never do. Thus, other things being equal, competition is inherently the more malign problem of the two.[18]

Problems of incongruity can be particularly hard to solve through voluntary cooperation to the extent that they are also characterized by asymmetry or cumulative cleavages. A problem is *asymmetrical* to the extent that the parties involved are (or perceive themselves to be) coupled in such a way that their values are incompatible or their interests negatively correlated. The typical upstream-downstream relationship is a good example of negatively correlated interests. Unless some kind of cost-sharing scheme is established, measures to control unidirectional transfrontier pollution will benefit victims at the expense of polluters. Equally asymmetrical can be situations where one party values a particular species for consumptive uses and another for its beauty or its place in nature. Other things being equal, the more asymmetrical an incongruity problem, the more difficult it will be to find a solution that will be accepted by both or all parties. This is particularly so if the distribution of costs and benefits is determinate—that is, easy to predict for the parties themselves. When we are dealing with multidimensional problems, the presence of cumulative cleavages can be an additional source of complications. Cleavages

are *cumulative* to the extent parties find themselves in the same situation on all dimensions or issues, so that those who stand to win (or lose) on one dimension also come out as winners (or losers) on the other dimensions as well. Compromises and package deals are easier to find for problems characterized by crosscutting cleavages.

Problems of Coordination Contrary to the position taken by Keohane that "Cooperation takes place only in situations in which actors perceive that their policies are actually or potentially in conflict" (Keohane 1984, 54; cf. also Oye 1985, 6, and Stein 1982, 302), we argue that even in situations characterized by perfect harmony of interests coordination of behavior may be required to ensure collectively optimal outcomes (cf. Schelling 1960). In general, coordination can be useful also in situations where (1) the overall result depends on the compatibility of individual choices, (2) more than one route can lead to the collective optimum, and (3) the choice between or among these routes is not a trivial or obvious one, meaning that compatibility cannot be taken for granted even when actor interests are identical. We refer to such situations as problems of coordination. These problems are politically benign in that actor interests are fully compatible. Cooperative solutions will be stable in the sense that once such a solution is established, no actor will have any incentive to defect unilaterally.

Many international regulations, ranging from the allocation of radio frequencies to the designation of shipping lanes, are designed essentially to help actors tackle coordination problems. In some cases, such as regulation of maritime or air traffic, the major purposes of coordination are to avoid accidents or reduce transaction costs. In other cases, particularly in what Young (1996) refers to as "programmatic" activities, there may even be positive synergy to be tapped into. For example, the overall utility of emissions or environmental quality monitoring may be enhanced substantially by such international coordination as standardization of methods and a system for pooling of observations. This is also the case for quality of weather forecasting.

Problem Type: A Summary Summing up, we conceive of problem malignancy as a function of incongruity, asymmetry, and cumulative cleavages. We consider incongruity as the principal criterion for classification; the other two are properties that tend to augment the political intractability of incongruity problems (see table 1.1). The major differences between problems of incongruity and problems of coordination are summarized in table 1.2.

Table 1.1
Characteristics of malign and benign problems

Malign	Benign
Incongruity (in particular relationships of competition)	Coordination (synergy or contingency relationships)
Asymmetry	Symmetry or indeterminate distribution[a]
Cumulative cleavages	Cross-cutting cleavages[a]

a. As indicated above, these dimensions are relevant primarily for problems of incongruity.

Table 1.2
Characteristics of incongruity and coordination problems

Dimension	Incongruity	Coordination
Essence of problem	Incentive distortion ($q_I \neq k_i$)	Imperfect information, communication failure
Essence of cure	Incentive correction	Information or communication improvement
Consequences of unilateral cooperative moves	Risky, particularly in relationships of competition	No risk (except for transaction costs of own efforts)
Tactical implications for negotiations	Manipulation or coercion likely	Integrative negotiations, persuasion
Postagreement implications	Incentives to unilaterally defect tend to persist; transparency, monitoring, and enforcement mechanisms important	Self-enforcing; no incentives for unilateral defection from an agreed solution

The simple typology developed above can easily be refined—for example, by using game-theory concepts to introduce finer distinctions as well as to derive a more differentiated and precise set of testable hypotheses. We do not pursue that path here for two reasons. First and most important, to get full mileage out of fine-grained distinctions on an *in*dependent variable, we would need equally fine-grained distinctions on the *de*pendent variable. Since we use only a crude, four-level ordinal scale to measure effectiveness, the marginal utility of further specification of game structures declines rapidly. The general implication is that a strategy of specification

that would be essential in formal deductive analysis may be rather futile for purposes of comparative empirical research—particularly studies with a small number of cases. Second, in encounters with complex problems, fairly simple and crude distinctions often seem to provide the most robust and useful tools, at least as a first cut of comparative analysis. Moreover, we think a two-step procedure can help us avoid the danger of pounding square pegs into round holes. Game-theory models may be most useful when they are applied to fairly specific decision points rather than to broader problems. There is, for example, no compelling reason to assume that the essence of (for example) transboundary air pollution—let alone externality problems *in general*—can be adequately modeled as a (symmetrical) prisoner's dilemma game, even though many *specific* situations may well have that structure.

The line of reasoning that we have pursued in this section leads to the following main hypotheses:[19]

H_1: The more politically malign the problem, the less likely the parties will achieve an *effective* cooperative solution—particularly in terms of technical optimality.

The reasoning behind the latter part of H_1 is that—up to a certain level—the more malign the problem, the more suboptimal the noncooperative state of affairs is likely to be.[20] The worse the non-cooperative situation, the less capacity and effort it takes to bring about *some* improvement, but the more it will take to achieve an ideal solution.

H_2: Political malignancy and uncertainty in the knowledge base tend to interact to increase the intractability of problems.[21] Uncertainty in the knowledge base tends to slow down the development of effective responses to benign problems as well, but here the effect on the end result will be less detrimental.

