SMAGORINSKY'S GFDL

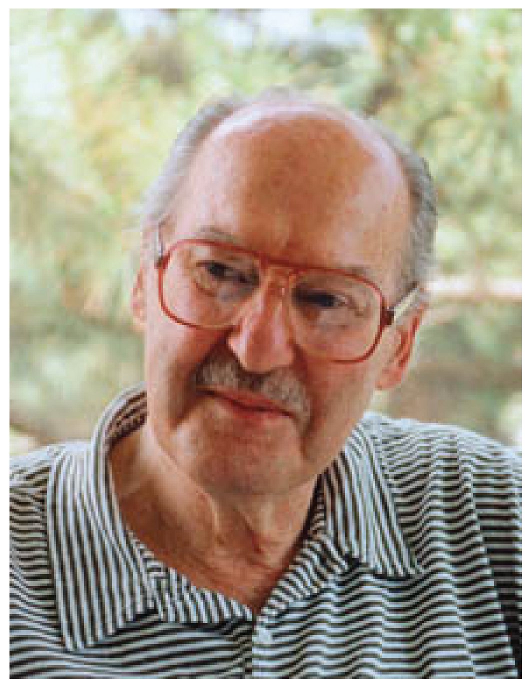
Building the Team

BY JOHN M. LEWIS

In pioneering climate prediction, Joseph Smagorinsky took an adventuresome approach, skillfully assembling a scientific group that could attack the challenges ahead.

Few would disagree that the Geophysical Fluid Dynamics Laboratory (GFDL) made great strides to extend the period of useful weather prediction and to attack key problems related to climate. Among the noteworthy accomplishments were the following:

- the development of a primitive equation model for baroclinic flow (late 1950s);
- the development of detailed radiative transfer algorithms (early 1960s);
- the first extended-range prediction experiments (4D forecasts) that impacted the planning of the Global Atmospheric Research Program (GARP) (late 1960s);
- the coupling of oceanic and atmospheric general circulation models that clarified selected effects of ocean circulation on climate (1970s); and
- prediction of climate response to predicted increases in CO_2 concentration (1980s).



Joseph Smagorinsky. (Photo: Peter Smagorinsky)

Joseph Smagorinsky (1924–2005) was the director of GFDL for nearly 30 yr (1955–83), known to many as "Smag" or "Joe Smag," sometimes referred to affectionately and sometimes with disdain. He was a forceful figure in meteorology during the last half of the twentieth century, both politically and scientifically. However, his manner and means of conducting business evoked some controversy in the larger atmospheric science community.

In this paper, we are particularly interested in Smagorinsky's philosophy of science and his associated style of management. To understand this philosophy and style, attention is given to his entry into meteorology and the scientific view he developed under the tutelage of Jule Charney and Harry Wexler. We then follow the steps that led to establishment of the General Circulation Research Section (GCRS) of the U.S. Weather Bureau (USWB; GCRS was eventually renamed the GFDL) and Smagorinsky's vision for this laboratory that included building a team of researchers and infrastructure that was germane to the success of this organization.

This research necessarily includes a scientific biographical component, intended as a complement to the biography found on Wikipedia, the Internet encyclopedia. Peter Smagorinsky, one of Joseph Smagorinsky's sons, is the principal author of this biography.

EARLY EXPERIENCES AND TEACHERS.

Entrée into meteorology. Joseph Smagorinsky's youthful passion was to become a naval architect and study at the prestigious Webb Institute in New York City, a short distance from his home. But this dream was not to be. Although he had an excellent public education at Stuyvesant High School on Manhattan's East Side, one of the renowned math/science high schools in the United States, he failed to gain entrance into the highly competitive Webb Institute. Studying naval architecture at the Massachusetts Institute of Technology (MIT) or the University of Michigan, the other schools with strong programs in naval architecture, was out of the question for financial reasons. Meteorology was Joe's second choice. In his own words, "And so when the time came for me to go to a university, my first interest was to study naval architecture, but for financial reasons I was not able to do it. So as a second choice I went into meteorology" (Smagorinsky 1971). He studied at New York University (NYU), again close to home, and recognized for its well-rounded program in both oceanography and meteorology under the direction of Athelstan Spilhaus. He entered NYU in fall 1941.

After a year and a half of undergraduate education at NYU, and with the likelihood of being drafted into military service during World War II (WWII), Smagorinsky applied for and was admitted into the Cadet Program in February 1943—a program designed to increase the number of military weather forecasters. The program had two parts for entrants without a bachelor's degree: a 6-month "B School," with preparatory work in science, math, and communication, followed by a 9-month "A School," where students were assumed to have a background equivalent to that of a student with a baccalaureate in science (Walters 1952). Joe attended Brown University for "B School" and MIT for "A School."

