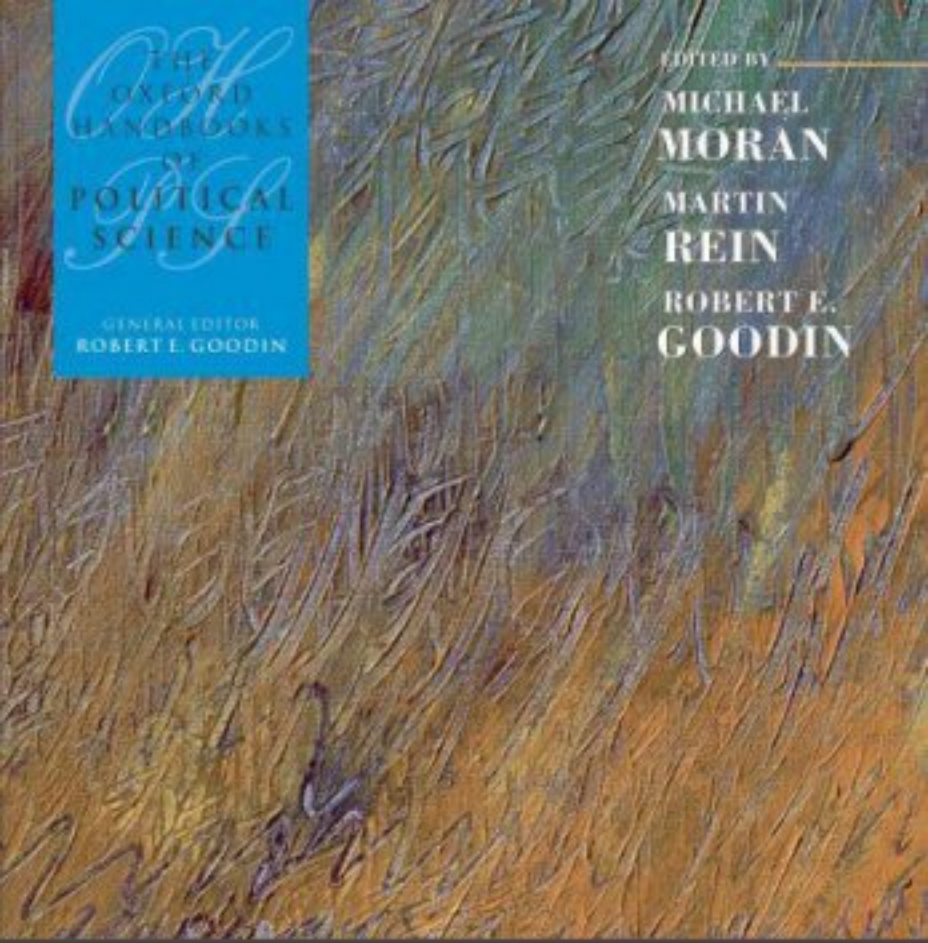

The Oxford Handbook *of* PUBLIC POLICY

public data will allow, and our interviews suggest that classified simulations produce similar results" (1989, 213). Salman, Sullivan, and Van Evera conclude their article by arguing that rigorous dynamic systems analysis should "define serious" nuclear discourse and determine what gets published:

> Policy concerns will always distort balance assessment to some degree, but scholars of security affairs can mitigate the problem by setting and enforcing higher professional standards. Specifically, they could require that research purporting to measure American nuclear strength, or dealing with issues that require its measurement, provide dynamic analysis that tests the propositions its advances. The provision of such information should define serious work on strategic nuclear issues; manuscripts that omit it should not be published or cited as authority. The academic community can impose such standards if it chooses, and the quality of net assessment will improve if it does. (1989, 244 5)

Thus, even as they document the sloppy use of policy modeling, Salman, Sullivan, and Van Evera simply propose better modeling. They have not, apparently, understood how there was both on the one hand, no way for the modeling to be more accurate, and on the other hand, how the modeling itself began to make the nuclear world.

6. Conclusion: How Abstraction Makes a World

Systems analysis was intended to help policy makers understand the complex and essentially unknown nuclear world and assist them in making the policy process more rational. It was intended to produce usable knowledge, to quantify and model the nuclear world. As Enthoven and Smith (1971, 64) say, "In any analysis, the assumptions drive the conclusions:" the virtue of systems analysis was the ability to use it to explore "all assumptions" and, "In this important sense, systems analysis becomes a method of interrogation and debate suited to complex issues. . . . a set of ground rules for a constructive and divergent debate." But while Enthoven and Smith recognize that assumptions drive the conclusions, they and other users of systems analysis were less than attentive to the ways that systems analysis is not simply analysis. The political-military discourse—in the sense of what we do and don't talk about, and how we talk about it—was structured in subtle and not so subtle ways by systems analysis.