So far, we have treated each problem on its own merits only. In real life, issues are often linked either through inherent functional (inter)dependence or through deliberate tactical moves. Moreover, an actor may support or oppose a particular solution for reasons that have little or nothing to do with the official purpose of the regime. Issue linkages or ulterior motives may thus contaminate a benign problem or render a malign problem easy to handle. Taking this broader context into account, we can now try to specify the conditions under which malign problems can be successfully solved:

H_3: Regimes dealing with truly malign problems will achieve a high degree of effectiveness only if they contain one or more of the following: (1) selective

incentives for cooperative behavior (see Olson 1965), (2) linkages to more benign (and preferably also more important) issues, or (3) a system with high problem-solving capacity. The presence of at least one of these factors is a *necessary*, but not a sufficient, condition for a high level of effectiveness.

Problem-Solving Capacity The alternative proposition formulated on the first page of this chapter offered the concept of *problem-solving capacity* as the key to explaining regime effectiveness. The general argument is that some problems are solved more effectively than others because they are dealt with by more powerful institutions or systems or because they are attacked with greater skill or energy. The time has come to look inside the elusive and complex notion of problem-solving capacity and to try to identify at least some of its main elements.

Let us first of all repeat that in this study our interest pertains primarily to the capacity to deal with the *political* rather than the intellectual aspects of a problem and to the interplay between those two dimensions. When solutions are to be shaped through collective decisions, problem-solving capacity (in its political interpretation) can provisionally be conceived of as a function of three main determinants:[22]

- The institutional setting (the rules of the game),
- The distribution of power among the actors involved, and
- The skill and energy available for the political engineering of cooperative solutions.

We try to deal with all three determinants, but our analysis is confined essentially to organizational structures and political capabilities and not to what might be called cognitive or behavioral aspects. An attempt at measuring the impact of behavioral skill or efficacy would have required a kind of in-depth analysis of individual actors and political processes that we had neither the time nor the resources to undertake on a broad scale.[23]

Second, it bears repeating that problem-solving capacity can be determined precisely only with reference to a particular category of problems or tasks. We have indicated some of the implications of that observation above. It also means that the significance of at least two of the determinants listed above clearly differs from one category of problems to another. Most obviously, while coercive power can be an important tool in dealing with malign problems, it is likely to be largely irrelevant—and, if exercised, often counterproductive—in benign situations. The premium on skills in political engineering is higher for malign than for benign problems. And the institutional arrangements conducive to integrative negotiations are to some

extent different from those that facilitate distributive bargaining (Walton and McKersie 1965). We do not aim at exploring all these nuances here. Since our main interest pertains to the conditions under which malign problems are successfully dealt with, the remainder of this section is framed primarily with such problems in mind.

The Institutional Setting As a basic social science concept, *institution* refers to constellations of rights and rules that define social practices, assign roles to participants in those activities, and guide interactions among those who occupy those roles (see Young 1994, 3). In this study, we use the term *institutional setting* broadly as a label for two different notions of institutions—namely, institutions as arenas and organizations as actors. The distinction refers to functions and does not imply a ranking in terms of importance. Institutions can shape outputs and outcomes as much by coupling actors and problems and determining the rules of the game, as by entering the game as more or less independent actors. Arenas are important in their own right and for different reasons.

Institutions as Arenas When we talk about an institution as a "framework within which politics takes place" (March and Olsen 1989, 16), we conceive of it as an arena. Arenas regulate the access of actors to problems and the access of problems to decision games. Moreover, they specify the official purpose as well as the rules, location, and timing of policy games. Institutions as arenas can be described by answering the following question: *who* deals with *what*, *how*, *when*, and *where*?

Arenas differ in terms of rules of access, decision rules, and rules of procedure, as well as in terms of informal culture. For example, the membership of a regime is in some cases restricted to countries that satisfy certain criteria (the Antarctic Treaty System would be a case in point). Others (such as the International Whaling Commission) are open to any state that cares to submit a formal application and pay its membership fee.[24] Consensus is the decision rule most frequently used in international organizations, but a number of organizations have some provisions for decision making by voting (usually requiring a qualified majority on substantive matters). As we discuss below, the decision rule is an important determinant of the capacity of an institution to aggregate diverging preferences. Other things being equal, aggregation capacity reaches its maximum in strictly hierarchical structures and is at its lowest in systems requiring unanimity. Furthermore, we know that rules

of procedure may differ in several respects—for example, in their differentiation into subprocesses (committees) and in the amount of discretion vested in committee or conference chairs to, for example, draft proposals ("negotiating texts"). Finally, in addition to these sets of formal rules, many arenas—particularly those that are in active use over a prolonged period of time—develop their own *in*formal codes of conduct or cultures.

An important research question is generated by these observations: to what extent and how do different rules of access, decision making, and procedure affect the capabilities of arenas to fulfill particular functions in the regime-building process? In other words, how does an institution's design affect its ability to provide actors with incentives to adopt and pursue an integrative, problem-solving approach (Walton and McKersie 1965), provide opportunities for transcending initial constraints (such as by coupling or decoupling issues) (see, e.g., Sebenius 1983), and enhance the institutional capacity to integrate or aggregate actor preferences?

This project cannot cover this comprehensive agenda in full. What we do, however, is focus particularly on *decision rules and procedures*—arguably the most important determinant of institutional capacity to aggregate actor preferences into collective decisions.[25]

Consensus is the default option and also the most commonly used decision rule in international politics.[26] Even though less demanding rules are sometimes adopted in specific contexts, we normally find that consensus is the master principle in one or both of two meanings. First, in a basically anarchical system any other decision rule adopted by a group of states will have to be approved by consensus. Second, in many instances we find that provisions for majority voting are coupled to some kind of right of reservation, meaning that a party who has strong objections to a particular regulation can—by filing a formal reservation by a certain deadline—declare that that decision will not apply to itself.