Throughout his instruction at MIT, he was puzzled by the absence of a solid scientific foundation for weather forecasting (Smagorinsky 1971, 1998). In this regard, he remembered a conversation with Bernhard Haurwitz, noted theoretician and instructor in the Cadet Program at MIT: "But he [Haurwitz] said, "That [Richardson's NWP experiment (Richardson 1922)] ended in a fiasco," and . . . convinced me not to try it myself. And here I was put off. I was told that it was impossible and that's the last I thought about the problem until after the war." (Smagorinsky 1971).

With graduation from the Cadet Program in June 1944, Joe was commissioned 2nd Lieutenant U.S. Army Air Corps and assigned to forecast duties at a B-29 base in Nebraska. In February 1945, he was transferred to the Air Force's Eighth Weather Region that stretched across the Atlantic Ocean from the eastern shores of Canada and the United States to the United Kingdom and the Azores. He accumulated many hours of flight over the Atlantic as a weather officer assigned to a weather reconnaissance squadron.

Wexler and Charney. After honorable discharge from the Army Air Corps in 1946, Smagorinsky returned to NYU to finish his bachelor's degree (granted in 1947) and begin work on his master's degree (granted in 1948). He continued his graduate education working toward at Ph.D., but in absentia—first while an assistant meteorologist at the USWB in Washington, D.C., and then at Princeton's Institute for Advanced Study (IAS).[1]

Smagorinsky's first civilian job in meteorology started in 1948, as an assistant to Harry Wexler, the chief, Special Scientific Services Division of the USWB. Wexler became a key figure in the planning for the Electronic Computer Project (ECP) at Princeton in the late 1940s. In the planning and organization of this celebrated project, Wexler worked closely with John von Neumann.[2] Wexler and von Neumann are pictured in Fig. 1.

Smagorinsky became involved in the excitement of work at the ECP in the early 1950s, but his precedent work at the USWB was rather mundane—tending to a "crank-letter file" and assisting Wexler on his research into the effect of solar flares on weather. In his oral history, Joe spoke disparagingly of this work,

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The abstract for this article can be found in this issue, following the table of contents.
DOI:10.1175/2008BAMS2599.1
In final form 15 February 2008

[1] Haurwitz was Smagorinsky's de jure advisor at NYU; but as will be seen, Jule Charney served as Smagorinsky's thesis advisor (de facto advisor).

[2] A detailed account of the events that led to the creation of the Electronic Computer Project is found in Smagorinsky (1983), while the project activities that led to the first numerical weather predictions are found in Platzman (1979) and Lynch (2006).

Fig. 1. (left) Harry Wexler and John von Neumann are shown standing in front of one of the panels of the Electronic Numerical Integrator and Calculator (ENIAC), Aberdeen Proving Ground, Aberdeen, MD, April 1950. The original photograph has been cropped to isolate Wexler and von Neumann. (Reproduced from the Collections, Library of Congress.)

"That cost me a year of my life . . . In retrospect, I don't think one could do any better today [1986] with models and new observations . . ." (Smagorinsky 1986). On the other hand, Joe felt his job as a crank-file correspondent paid dividends downstream. He learned to ". . . not be so nasty that you get the guy sore and have him get in touch with his congressman, but not to be so friendly that it invites a second letter" (Smagorinsky 1986).

Jule Charney entered Joseph Smagorinsky's life in 1949. His impact on Joe's career was personally monumental and recognized instantly. In January 1949, Smagorinsky attended an American Meteorological Society (AMS) national meeting in New York City.[3] Charney, leader of the ECP, gave a talk where he presented the rationale for dynamical weather prediction (Charney 1948), a justification for prediction based on the quasigeostrophic theory. As remembered by Joe, "In one day, my visions were completely transformed. Little did I know that I would be privileged to participate in a scientific revolution that, when I first made my career choice, I had mistakenly thought had already happened . . ." (Smagorinsky 1983).