As Enthoven and Smith suggest, "The issue here is not numbers versus adjectives, but clarity of understanding and expression. Numbers are an important part of our language. Where a quantitative matter is being discussed, the greatest clarity of thought is achieved by using numbers, even if only expressed as a range" (1971, 69). Yet as one prominent systems analyst wrote, "Quantification is desirable, but it can be overdone; if we insist on a completely quantitative treatment, we may have to

simplify the problem so drastically that it loses all realism" (Quade 1968*b*, 359). But systems analysis did more than abridge nuclear reality too far. Systems analysis, a "knowledge"-making process that is embedded in the organizational routines of government offices, private think tanks, and sometimes part of public debate, began to make its own "reality" more than that reality was simply uncovered, understood, or even obscured through its techniques. How did systems analysis do this?

As scholars of nuclear discourse have shown, the "clean," precise, sometimes humorous ways that strategic nuclear planners talk and write to each other about nuclear weapons and nuclear war distances them from the reality of nuclear use and enables them to contemplate using nuclear weapons without political and moral connotations.[29] The abstractions of nuclear discourse also help to "conventionalize" nuclear weapons—that is, make them appear to be more benign, like non-nuclear weapons. The conventionalization of nuclear weapons is illustrated by their inclusion in conventional war planning scenarios and of nuclear weapons among conventional forces. Conventionalization is also seen in the way that blast effects are "privileged" in systems analysis while thermal and radiation effects are usually given secondary status if considered at all (Eden 2004).[30] That nuclear weapons be seen as more like conventional weapons, whose use is more familiar, whose consequences are believed to be less totally devastating, is important because the ability to contemplate their use, and actually to use them requires that the users not be afraid of the violence entailed in making and using the weapons. In this way, nuclear weapons are demystified, normalized, and familiarized for the specialists in violence.

On the other hand, the informal and formal systems analysis discourse on nuclear weapons has the opposite effect when it is used by specialists in violence to mystify the weapons and the strategy. So, the formal discourse limits those from outside the strategic nuclear weapons analytical community from understanding, much less critiquing nuclear arguments on the technical level at which they are conducted. In this way, the technical discourse of policy modeling decreases the accountability and transparency of the policy modeling process not only to ordinary citizens and to non-expert decision makers.

But, the consequences of abstraction go beyond conventionalization and mystification. The linguistic abstractions and the mathematical procedures do more than numb; they also mystify the subject *for and among the experts*. The ways that nuclear weapons help shape our understanding of and relations to others, what the weapons do to our own and others' bodies, and how making and preparing to use the weapons structures our ways of organizing ourselves, economically, politically, and militarily, is obscured through the practice. In systems analysis, the focus is on technique, and by using systems analysis, we simultaneously move further from (by omission and

[29] See e.g. Cohn 1987; Eden 1991; Green 1966; Gusterson 1996; Nash 1981; Thompson 1981.

[30] The exception is the consideration of radiation effects in the battlefield, and in the case of the neutron bomb a design where radiation effects are "boosted" over blast effects.

abstraction) and closer to (through a focus on detail and a *sense* of precision) the acts of nuclear violence. It was not just the fact that planners were dealing with a world and conditions that they had never encountered that shaped their conclusions and practices.[31] The formal discourse abstracts the logic and uses of nuclear weapons to the point where the consequences of making and using nuclear weapons are not fully appreciated by the experts, much less raised. Omission, elision, assumption, and false precision were layered upon opacity, hedging, and imprecision. Thus, the most fundamental workings of the logic, belief, and arguments are no longer questioned, debated, re-examined, or perhaps even remembered, much less fully understood by those within the intricate discourse.