After unanimity, consensus is the most demanding decision rule there is. If we combine the requirement of consensus with a requirement of *inclusiveness* (meaning that all parties in a given group must join for a solution to be implemented) and assume that each option is evaluated only in terms of its own merits, it leads to "the law of the least ambitious program" (Underdal 1980, 36), meaning that collective action will be limited to those measures that are acceptable to the least enthusiastic party (that is, limited by the Pareto frontier). In fact, even the least ambitious program may prove unattainable in practice. It is easy to see that the amount of collaboration actually achieved may fall short of what even the least enthusiastic

party considers desirable. The rule of consensus gives each party a veto not only over the overall amount of (for example) emission reductions; the right of veto also pertains to every conceivable means of achieving that reduction (including, of course, the distribution of reductions or costs). Moreover, inherent in the process of (distributive) bargaining are certain "perversities," providing "incentives to actors to behave in ways that have the effect of hindering mutually beneficial cooperation" (Keohane 1988, 29; see also Johansen 1979 and Underdal 1987). A well-known economist has even formulated what is sometimes referred to as the "law of bargaining inefficiency," saying that "bargaining has an inherent tendency to eliminate the potential gain which is the object of the bargaining" (Johansen 1979, 520).[27]

Even though pervasive and resilient, there is fortunately nothing inevitable about these "perversities." Institutional arrangements as well as behavioral strategies can be designed to counter such risks. Most fundamentally, institutionalization can help by encouraging actors to extend their time horizons beyond those of one-time encounters (see, e.g., Axelrod and Keohane 1985, 232f) and shift from norms of *specific* to *diffuse reciprocity* (see Keohane 1988). Moreover, as Sand (1990) has reminded us, actors can use several strategies to beat the "law of the least ambitious program." These strategies include the creative use of selective incentives, differential obligations (including loopholes), and promotion of voluntary overachievement by pusher countries (leading to two- or multiple-track cooperation schemes). Even though there can be no doubt that the decision rule of consensus is a major constraining factor in international cooperation, there are indeed quite a few things one can do to enhance the possibilities of cooperation.

To summarize, we hypothesize:

H_4: The establishment of (negotiation) arenas as formal *institutions* that exist and are used over an extended period of time tends to facilitate cooperation and enhance the effectiveness of international regimes by encouraging actors to adopt extended time horizons and norms of diffuse rather than specific reciprocity and by reducing the transaction costs of specific projects.

H_5: In dealing with *malign* problems, the decision rules of unanimity and consensus tend, other things being equal, to lead to less effective regimes than rules providing for (qualified) majority voting. More precisely, the use of majority voting tends to lead to more ambitious regulations (output). However, to the extent that this is accomplished at the cost of sacrificing the interests of significant actors, it will do so at the risk of impairing compliance (outcome). Except for strongly malign issues, the former effect will most often be somewhat stronger than the latter.

Organizations as Actors All organizations can serve as arenas, but only some can also qualify as significant actors in their own right. International organizations can be considered actors to the extent that they provide independent *inputs* into the problem-solving process or somehow amplify *outputs* of these processes. To qualify as actor, an organization must have a minimum of internal coherence (unity), autonomy, resources, and external activity. Without a certain minimum of coherence, an organization cannot be considered *one* actor. Without some autonomy (notably in relation to its members), it would be a mere puppet commanded by its masters. By definition, an actor must somehow in fact *act*. Without a certain minimum of activity within its environment, an organization can hardly be said to play the role of an actor (even though it may have the *capacity* to act). Finally, unless an organization has a certain minimum of resources at its disposal, its own contributions to its activities would tend to be inconsequential.

Organizations, and even specific bodies within international governmental organizations (IGOs), vary considerably in terms of scores on these three dimensions. One obvious candidate for a top score would be the European Union. At the level of specific organizational bodies, the same can be said about the Commission of the European Union. Most of the secretariats serving international regimes seem, however, to find themselves closer to the critical minimum required (particularly, it seems, on the autonomy and resources dimensions) to achieve actor status, and some even fail to meet that threshold. Also other systemic roles, such as those of conference presidents and committee chairs, are most often quite narrowly circumscribed in terms of formal authority.[28] As a consequence, the amount of organizational energy available to pursue "the common good" in international politics is most often only a small fraction of the aggregate amount of energy geared to the pursuit of national interests. We nevertheless believe that variance in the actor capacity of IGOs or subordinate bodies and officials can affect the amount of success obtained in attempts to create and implement international regimes. Thus, we suggest

H_6: Actor capacity on the part of the international organization in charge and its subordinate bodies and officials tends, *ceteris paribus*, to enhance regime effectiveness. The impact tends, other things being equal, to be larger in the case of malign than in the case of benign problems and greater for moderately malign than for strongly malign problems.

The institutional setting itself is not a truly independent variable in the sense of being an exogenous parameter completely beyond the control of the members of

the regime. On the contrary, institutions are social constructions, established and modified through joint decisions. A hard-nosed realist might even argue that international organizations are essentially epiphenomena—puppet instruments that powerful states can use to promote their own interests and will transfer power to only when most or all other options are exhausted (see, e.g., Mearsheimer 1995). The fact that institutions are negotiated entities has two important implications. First, in bargaining about the design of arenas or organizations, each actor is likely to evaluate alternative settings primarily as a means to protecting or promoting its *own* substantive interests. Whenever these interests diverge, as they most often do, the setting may itself become an issue of delicate bargaining. This means that the more malign the substantive issues to be dealt with by a particular institution, the more difficult it will be to reach agreement about the shape of that institution and the weaker and more constrained it is likely to be. Where two or more existing institutions present themselves as alternatives, the same can be said about the choice between or among these institutions. As a consequence, *institutional capacity tends to be most difficult to build up or draw on where it is most needed*. The overall causal relationship thus looks something like this:

$$\text{Problem malignancy} \xrightarrow{-} \text{Decision rule/actor capacity} \xrightarrow{+} \text{Effectiveness}$$

Second, some regimes are served and managed by an organization or body established with a particular purpose in mind, while others are served by an organization established (primarily) for another purpose. In the former case, the institutional setting in which a regime was formed often is different from the setting in which it is implemented. Even when the setting is constant, its significance may change; it is by no means obvious that the institutional arrangements that contribute to success in the phase of regime formation contribute as much in the phase of implementation. Some functional requirements are specific to one particular phase; for instance, implementation and compliance-review mechanisms can no doubt enhance effectiveness at the implementation stage but can hardly contribute much in the phase of regime formation. Similarly, it is easy to see that some of the behavioral strategies that can be helpful in forging agreement—such as the use of what Henry Kissinger referred to as "creative ambiguity"—may jeopardize implementation. Much work remains to be done, however, before we can differentiate with precision and confidence institutional design principles with reference to the various stages that international regimes typically go through (see, though, Wettestad 1999).