A second encounter with Charney occurred at the D.C. Weather Bureau office in spring 1949. On this occasion, Arnt Eliassen accompanied Charney and they presented their work on a 1D prediction (along a latitude circle) that explored the influence of orography on the generation of quasi-stationary perturbations in the zonal flow (Charney and Eliassen 1949). This line of reasoning entranced Smagorinsky and he engaged them in discussion. As recalled by Charney,

> . . . when Eliassen and I had developed this simple linearized approach to the . . . a kind of one-dimensional prediction equation using observed motions at forty-five degrees latitude, five hundred millibars, and had some reasonable results, we gave a lecture at the Weather Bureau and it was then that one of the brighter, one . . . the person whom I recall very distinctly, who asked intelligent questions and who made an impression on us was Joe Smagorinsky. And it was . . . right after that that we invited him to the Institute for Advanced Study. (Platzman 1987)

Charney and Smagorinsky are shown in Fig. 2 (left panel) while in attendance at the Numerical Weather Prediction (NWP) meeting in Tokyo. Smagorinsky is pictured at the NWP conference banquet in the right panel of Fig. 2.

DISSERTATION. Smagorinsky left the USWB and joined the ECP in summer 1950. He would remain there for 3 yr, until spring 1953. At the ECP, he benefited from contact with Eliassen and Norman Phillips (Smagorinsky 1986). Phillips had arrived shortly after completing his Ph.D. at the University of Chicago in 1951 and became a junior colleague of Charney in their quest to advance NWP through the use of filtered baroclinic models. Smagorinsky and Phillips were close, initially meeting during the war when they served as Army Air Corps weather officers in the Azores, and then working together on assembly language programs for the filtered models (Phillips 1988). Charney encouraged Joe to pursue the Ph.D. at NYU in concert with his half-time appointment at the ECP.

Although Charney and other members of the project team were concentrating on issues related to quasigeostrophic dynamics, Charney had a more expansive view of the research frontiers in meteorology. As remembered by Smagorinsky, ". . . he [Charney] was also thinking ahead in terms of longer-term evolutions in the atmosphere, and what maintains the general circulation, and its features, in particular, the normal patterns of the atmosphere" (Smagorinsky 1986). In accord with these thoughts, Charney suggested that Smagorinsky begin a study to shed light

[3] The AMS held national meetings at various cities during the 1940s. In 1949, the winter meeting took place in New York City and the spring meeting took place in Washington, D.C.

Fig. 2. (left) Joe Smagorinsky and Jule Charney are shown standing in front of the statue of Buddha in Tokyo, Japan, while attending the First International Conference on NWP, Tokyo, Japan, November 1960. (right) Smagorinsky, dressed in a kimono, at the conference banquet. (Courtesy of George Platzman.)

on the mechanisms that lead to quasi-stationary flow patterns over the hemisphere. In particular, he suggested that Joe try to unravel the controversy over the influence of continentality and orography on these patterns—Reginald Sutcliff's work favored continentality/baroclinicity (Sutcliffe 1951), whereas orography was the mechanism studied by Charney and Eliassen (1949).

Over the next 3 yr (1951–53), Smagorinsky would attack this problem (that eventually became his dissertation) while contributing to the project as a scientific programmer. When one reads Smagorinsky's dissertation as found in the *Quarterly Journal of the Royal Meteorology Society* (Smagorinsky 1953), there are the unmistakable "fingerprints" of Charney, namely, the reliance on the equations that govern baroclinic instability (Charney 1947) coupled with approximations for the heat sources/sinks that stemmed from studies by government climatologists Helmut Landsberg and Woodrow Jacobs. Steady-state solutions to the governing equations were found under the assumption of periodicity in both latitude and longitude. The essence of the thesis is contained in Joe's succinct statement (Smagorinsky 1953, p. 347): "Since we are dealing with stationary motions it is permissible to specify the field of heating and cooling, and the problem will be to find the field of motion which is dynamically consistent with it."

The substantive results from Joe's dissertation are captured in Fig. 7 of Smagorinsky (1953) where the fields of orography and source/sinks of heating are superimposed over the stationary perturbations of sea level pressure and 500-mb heights (along a latitude circle representative of the 20°–50°N band).

In retrospect, Joe reviewed his contribution: "My final conclusion was, based on my dissertation, that the two are indeed competitive and that the continatality dominates in the lower levels, but by the time you get to the upper troposphere, the baroclinic effects poop out and the barotropic orographic effects can still be seen" (Smagorinsky 1986).