Thus, systems analysis became its own baroque and self-fulfilling construction. As a consequence, few analyses looked at the effects of a nuclear "exchange" in the aggregate—counterforce exchange models focus on the effects on weapons, and human deaths are rarely counted in those models (e.g. CBO 1978*a*; Salman, Sullivan, and Van Evera 1989). Instead, "The question of military or political victory if deterrence fails would depend upon the net surviving destructive capacity of the two sides after the initial counterforce exchanges" (Nitze 1976, 213). Even when numbers of humans injured or killed in a nuclear war are modeled and discussed, analysts have often argued over whether the right assumptions went into the models and the correct quantities were being given in the conclusions (Drell and von Hippel 1976). The debate in other words, was about improving the models so as better to represent the nuclear reality.

But the logic of modeling and its application begins to make its own world, both a cognitive and a real world. Greater numbers of weapons were often "required" as a result of the analysis while the assumptions and results of systems analysis also tended toward increasing the sophistication of weapons and their delivery systems. Thus, systems analysis compounded the effects of other factors that were pushing the development, production, and deployment of ever greater numbers of nuclear weapons—organizational interests, pork barrel politics, technical innovation, and action–reaction dynamics. One had to hedge against failure. Planners assumed that cities would be destroyed with blast (rather than thermal) effects, requiring more and also more accurate nuclear weapons than would otherwise be necessary to destroy a city. In the quest to reduce uncertainty for their own side in a nuclear war scenario, nuclear planners increased the number of nuclear weapons and improved their capabilities (accuracy and range) and this increased uncertainty for the other side, which then boosted as best they could their own nuclear capabilities. Further, these scenarios presume a larger conflictual context, and from within these scenarios of deterrence and war fighting, there is no way out of the conflictual contest. The analysis is often so abstract and disaggregated that the nuclear world is rarely glimpsed for what it is or, more to the point here, how it is

[31] Eden (1990) stresses the fact that nuclear outcomes have not been "enacted." Also see Derrida 1984. Adler describes the " 'imaginary' science of nuclear strategy" (1992, 107).

being made.[32] The ways that policy modeling and subsequent preparations for nuclear war reinforce the conflictual context were generally left out of the analysis. Reflexivity was driven out of the process. The unknowable is made known and superficially precise by these formal abstractions, but the price of making it "known" was to paradoxically decrease security.

That there was a nuclear world—nuclear weapons and a nuclear arms race—was *not* the consequence of systems analysis. Nuclear weapons don't just appear out of thin air to meet the requirements of nuclear planners. What humans have done, in their concrete actions in preparing to use nuclear weapons, is to create elaborate systems for the production, further development, stockpiling, transportation, and use of nuclear weapons. Weapons planners and militaries also developed plans and means for the protection of nuclear forces from attack by other nuclear weapons.

In sum, the material and the ideational came together in systems analysis—which should not be surprising since that was the goal of the practice. The nuclear world was in part remade when, based on nuclear "rationality," one side constructed its nuclear forces, thereby mobilizing and making the nuclear world of development, production, stockpiling, and deployment of nuclear weapons. When the other side responded by political or military means, to the forces and policies in part determined by systems analysis, the entire context was further shifted. Good analysts change or redo their calculations when conditions change, and some of the arguments that follow from their analysis may be used to change the world of weapons and strategy yet again. The strategic nuclear belief system existed and elaborated itself, impelled by its own logic, and was only partially stopped by a major shift within the larger political system, the end of the cold war. That the nuclear arms race ended was not the result of some change in systems analytical practices. But nuclear operations research and systems analysis helped make it the kind of nuclear world it became.

Several questions remain. First, in trying to understand the enormous nuclear arsenal of the USA, can one separate the effects of other forces such as organizational biases, from the effects of systems analysis? Was systems analysis too embedded in other processes to be considered as a force on its own? Second, I have not shown why analysts recognized and cautioned against "pitfalls" in using systems analysis, but nevertheless continued to ignore the caveats the best among them would state. Rational actor and cybernetic theories of decision might argue that complex problems will be simplified by decision makers. But why were certain behaviors (such as the tendency to recognize that implausible assumptions were being made, and to make them anyway) so common in systems analysis? Third, why was systems analysis

[32] The brilliance of the anti nuclear activists who argued against all nuclear weapons modernization, and for the abolition of nuclear weapons, was that they drew the whole nuclear "reality," especially the futility of civil defense, to the forefront and ignored arguments about numbers of survivable nuclear forces. Anti nuclear activists who argued from within the discourse of nuclear planners (see e.g. Forsberg 1982) were sometimes perhaps co opted in some ways by the logic of nuclear analysis.

adopted over other methods of analysis? Are or were there plausible alternatives to systems analysis? Fourth, to what extent was systems analysis, or something like it a part of Soviet military planning?