Power The more complex the problem and the more demanding the decision rule, the more critical leadership of some kind becomes. And the less formal authority that is vested in systemic actors (such as conference chairs and secretariats), the more important *in*formal sources of leadership become. One such source is *power*, here defined narrowly as the control over important events (Coleman 1973).

Following Coleman (1973), two faces of power may be distinguished. One derives from control over events important to *oneself*; the other from control over events important to *others*.[29] The former provides autonomy—the privilege of being able to pursue one's own interests without having to worry about what others might wish or do. The latter provides an actor with the means to impose its own will on others. The notion of *hegemony* combines the two (see, e.g., Snidal 1985). A *benevolent* hegemon is an actor sufficiently predominant to be able and willing to provide collective goods at its own expense, or—more generally—to establish and maintain unilateral solutions to collective problems.[30] By contrast, the *coercive* hegemon rules by virtue of its control over events important to others, and it uses this control to induce their submissive cooperation or—more bluntly—to impose its own will on them. Either way, power can be a device to bypass or break aggregation deadlock. We may therefore conclude that the existence of a *unipolar* distribution of power tends to enhance the decision-making capacity of a system, and—by implication—also the probability that at least *some* collective decision will be made. In this particular respect, a unipolar distribution of power can be seen as a functional substitute for formal hierarchy or other strong decision rules.

Control over events important to others may be transformed into coercive leadership through at least three different mechanisms. First and most obviously, an actor can use its control over important events as a device to provide selective incentives (rewards) to those who join or comply or to punish anyone who refuses to go along with or defects from an established order. Although perhaps a less predominant mechanism of hegemony than one might think (see, e.g., James and Lake 1989), examples of leadership by (promises of) rewards or (threats of) punishment are not hard to come by, even among friends. One example: in the period of reconstruction after World War II the U.S. government forged economic cooperation in Western Europe by making Marshall aid grants contingent on a commitment to join the Organization for European Economic Cooperation (OEEC) and to accept a modest level of coordination of economic policies. Coercive leadership involves tactical diplomacy; an actor may promise or threaten to do things it would not contemplate except for the purpose of influencing the behavior of others.

Second, unilateral measures taken by a predominant actor for its own benefit may simply set the pace to which other parties will find it in their own interests to adapt. Thus, the industries and governments of the small European Free Trade Association (EFTA) countries have little choice but to adapt to whatever standards the European Union may establish for products or services in their own internal market. They are, to be sure, not automatically bound in a legal sense by EU decisions, and they may even have been able to wield some occasional influence over the substance of EU regulations. But the sheer weight and importance of the European Union as an economic actor is such that when the European Union moves to establish a certain standard for its own area, its small neighbors will probably find it in their own best interests to make sure that they are capable of meeting that requirement—even without any hint from Brussels that such a move would be appreciated.

Coercive leadership may also be exercised through a similar but more indirect and complex mechanism. As described by James and Lake (1989, 6f) it works as follows. A unilateral policy choice made by the strong actor (the hegemon) alters the structure of opportunities facing other societies. The greater the market power of the hegemon, the greater the impact of its actions. As some industries or subgroups begin to take advantage of their new opportunities, these sectors tend to expand relative to others. As a consequence, their economic weight and domestic political influence will tend to increase. Moreover, their private interests tend to become wedded to policies that maintain or reinforce the new order. By causing a restructuring of the *domestic* configuration of interests and by affecting the distribution of political influence *within* other societies, a unilateral policy decision made by the hegemon may generate and strengthen domestic demands within weaker nations for making adaptive adjustments in their own policies.

Since we are interested in the extent to which collective problems are solved, we need to investigate not only whether a system is capable of producing *some* joint decision but also the *substance* of whatever decisions it produces. This implies that we must ask not only whether the distribution of power is unipolar, bipolar, or multipolar but also whether it is skewed in favor of parties advocating strong or weak regulatory measures. We would, in other words, like to know the distribution of power *over the configuration of preferences*.[31] The basic assumption behind this coupling can most simply be stated as follows: the probability that a particular solution will be adopted and successfully implemented is a function of *the extent to which it is perceived to serve the interests of powerful actors*.

At least two mechanisms might disturb this proposition. In dealing with *benign* problems, a benevolent hegemon may well provide collective goods at its own

expense but in doing so may inadvertently weaken the incentives of others to contribute. The net balance, then, depends on the relative strength of these two effects. When it comes to malign problems—particularly those that are characterized by severe asymmetries and cumulative cleavages—concentration of power in the hands of *pushers* might generate fear among *laggards* and possibly also other prospective parties that *their* interests will not be accommodated within the regime. If so, they may very well conclude that they had better not get involved at all. This suggests that at least for problems characterized by severe asymmetry, a rough balance between pushers and intermediaries may in fact be more conducive to the development and implementation of cooperative solutions than a high concentration of coercive power in the hands of the former.