British theoretician Brian Hoskins has examined the impact of Smagorinsky's dissertation work as follows:

> Until the time of Jo's paper, the fundamental theoretical work on stationary waves was the classic paper of Charney and Eliassen (1949), and also Bolin (1950), that looked at the importance of orographic forcing. After Jo's paper a continuing topic was and still is the relative importance of topographic and asymmetric diabatic forcing in determining the stationary waves. (See e.g. Held Ch 6 in the 1983 book by myself and Bob Pearce [Held 1983]). I think I remember that Sutcliffe who stressed the baroclinic nature of the atmosphere welcomed Jo's paper at the time (was it one of the discussions recorded in the *QJ* [*Quarterly Journal of the Royal Meteorological Society*]?). (B. Hoskins 2007, personal communication)

Sutcliffe did submit a carefully crafted response to the *Quarterly Journal* that was read on 17 February 1954 (Smagorinsky 1954):

> It is most satisfactory to see quantitative dynamical methods being applied to these very difficult problems of the quasi-steady states of the general circulation and to see how heating and cooling in the westerlies may set up long waves. I think this paper is another lesson on the extreme difficulty of practical meteorological problems. The

> author hopes to throw light on the problems of long-range forecasting and I am sure he does so in a fundamental way, but we are delighted when he manages to show that something a little bit like the real atmosphere can be inferred from the physics. To go beyond this and get a first quantitative approximation to seasonal variability and then to seasonal anomalies, a small effect, and then further to prediction, is to meet difficulties of greater order at each stage. Unless we are very lucky in striking an unexpectedly useful result there is a long long way to travel from this paper to forecasting based on the same principles. But it is always necessary to begin.

Upon completion of his dissertation, Smagorinsky returned to Washington, D.C., as a USWB meteorologist. Because of his experience at the ECP, Wexler assigned him the task of evaluating computational machinery in anticipation of operational NWP at the Bureau (Smagorinsky 1971). We know that Smagorinsky delivered a paper titled "Data processing requirements for the purpose of numerical weather prediction" at the Eastern Joint Computer Conference held in Washington, D.C., in December 1953 [reviewed in Gammon (1954)]. And when the Joint Numerical Weather Prediction Unit (JNWPU; sponsored by the USWB, U.S. Navy, and U.S. Air Force) was formed in July 1954, Smagorinsky was named Chief, Computation Section.

FORMATION OF THE GCRS. Almost simultaneous with the commencement of operational weather prediction, Norman Phillips completed his numerical experiment on the general circulation (Phillips 1956; reviewed in Smagorinsky 1983 and Lewis 1998). In chapter 5 of Smagorinsky (1983), titled "The advent of the general circulation modeling era," Joe eloquently recounts the impact of this event on him and the larger scientific community. Von Neumann was so taken with the work that he developed a proposal with Harry Wexler to explore long-range prediction. This proposal, in its entirety, is found in Smagorinsky (1983). The proposal, dated 1 August 1955, was accepted as an adjunct to the JNWPU. Joe Smagorinsky was asked to head this new effort, named GCRS. He took charge of GCRS on 23 October 1955.[4]

SMAGORINSKY'S VISION FOR THE GCRS. The initial plan for the GCRS called for a staff of 13: the meteorologist-in-charge (director), 3 meteorologists, 4 computer programmers, 2 computer operators, 2 meteorological aids, and a clerk–typist. The yearly budget for GCRS was $262,000 (almost half going toward computer operations in the IBM 701, which was to be shared with the operational NWP component of the USWB). Leadership of this organization was a daunting task, especially for the 31-year-old government meteorologist who had just obtained his Ph.D. But this youthful director had the benefit of involvement in the ECP and mentorship under Charney and Wexler.

The mandate for the GCRS hinged on results from Phillips' experiment and von Neumann's interpretation of NWP's future, and, as might be expected, there was consistency in this background information. The intermediate range prediction, on the order of months or a season, was deemed to be the most difficult. In von Neumann's words [from a talk he gave in 1955, and published later in Pfeiffer (1960)],

> The approach is to try first short-range forecasts, then long-range forecasts of those properties of the circulation that can perpetuate themselves over arbitrarily long periods of time (other things being equal), and only finally to attempt to forecast for medium-long periods which are too long to treat by simple hydrodynamic theory and too short to treat by the general principles of equilibrium theory. (von Neumann 1955)

It is difficult to know exactly how Smagorinsky reacted to these ideas in the mid-1950s, but by reading his retrospective review of NWP and general circulation (GC) modeling (Smagorinsky 1983), we know the first steps he took:

> In the brief interval of our close cooperation with the IAS group [~ 1955–1956], they were responsible in getting us started on a fruitful line of research. We already were busy with the precipitation problem. In the case of general circulation modeling, it seemed the next logical step beyond Phillips' model was to allow nongeostrophic modes which could be of great significance in how the tropics operated in, and interacted with, the general circulation. (Smagorinsky 1983)

[4] The group changed its name and location several times: from GCRS (1955–59) to General Circulation Research Laboratory (GCRL, 1959–63) to GFDL (1963–present). The respective locations were Federal Office Building 4 (FOB 4), Suitland, Maryland; 15 Pennsylvania Avenue, Washington, D.C.; and both 15 Pennsylvania Avenue and Forrestal Campus of Princeton University.