Finally, turning to counterfactuals, what would US nuclear planning have looked like in the absence of the practice of systems analysis? Would there have been even more nuclear weapons of greater destructive capability? Did systems analysis actually function as a tool to constrain the organizational and pork barrel elements of the military and politics? Or, would US nuclear weapons policies have been more or differently "rational" without systems analysis? In other words, nuclear forces might have been designed by other criteria, such as Clausewitzian or Just War views of proportionality of political purpose and military means. The best strategists recognized the dilemma of trying to deal with the unknown through policy analysis. As Brent Scowcroft said in congressional testimony on the MX missile that hints at both the role and the limits of any kind of analysis:

> We have argued among ourselves for years about what is an adequate deterrence. It doesn't really matter. We will never know what is an adequate deterrence unless these weapons are used, and then we will know what was not an adequate deterrence.
>
> What we have to try to do, though, is to calculate as best we can what is in the minds of the Soviet Union. That is a very difficult thing to do. Deterrence is an attitude, a frame of mind. The best we can do is look at the kind of things they do, the kinds of systems they deploy, the kind of things they rely on, the kinds of defenses that they develop to ascertain what might be an adequate deterrence. (HASC 1983, 95)

Although Scowcroft said, "It doesn't really matter," of course it did matter what the USA built, how much it cost, and how the Soviets reacted. Scowcroft was simply acknowledging the inadequacy and indeed, absurdity of the nuclear policy modeling process. But even Scowcroft failed to recognize that the technical arguments and in particular the policy modeling process itself, were part of the process driving the arms race.

References

Adler, E. 1992. The emergence of cooperation: national epistemic communities and the inter national evolution of the idea of nuclear arms control. *International Organization*, 46: 101 45.

Arkin, W., and Fieldhouse, R. W. 1985. *Nuclear Battlefields: Global Links in the Arms Race*. Cambridge: Ballinger.

Ball, D. 1980. *Politics and Force Levels: The Strategic Missile Program of the Kennedy Admin istration*. Berkeley: University of California Press.

1986*a*. Toward a critique of strategic nuclear targeting. In Ball and Richelson 1986, 15 32.

1986*b*. The development of the SIOP, 1960 1983. In Ball and Richelson 1986, 57 83.

and Richelson, J. (eds.) 1986. *Strategic Nuclear Targeting*. Ithaca, NY: Cornell Univer sity Press.

Batcher, R. T. 2004. The consequences of an Indo Pakistani nuclear war. *International Studies Perspectives*, 6 (4): 135 62.

Blair, B. 1985. *Strategic Command and Control: Redefining the Nuclear Threat*. Washington, DC: Brookings Institution.

Brewer, G., and Shubik, M. 1979. *The War Game: A Critique of Military Problem Solving*. Cambridge, Mass.: Harvard University Press.

Broad, W. J. 1993. Russian says Soviet atom arsenal was larger than West estimated. *New York Times*, 26 Sept.

Brown, M. 1994. *Flying Blind*. Ithaca, NY: Cornell University Press.

Bunn, M., and Tsipis, K. 1983. The uncertainties of a preemptive nuclear attack. *Scientific American*, 249 (Nov.): 38 47.

Carter, A., Steinbruner, J., and Zraket, C. (eds.) 1987. *Managing Nuclear Operations*. Washington, DC: Brookings Institution.

1978*b*. *Retaliatory Issues for the U.S. Strategic Nuclear Forces*. Washington, DC: Govern ment Printing Office.

Chilton, P. (ed.) 1985. *Language and the Nuclear Arms Debate: Nukespeak Today*. London: Frances Pinter.

Cimbala, S. J. 1995. Deterrence stability with smaller forces: prospects and problems. *Journal of Peace Research*, 32 (Feb.): 65 78.

Cohn, C. 1987. Sex and death in the rational world of defense intellectuals. *Signs*, 12: 687 718.

Congressional Budget Office (CBO) 1978*a*. *Counterforce Issues for the U.S. Strategic Nuclear Forces*. Washington, DC: Government Printing Office.

Crawford, N. C. 1987. *Soviet Military Aircraft*. Lexington, Mass.: Lexington Books.

Davis, L. E., and Schilling, W. R. 1973. All you ever wanted to know about MIRV and ICBM calculations but were not cleared to ask. *Journal of Conflict Resolution*, 17: 207 41.