The argument outlined above has been framed essentially with reference to what we might call power in the *basic game* itself—that is, in the system of activities that is the subject of regulation. International regimes are, however, established through political processes and will therefore be influenced also by resources that are specific to those *decision games*. Two important types of decision-game resources are *votes* and *arguments*. Numbers obviously count if the decision rule prescribes voting. Also in a system practicing the rule of consensus, coalition size will often be an asset, exerting social pressure on a reluctant minority. Although certainly a soft currency in international politics, there is often some clout also in the better argument—whether based on superior knowledge or moral stature (Risse 2000). The important point to be made here is that the distribution of such decision-game resources will sometimes differ significantly from the distribution of control over important events in the system of activities to be regulated. Such incongruity may be a severe source of strain. The winning coalition may be tempted to use its control over the decision game to establish rules and regulations that are not acceptable to those who control the basic game. Strong polarization of actors in the upper left-hand and the lower right-hand cells of figure 1.5 is likely to be a good indicator of trouble.

Let us now try to summarize the argument:

P_1: Other things being equal, the more unipolar the distribution of power within a system, the greater is its capacity to aggregate actor preferences into collective decisions. This impact is strongest where decision rules are most demanding.

H_7: Concentration of power in the hands of pushers tends by and large to enhance effectiveness, while concentration of power in the hands of laggards has the opposite effect. But

Actor's prospects of promoting own interests *outside* the regime	Actor's prospects of promoting own interests *inside* the regime: **Poor**	**Good**
Good	Will resist or withdraw Prefers *weak* regime	Will show moderate support Prefers *moderately strong* regime
Poor	Will be passive or indifferent (or act as revolutionary)	Will show strong support Prefers *strong* regime

Figure 1.5
Actor stance as a function of prospects of promoting own interests

- In dealing with *benign* problems, unilateral provision of collective goods by a benevolent hegemon tends, although with some exceptions, to weaken the incentives of others to contribute; and
- In dealing with *malign* problems—particularly those characterized by severe asymmetry and cumulative conflict—concentration of power in the hands of pushers tends to generate fear and withdrawal among laggards. For such problems, the most conducive distribution of power is likely to be one in which the aggregate strength of pushers is roughly balanced by the aggregate strength of intermediaries, with laggards in a weaker position but not completely marginalized.

H_{7a}: The prospects for regime effectiveness tend to decline if the distribution of power in the decision game differs substantially from the distribution of power in the basic game.

Previous research has concluded that power-based propositions by and large perform poorly in empirical tests (see, e.g., Efinger, Mayer, and Schwarzer 1993, 269; Young and Osherenko 1993, 228f). We suspect that this conclusion is due to the way power has been conceptualized and measured—mainly as the concentration of capabilities rather than as the distribution of power over the configuration of interests, and as "overall structural power" rather than control over the system of activities to be regulated. Young (1994, 118f) is certainly right that bargaining leverage in a specific negotiation process cannot be inferred in any straightforward

way from general capability indices. The conceptualization of power proposed above is intended to bridge that gap. We are not at all prepared to dismiss the distribution of power, as conceptualized in this study, as largely irrelevant to the formation and operation of international regimes.

Skill and Energy The third constitutive element in our conceptualization of problem-solving capacity takes us from the study of structure to the study of behavior. In this project we are not able to undertake the kind of in-depth, comparative analysis of individual behavior that would be required to say much about this component.[32] Some general remarks seem nonetheless appropriate to indicate how we see its place in the overall model.

Two basic questions raised by our conceptualization of problem-solving capacity may be formulated as follows: How well can outcomes of regime-formation and -implementation processes be predicted and explained on the basis of knowledge about the structure of problems and systems? To what extent do skill and effort make a significant difference?

Our model is premised on a set of assumptions about what determines problem-solving capacity. Two of these assumptions may be summarized as follows. First, the structure of a decision situation circumscribes, with some elasticity, the range of politically feasible solutions. More precisely, if we know the configuration of actor preferences, the institutional setting (notably "the rules of the game"), and the distribution of power, we can get a fairly good idea of what will be politically feasible and—although with less accuracy and confidence—indicate a more or less narrow range of likely outcomes. Second, for reasons to be indicated below, the *structural logic* of most situations will to some extent be indeterminate and be so perceived by the actors involved. (This is, of course, the implicit rationale for spending time and energy on negotiation efforts.) Some situations, notably those characterized by high complexity or instability, are likely to be seen as more indeterminate than others.

The logic of a negotiation process may be indeterminate for at least three reasons. First, the problem or situation itself may be ambiguous and be perceived differently by the actors involved. The cause-and-effect relationships that we need to understand to cope with environmental and other types of problems are quite often poorly understood. The present state of knowledge about anthropogenic causes of global climate change is a case in point.

Second, even if actors agree completely on how to describe a situation, its policy implications may be anything but obvious. A closer look at formal bargaining theory

itself provides instructive evidence. Research in the axiomatic-static tradition of Nash (1950) has produced several solutions to the same bargaining problem, none of which has so far been conclusively authorized as uniquely compelling by rational choice criteria and even less so by normative standards. Even if one of these general formulas were to emerge as *the* unique solution, we should recognize that the more complex the game to which it is applied, the less conclusive the specific, operational advice it can produce tends to become. A cursory glance at Axelrod's (1984) computer tournaments suffices to demonstrate (1) that once we iterate even the most simple two-actor prisoner's dilemma game for an unspecified number of rounds, it is by no means obvious what the optimal strategy is and (2) that the choice of strategy can make a significant difference with regard to outcomes. Decision makers will probably often find clues and be able to converge on focal points that are alien to formal game theory (Schelling 1960). But it seems likely that just as often they will bring to the problem a wider range of policy interpretations, rendering its structural logic more rather than less ambiguous.

Third (and more important), international problem solving is a different kind of game. While the players portrayed in formal bargaining-theory models face a problem of making a collective choice of one solution from a predefined set of options, actors in international negotiations typically enter negotiations with incomplete and imperfect information and perhaps also with tentative and vague preferences (Iklé 1964, 166f). This implies that *discovering*, *inventing*, and *exploring* possible solutions may be important elements of the process (see, e.g., Walton and McKersie 1965; Winham 1977; Zartman and Berman 1982). Search, innovation, and learning are processes that can hardly be captured in deterministic models. Moreover, while formal rational-choice theory pictures actors as being "prisoners" of a game that is exogenously defined, actors in real-life negotiations can redefine their game. Together they can determine the institutional setting and redefine the nature of the problem. And even though the basic interests and values of states are likely to prove quite nonsusceptible to modification in the short run, an actor may be able to influence the specific positions of its prospective partners through various kinds of techniques—including persuasion, manipulation, and coercion—or by more or less covert intervention in foreign games of domestic politics (see, e.g., Putnam 1988).

Without further specification, the argument that skill and effort *can* make a difference is not in itself a useful proposition. It becomes interesting only to the extent that we can specify, with a fair amount of precision, what constitutes *skill* and *energy* in this context and how these factors influence cooperation processes. This

is a complex challenge that we do not take on here. All we can do here is to explore briefly just one dimension that is central to our interest in explaining regime effectiveness—namely, the political engineering of effective solutions.

The political engineering of international cooperation includes at least three major tasks. One is the design of *substantive solutions* that are politically feasible. The most restrictive approach to solution design would take the institutional setting, actor preferences, and the distribution of power as exogenously given parameters and ask three main questions: (1) What are the *minimal* requirements that a solution must meet to be adopted and implemented? (2) What is the *maximum* (in terms of efficiency or fairness) that we can hope to accomplish? And (3) How would we design a solution if our only concern were to maximize its political feasibility? Another major task is to design *institutional arrangements* that are conducive to the development, adoption, and implementation of effective solutions. Here, too, the configuration of preferences and the distribution of power are often taken as givens, but institutional arrangements can themselves have a significant impact on actors' willingness to enter into cooperative arrangements. The third design target is *actor strategies*—notably, strategies that can be effective in inducing the constructive cooperation of prospective partners.

This is not the place to examine the anatomy of leadership or try to formulate relevant design principles.[33] All we can hope to do in the empirical analysis is to explore to what extent one or more of these functions of *instrumental leadership* was in fact performed in the various regime-formation and -implementation processes. The basic hypotheses guiding this part of our analysis can be stated simply as follows:

H_8: Instrumental leadership tends to facilitate regime formation and implementation. And the more skill and energy that are available for and actually invested in instrumental leadership, the more likely an effective regime will emerge.

H_{8a}: The *need* for instrumental leadership tends to increase with problem malignancy. However, *supplying* such leadership tends to become increasingly difficult as malignancy increases. Instrumental leadership thus tends to make the most difference in dealing with problems that are *moderately malign*.

Instrumental leadership may come from several sources: officers of intergovernmental organizations, conference or working group chairs, national delegates, and transnational organizations or informal networks. The former two will be subsumed under the notion of IGO actor capacity. With regard to informal transnational networks, we focus on those referred to as *epistemic communities* (Haas 1990, 55;

1992, 3).[34] According to Haas and his colleagues, such communities can play an important role in the establishment of and, at least in some cases, in the operation of international environmental regimes:

H_9: Informal networks of experts—*epistemic communities*—contribute to regime effectiveness by strengthening the base of consensual knowledge on which regimes can be designed and operate. The more integrated an epistemic community and the deeper it penetrates the relevant national decision-making processes, the more effective—*ceteris paribus*—the regime it serves tends to be.

Finally, let us point out that skill should not be considered a *constant*. On the contrary, there are many good reasons to believe that actors learn from their own experience as well as from knowledge supplied by others. Thus, we hypothesize that

H_{10}: Skills in designing effective regimes tend to improve over time as actors learn. This implies that regimes tend to become more effective over the first decade or two of their lifetime and that the latest generation of regimes tends to be—by and large—more effective than regimes of previous generations.[35]

Summary

The essence of the line of reasoning that we have outlined above can now be summarized in a rather simple core model (see figure 1.6). As this graphical representation indicates, our core model has important limitations, two of which remain to be spelt out clearly. First and most important, it considers each regime as a stand-alone arrangement and assumes that actors evaluate it on its own merits only. In the real world, of course, context matters. Regimes may be embedded, nested, or in some other way linked to other institutions, and these links may be sources of strength or weakness. Moreover, a government has multiple concerns and objectives, and some of these may well influence its negotiation and implementation behavior. We try to determine whether such links and ulterior motives significantly affect regime formation and operation, but we do not study the impact of such contextual elements in depth. Second, figure 1.6 offers a *static* picture. A dynamic representation would also include feedback lines. For example, an effective regime may well, over time, transform the structure of the problem it addresses or enhance the problem-solving capacity of the system of which it is a part. Again, these are effects we recognize and explore, but only summarily. The comparative analysis focuses essentially on the factors included in the core model.

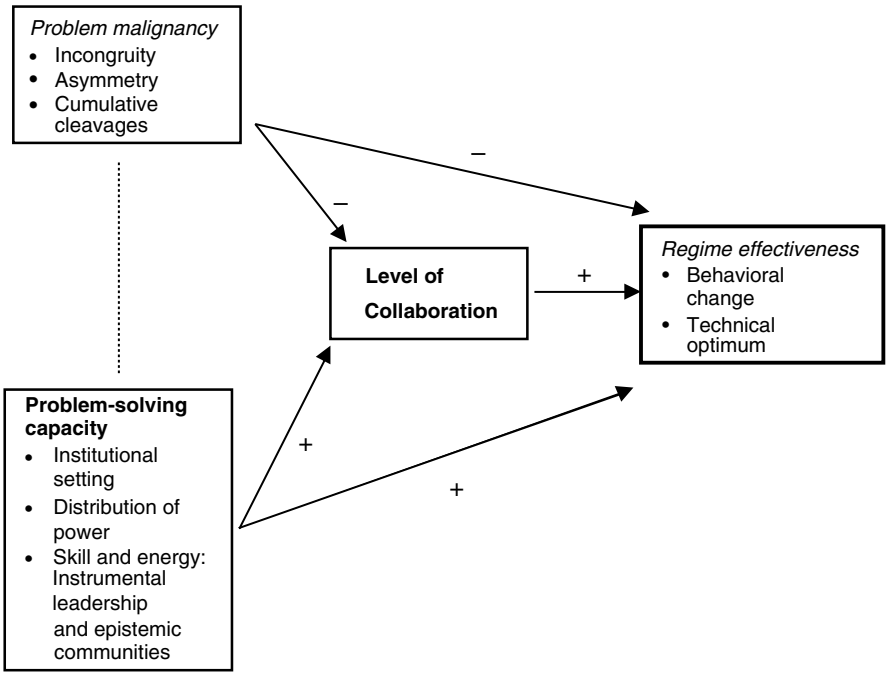

Figure 1.6
The core model

Empirical Evidence

In part II we examine these hypotheses against evidence from fourteen international regimes (see table 1.3). All but one of these (nuclear nonproliferation) deal with environmental protection or management of natural resources.