Davis, P. (ed.) 1994. *New Challenges for Defense Planning: Rethinking How Much is Enough*. Santa Monica, Calif.: RAND.

Department of Defense (DOD) 1994. *Nuclear Posture Review*. Unclassified version. Washington, DC: Department of Defense.

2002. *Nuclear Posture Review*. Unclassified excerpts; available at: www.defenselink.mil/ news/Jan2002/d20020109npr.pdf;www.globalsecurity.org/wmd/library/policy/dod/npr.htm.

Director of Defense Research and Engineering (DDR&E) 1964*a*. Damage limiting: a rationale for the allocation of resources by the US and USSR. 21 Jan.

1964*b*. A summary study of strategic offensive and defensive forces of the U.S. and USSR. 8 Sept.

Derrida, J. 1984. No apocalypse, not now (full speed ahead, seven missiles, seven missives). *Diacritics*, 14 (Summer): 20 31.

Dougherty, R. 1987. The psychological climate of nuclear command. In Carter, Steinbruner, and Zraket 1987, 407 25,

Drell, S. D., and von Hippel, F. 1976. Limited nuclear war. *Scientific American* (Nov.) In Russett and Blair 1978, 144 54.

Eden, L. 1990 The hypothetical organization: organizational learning and interpretation in U.S. strategic nuclear targeting. Paper presented at the Annual Meeting of the American Political Science Association, Chicago.

1991. Sterilizing destruction: the imaginary battlefield in contemporary U.S. nuclear targeting. Paper presented at the Annual Meeting of the American Historical Association.

2004. *Whole World on Fire: Organizations, Knowledge, and Nuclear Weapons Devastation.* Ithaca, NY: Cornell University Press.

and Miller, S. (eds.) 1989. *Nuclear Arguments.* Ithaca NY: Cornell University Press.

Enthoven, A. C., and Smith, K. W. 1971. *How Much is Enough? Shaping the Defense Program, 1961 1969.* New York: Harper and Row.

Evangelista, M. 1988. *Innovation and the Arms Race: How the United States and the Soviet Union Develop New Military Technologies.* Ithaca, NY: Cornell University Press.

Forsberg, R. 1982. A bilateral nuclear weapon freeze. *Scientific American*, 247 (Nov.): 52 61.

Francis, S. 1995. Warhead politics: Livermore and the competitive system of nuclear weapon design. Ph. D. dissertation, Massachusetts Institute of Technology.

Freedman, L. 2003. *The Evolution of Nuclear Strategy*, 3rd edn. New York: Palgrave.

Glaser, C. L. 1990. *Analyzing Strategic Nuclear Policy.* Princeton, NJ: Princeton University Press.

Glasstone, S., and Dolan, P. J. 1977. *The Effects of Nuclear Weapons*, 3rd edn. Washington, DC: Government Printing Office.

Green, P. 1966. *Deadly Logic: The Theory of Nuclear Deterrence.* Columbus: Ohio State University Press.

Greenwood, T. 1975. *Making the MIRV: A Study of Defense Decision Making.* Cambridge: Ballinger.

Gusterson, H. 1996. *Nuclear Rites: A Weapons Laboratory at the End of the Cold War.* Berkeley: University of California Press.

Haas, P. 1992. Introduction: epistemic communities and international policy coordination. *International Organization,* 46: 1 35.

Hilts, P. J. 1994. Inquiry links test secrecy to a cover up. *New York Times*, 15 Dec.

1995. Healthy people secretly poisoned in 40's test. *New York Times*, 19 Jan.

1996. Payments to make amends for secret tests of radiation. *New York Times*, 20 Nov.: A1.

Hitch, C. J. 1965. *Decision Making for Defense.* Berkeley: Unversity of California Press.

House of Representatives, Armed Services Committee (HASC) 1983. Hearings on H.R. 2287 Department of Defense authorization of appropriations for fiscal year 1984: part 2 of 8 parts strategic programs. Washington, DC: Government Printing Office.

International Institute for Strategic Studies (IISS) 1989. *The Military Balance, 1989 1990.* London: Brassey's.

Jervis, R. 1984. *The Illogic of American Nuclear Strategy.* Ithaca, NY: Cornell University Press.

Joint Chiefs of Staff 1977. *Military Posture Statement of the Joint Chiefs of Staff, FY 1978.* Washington, DC.