This set of cases has been selected on the basis of two main criteria. Our principal concern has been to make sure that we had a sufficient range of variance on our dependent variable (regime effectiveness) as well as in terms of problem structure, institutional setting, and distribution of power. Moreover, we wanted to ensure variance not only across cases but also *within* regimes, across components or phases. A second—and also a secondary—criterion has been practical feasibility. To be able to cover such a relatively large number of cases in some depth, we decided to draw heavily on previous work in which we have ourselves been involved over the years and—wherever relevant—to utilize available data and material to which we already had access. Even though we consider such a pragmatic approach to be

Table 1.3
Overview of cases

Regimes
1. Satellite telecommunications
2. Dumping of low-level radioactive waste at sea
3. Ship-generated oil pollution
4. Oslo Convention
5. Paris Convention
6. Barcelona Convention
7. Long-range transport of air pollutants (LRTAP)
8. Stratospheric-ozone regime
9. International trade in endangered species (CITES)
10. International Whaling Convention (IWC)
11. High-seas salmon in the North Pacific
12. Antarctic resources (CCAMLR)
13. South Pacific tuna
14. Nuclear nonproliferation

perfectly legitimate, we should like to emphasize that we make no claim whatsoever that our set of cases is in some sense a *representative* sample of the universe of international regimes. In fact, we know that it is *not*. For one thing, it is clearly biased in terms of substantive problem areas; as pointed out above, all but one of the regimes included deal with environmental protection or resource management.[36] Less obvious but perhaps more important is the fact that our set includes only cases in which at least some level of cooperation has been achieved. Research on collective problems that have integrative potential but have *not* generated cooperation may well have shed important new light on the causes of failure and thus enabled us to transcend conventional perspectives in the study of international regimes. We recognize that this category of problems remains a neglected field of study but have to leave it as a challenge for future research.

In part II, we organize our fourteen cases into three broad categories according to scores on our *dependent* variable. We begin with those regimes that we have scored as effective (in comparative terms) and end with those we have scored as relatively ineffective. For each of these categories we provide a brief introduction, summarizing what we expect to be common, distinctive characteristics of each category. We hope that these summaries help the reader keep the essence of

our theoretical argument fresh in mind when examining the details of each of our case stories.

Notes

1. The discussion in this section is based on Underdal (1992a).

2. One option that has been frequently used in previous research is to focus on the formal properties of the regime—such as level or scope of cooperation, usually defined in terms of the kind of functions fulfilled (information exchange, rule making, rule enforcement, and so on) (see, e.g., Kay and Jacobson 1983, 14–18). Others (including Aggarwal 1985, 20; Zacker 1987; and Haggard and Simmons 1987) have focused on notions of regime strength, meaning—for Aggarwal—"the stringency with which rules regulate the behavior of countries." For more recent contributions that are closer to the concept used in this study, see, e.g., Young (1994), Levy, Young, and Zürn (1995), Bernauer (1995), and Helm and Sprinz (1999).

3. We are, though, struck by the fact that much of the political discussion seems to focus mainly on notions of gross benefits.

4. The life of a regime should be seen as a *sequence* of events. At the stage of regime *formation*, level of collaboration is a feature of the arrangement being developed (a *product*). However, once a regime is established and it enters the implementation stage, level of collaboration may be seen as a component of its (institutional) capacity.

5. This formulation does not imply an assumption that a new regime will necessarily improve the present state of affairs. As conceived of here, relative improvement can be negative as well as positive.

6. Although analytically distinct, the two perspectives are often hard to distinguish in practice. For example, faced with a piece of advice from a scientific advisory body, decision makers may find it hard to determine whether the solution prescribed is considered optimal or merely satisfactory.

7. The latter is an important proviso. If a group of actors succeeds in accomplishing all that could be accomplished given the best knowledge available by the time, any distance remaining to the objective optimum would be a failure of *knowledge making* rather than *policy making*. As students of politics, we are more concerned with the latter than with the former.

8. One might also interpret relative improvement as the standard that better fits a *remedial* orientation or an *incrementalist* perspective and distance to the collective optimum as the standard that makes more sense in a *synoptic* approach to decision making (see Braybrooke and Lindblom 1963).

9. They suggest conceiving of effectiveness in terms of *actual* improvement expressed as a fraction of *potential* or maximum improvement. In game-theory terms, the formula can be written as follows:

$$\frac{(\text{Actual regime solution}) - (\text{Noncooperative solution})}{(\text{Fully cooperative solution}) - (\text{Noncooperative solution})}.$$

Applied to their case (LRTAP), the *actual* regime solution is the actual change in emissions of a particular pollutant (such as SO_2) under the regime; the *noncooperative* solution is the

change in emissions that would have occurred in the absence of the regulation in question; and the *fully cooperative* solution is the change in emission levels required to maximize social welfare in the region.

10. We do realize that this formulation leads into intriguing conceptual problems if we accept the claim made by Puchala and Hopkins (1982, 247) that "a regime exists in every substantive issue-area in international relations where there is discernibly patterned behavior." The notion of a *no-regime* condition seems to require a stricter definition of *regime*, notably one where the existence of *explicit* norms, rules, and procedures is considered a defining characteristic.