1982. *Military Posture Statement of the Joint Chiefs of Staff, FY83.* Washington, DC.

Kahn, H. 1960. *On Thermonuclear War.* Princeton, NJ: Princeton University Press.

and Mann, I. 1957. *Ten Common Pitfalls.* Santa Monica, Calif.: RAND.

Kaplan, F. 1983. *The Wizards of Armageddon.* New York: Simon and Schuster.

Kull, S. 1988. *Minds at War.* New York: Basic Books.

Larson, E. V., and Kent, G. A. 1994. *A New Methodology for Assessing Multilayer Missile Defense Options.* Santa Monica, Calif.: RAND.

Lifton, R. J., and Markusen, E. 1990. *The Genocidal Mentality: Nazi Holocaust and Nuclear Threat.* New York: Basic Books.

Litfin, K. T. 1994. *Ozone Discourses: Science and Politics in Global Environmental Cooperation.* New York: Columbia University Press.

LITTLE, R. and SMITH, S. (eds.) 1988. *Belief Systems and International Relations*. Oxford: Basil Blackwell.

MACKENZIE, D. 1990. *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance*. Cambridge, Mass.: MIT Press.

NASH, H. T. 1981. The bureaucratization of homicide. Pp. 149–60 in *Protest and Survive*, ed. E. P. Thompson and D. Smith. New York: Monthly Review Press.

NITZE, P. H. 1976. Assuring strategic stability in an era of détente. *Foreign Affairs*, 54: 217–18.

O'NEILL, B. 1993. Operations research in strategic warfare. Pp. 2567–73 in *International Military and Defense Encyclopedia*, ed. T. Dupuy. New York: Pergamon.

1994. Game theory models of peace and war. Pp. 995–1053 in *Handbook of Game Theory*, vol. ii, ed. R. J. Aumann and S. Hart. New York: Elsevier Science.

OFFICE OF TECHNOLOGY ASSESSMENT (OTA) 1979. *The Effects of Nuclear War*. Washington, DC: Government Printing Office.

POSTOL, T. A. 1987. Targeting. In Carter, Steinbruner, and Zraket 1987, 373–406.

QUADE, E. S. 1968*a*. Introduction. In Quade and Boucher 1968, 1–19.

1968*b*. Pitfalls and limitations. In Quade and Boucher 1968, 345–63.

and BOUCHER, W. I. 1968. *Systems Analysis and Policy Planning: Applications in Defense*. New York: Elsevier.

RATHJENS, G. W. 1969. The dynamics of the arms race. *Scientific American* (Apr.). In Russett and Blair 1978, 33–43.

RICKS, T. E. 2004. Rumsfeld gets earful from troops: complaints cite equipment woes, extended tours and pay delays. *Washington Post* (9 Dec.): A01.

ROCKEFELLER, A. 1974. Letter of 24 Apr. to Herbert F. York in reply to his letter of 18 Apr.

ROSENBERG, D. A. 1986. U.S. nuclear war planning, 1945–1960. In Ball and Richelson 1986, 35–56.

RUSSETT, B., and BLAIR, B. 1978. *Progress in Arms Control*. San Francisco: W. H. Freeman.

SALMAN, M., SULLIVAN, K. J., and EVERA, S. V. 1989. Analysis or propaganda? Measuring American strategic nuclear capability, 1969–1988. In Eden and Miller 1989, 172–263.

SAPOLSKY, H. M. 1972. *The Polaris System Development: Bureaucratic and Programmatic Success in Government*. Cambridge, Mass.: Harvard University Press.

SCHWARTZ, S. I. 1995. Four trillion dollars and counting. *Bulletin of the Atomic Scientists* (Nov.–Dec.): 32–52.

SCOURAS, J., and NISSEN, M. J. 1994. *FALCON: A Rule Based Strategic Force Allocation Model User's Guide*. Prepared for the US Arms Control and Disarmament Agency under AC94OD4112.

SECRETARY OF DEFENSE 1969. *Annual Report, FY 1969*. Washington, DC

1980. *Annual Report of the Secretary of Defense, FY 1981*. Washington, DC

SPINARDI, G. 1990. Why the U.S. Navy went for hard target counterforce in Trident II. *International Security*, 15: 147–90.

TELLER, E., and FOSTER, J. S. 1961. Some new considerations concerning the nuclear test ban. A RAND Special Report attached to a Memorandum for the President, from Col. (USAF) Godfrey T. McHugh, 7 Apr.