11. The choice between human welfare or ecological sustainability may be seen as a choice of *metric* but also as a choice of evaluation *standard*. Thus, there is a close link between evaluation standard and metric of measurement.

12. One regime dominates another if and only if it has a higher score on one criterion and at the same time is inferior on none of the other relevant criteria. Strong dominance requires one regime to have a higher score on *all* relevant dimensions.

13. *Process effectiveness* is defined by Young (1994, 146) as a matter of "the extent to which the provisions of an international regime are implemented in the domestic legal and political systems of the member states as well as the extent to which those subject to a regime's prescriptions comply with their requirements." A regime is effective in *constitutive* terms if "its formation gives rise to a social practice involving the expenditure of time, energy, and resources on the part of its members" (1994, 148).

14. The latter occurs mainly where imperfect knowledge leaves a "veil of uncertainty" around the *distribution* of costs and benefits (see, e.g., Brennan and Buchanan 1985; Young 1989). As a simple rule of thumb, we might say that uncertainty tends to interact negatively with political conflict to the extent that it can be interpreted as undermining the overall rationale for action but can potentially facilitate agreement to the extent that it makes it hard for each actor to predict the *distribution* of costs or benefits that would flow from the choice of a certain policy option. In conflict-of-interest situations, *in*determinate distributions tend to generate politically more benign games than those that are determinate (everything else being constant).

15. Besides, we know of no straightforward method for determining the intrinsic intellectual complexity of a problem.

16. Substantive-issue typologies, such as the one suggested by Czempiel (1981), also belong to this category.

17. A more technical description is provided in appendix A.

18. One of the points of contention between realist and liberal approaches to the study of international cooperation is that the former see competition as a more pervasive characteristic of interstate relations than the latter. One argument is that even goods that on their own might have been considered absolutely assessed are in many cases indirectly linked to military capabilities—the ultimate relatively assessed good in an anarchical system (see, e.g., Grieco 1990; Hasenclever, Mayer, and Rittberger 1997).

19. All hypotheses are subject to the *ceteris paribus* ("other things being equal") condition.

20. I am grateful to Jon Hovi for reminding me that this relationship is curvilinear. As we approach a zero-sum conflict, the potential gains from cooperation approach zero.

21. As indicated above, the interaction goes both ways: not only does uncertainty that can be interpreted as questioning the rationale for action add to the political intractability of malign problems, but political malignancy tends in turn to contaminate and thereby impede the development of (consensual) knowledge.

22. This is not intended to be an exhaustive list. It is easy to think of other elements. Some of these are captured in the notion of the social capital of a system. At a very general level, *social capital* can be defined as "the arrangement of human resources to improve flows of future income" (see, e.g., Ostrom 1995, 125–126). This definition includes institutions (in a broad sense), networks, and even common beliefs.

23. Moreover, as explained above, our interest in some dimensions of problem-solving capacity grew along the way. We started out with a somewhat narrower focus on problem structure and a small set of regime-design questions.

24. In fact, the latter seems sometimes not to be a strictly necessary condition; failure to pay one's membership fee seems in some international organizations *not* to lead to automatic exclusion.

25. Although decision rules are relevant for both benign and malign problems, they gain particular importance when preferences diverge, as they do when issues are politically malign.

26. In multilateral conferences, a distinction is often made between *unanimity* (requiring the positive approval of all) and the somewhat less demanding rule of *consensus* (requiring "only" the absence of any formal, substantive objections).

27. He then ominously and correctly adds: "It may in fact eliminate *more* than the potential gain" (Johansen 1979, 520).

28. Nonetheless, incumbents often succeed in using such roles as an important basis for exercising influence—sometimes to promote private or national rather than common interests.

29. In formal terms, actor A's direct power over actor B with regard to a specific issue (*i*) can be conceived of as a function of B's relative interest in *i* (U_{iB}) and A's share of control over *i* (K_{iA})—that is, ($U_{iB} \cdot K_{iA}$). As pointed out by Bacharach and Lawler (1981), *interest* in a particular event works two ways: it increases willingness to pay (and hence serves as a source of bargaining weakness), but it also motivates greater tactical effort (which can be a source of strength). It is not obvious which of these effects is the greater.

30. This is equivalent to saying that its own cost-benefit calculations correspond fairly well to that of the group or system.

31. In other words, rather than following the conventional path of treating interest-based and power-based approaches as competing rivals (see Levy, Young, and Zürn 1995; Hasenclever, Mayer, and Rittberger 1997), we *combine* them.

32. To get an idea of what would have been required, see, e.g., Miles (1998).

33. For an attempt to explore what will qualify as politically feasible solutions, see Underdal (1992b).

34. Haas (1992, 3) defines an *epistemic community* as a "network of professionals with recognized expertise and competence in a particular domain and an authoritative claim to policy-relevant knowledge within that domain or issue-area. These communities are, furthermore, characterized by (1) a shared set of normative and principled beliefs, (2) shared

causal beliefs, (3) shared notions of validity, and (4) a common policy enterprise. Sebenius (1992, 354) suggest that epistemic communities can be seen as "de facto, cross-cutting natural coalitions of 'believers.' "

35. The *ceteris paribus* condition is critical in this case. A plausible hypothesis would be that international cooperation tends to start with problems that are relatively easy to solve and then perhaps to move on to deal with more malign problems. Such a pattern would lead us to expect that new steps will tend to become increasingly *difficult*, requiring greater problem-solving capacity to achieve the same level of effectiveness.

36. The one exception—nuclear nonproliferation—has been included mainly to give us an opportunity to explore whether the dynamics of regime formation and implementation in a field closely linked to national security differ substantially from what we can observe in a low-politics area such as environmental protection and resource management. We fully realize, of course, that one single case cannot provide an adequate control sample, and we certainly make no claim of having penetrated the field of national security. At the same time, we do believe that the general line of reasoning outlined in this chapter is valid also beyond the field of environmental regimes. If national security is, as some claim, a very different ballgame, then even one case study may suffice to show that we had better abstain from generalizing our propositions and findings to other issue areas.

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