THEE, M. 1986. *Military Technolgy, Military Strategy and the Arms Race*. London: Croom Helm.

THOMPSON, E. P. 1981. A letter to America. *The Nation*, 232 (24 Jan.): 68–93.

WALD, M. L. 1997. U.S. atomic tests in 50's exposed millions to risk. *New York Times*, 29 July: A10.

WILKENING, D. 1994. Future U.S. and Russian nuclear forces: applying traditional analysis methods in an era of cooperation. In Davis 1994, 301–48.

Woolf, A. 2002. The nuclear posture review: overview and emerging issues. Congressional Research Service.
Yengling, A. 1997. Telephone interview with the author, 19 Aug.
York, H. H. F. 1974. Letter to Albert Rockefeller, 18 Apr.
Zuckerman, S. 1983. *Nuclear Illusion and Reality.* New York: Vintage.

CHAPTER 39

SOCIAL EXPERIMENTATION FOR PUBLIC POLICY

CAROL HIRSCHON WEISS
JOHANNA BIRCKMAYER

1. Policy Experiments

Lift the curtain and "the State" reveals itself as a little group of fallible men in Whitehall, making guesses about the future, influenced by political prejudices and partisan prejudices, and working on projections drawn from the past by a staff of economists. (Enoch Powell in Jay 1996, 297–8)

The statement, made by the British Conservative politician Enoch Powell, highlights the fact that public policy making involves not only the higher arts of principle, intellect, and persuasion, but also the play of interests and the pushing and hauling of partisans for power and control. While the centrality of interests and prejudices has received a great deal of attention in both the scholarly and popular media, it is Powell's "guesses about the future" and that "staff of economists" that concern us in this chapter.

Policy inevitably deals with an uncertain future. Even with the plethora of statistical series and policy research currently available, policy making has to be based on some degree of guesswork. Powell's economists who project past trends into the future, now supplemented by sociologists and policy scientists of several hues, shed sometimes flickering light on what the effects of policy interventions will be. It is to get closer to understanding the likely effects of a prospective policy that social experimentation was born. The idea is simple: Try out a policy on a small scale and see what happens.

Since the late 1960s, spending on trials of social policy proposals in the USA has consumed over a billion dollars (Burtless 1995). In this chapter we consider the nature of social experiments that have been conducted in the past forty years. We review the efforts of many social scientists and economists to develop systematic empirical evidence about the likely advantages and disadvantages of specific policy proposals through the conduct of social experiments. Then we examine the advantages and disadvantages of social experiments themselves and try to project the current trend line into the hazy future.

2. Definition

Social experiments are randomized field trials of a social intervention. Within that rubric, two emphases jostle for primacy (and a third emphasis tags along). Some authors define social experiments (SE) by emphasizing the "trial" in randomized field trial. For them, the hallmark is that a prospective intervention is being tried out on a small scale before it is widely adopted. Not only is it being tried out; it is being studied in its pilot version. The aim is to find out whether the intervention achieves its aims. If so, the assumption is that policy makers should adopt it on a system-wide basis. There is a sense of self-conscious intention to influence policy, and often this intention is accompanied by a sense of urgency as the policy window opens.

Other authors put the stress on randomization. It is randomization that allows experimenters to have confidence that the intervention was the *cause* of whatever changes are observed. In a randomized study, the experimenters select samples from the same population, assign one to the intervention, or "experimental" condition, and the other to a "control" condition. At the end of the period, the groups are compared. Inasmuch as they were very much the same at the start and the only thing that differed over time was exposure to the intervention, any differences at the end are due to the intervention. From a methodological point of view, randomization gives experimenters confidence in their estimates of effects.

The third focus in the definition of social experiments, now widely taken for granted, is that the trial is done in the "field." Gone is the comfortable milieu of the laboratory for studying outcomes. Rather the social scientist conducts the studies in the precincts in which the actual policy will be run. Thus we have randomized field trials.

If the emphasis on randomization is accepted as the guiding principle, then any study of desired outcomes conducted through randomization is an SE. Such a definition sweeps in large numbers of evaluations of existing programs. Many evaluations of social programs are conducted after the programs are enacted, and some of the evaluations (although not nearly as many as evaluators would like) randomize prospective participants into "experimental" and "control" groups. After a period of time, the evaluator compares the status of the two groups on the desired