It seems fair to say that Smagorinsky was not immediately going to attack the "equilibrium" problem mentioned by von Neumann; rather, he was going to step beyond the quasigeostrophic principles that were at the heart of short-range prediction, a modest but very reasonable first step.

With Charney's help, Smagorinsky had ventured into the precipitation prediction problem, and he had given conscientious thought to the inadequacy of the upper-air observation network, a concern for short-range prediction but a monumental obstacle standing in the path of progress in general circulation modeling (Platzman 1987). This issue would continue to occupy Smagorinsky's time, especially from the early 1960s through the late 1970s when the Global Weather Experiment would occupy center stage (Smagorinsky 1978; Smagorinsky and Phillips 1978).

Smagorinsky started his journey toward a "framework for understanding for the general circulation" (Smagorinsky 1983). Only a decade earlier, David Brunt held the pessimistic view that such a framework was beyond our grasp (Brunt 1944), but the pessimism was fading in the light of Phillips' work (Phillips 1956). Smagorinsky now envisioned a team of researchers that could bring their knowledge to bear on the following problem areas:

- condensation modeling (with cloud)
- radiation modeling
- dynamics of convection
- parametric internal diffusion
- mathematical properties of differencing techniques
- ocean circulation.

ASSEMBLING THE TEAM. Like Charney before him, Smagorinsky would both lead and contribute to the project, the ECP in Charney's case and the GCRS in his case (see Smagorinsky 1958, 1960). As stated earlier, construction of the basic model, a primitive equation model applicable over the globe, was the first order of business. To accomplish this challenging task, Smagorinsky would engage a team of top-notch meteorologist–programmers to work by his side. The star performer was J. Leith Holloway, a gifted logical thinker who Smagorinsky labeled the "premier, sterling" programmer (Smagorinsky 1986). Smagorinsky knew Holloway as a member of Wexler's team at the USWB, joining that team after receipt of his B.S. (1952) and M.S. (1953) degrees in meteorology at MIT. Over the next 10 yr (1955–65), Smagorinsky would hire other exceptional meteorologist–programmers—Robert Strickler, George Collins, and Dick Wetherald were among this group. They would be prominently included as coauthors on seminal papers from the GFDL.

One of Smagorinsky's management tenets was the following[5]: *A(i): Maintain a stable and well-balanced program (balanced in the sense of basic vs. applied research).*

Thus, he would seek observationalists and practical meteorologists as complements to theoreticians. Further, he would exhibit patience in forming his team, again satisfying his principles:

- *C (iii): Personnel decisions are the most important actions taken.*
- *C (iv): It is better to not fill a vacancy than to compromise quality; patience always pays off.*

Hiring Syukuru ("Suki") Manabe is a case in point:

> I had been reading some papers about Japanese scientists, came across two names that seemed to crop up time after time. One was Manabe, the other was Kikuro Miyakoda. The thing that intrigued me about Manabe's name is that it didn't so much appear on papers that he had written, but on papers that his colleagues had written where they were crediting him with some of the basic ideas . . . So I made him an offer . . . he came as a visitor . . . I couldn't offer him a permanent job, he was an alien. I needed again very special permission to hire a foreigner. It really probably had never been done, but that started a precedent. (Smagorinsky 1986)

Manabe recounts his first assignments:

> Joe already had a grand vision of modeling the atmospheric general circulation and climate using the so-called primitive equations of motion and had begun to construct such a model. Immediately, I was involved in simple parameterizations of land-surface processes and cumulus convection, and the construction of a radiative transfer algorithm. This was the beginning of a long-term research project which continues today [1997]. (S. Manabe 1997, personal communication)

The hiring of observationalist Abraham Oort is another case where Smagorinsky's patience paid off.

[5] Smagorinsky's management principles are found in the appendix. When used in the main body of the text, these principles will appear in italics with categorization identifiers.

The story as recollected by Oort:

> The first time of indirect contact was when I wrote a letter to Harry Wexler of the US Weather Bureau around 1960 to ask about possible training in Meteorology after completing my studies in Physics at the University of Leiden, The Netherlands . . . Dr Wexler wrote a nice letter back that he had consulted Joe Smagorinsky but that they had come to the conclusion that it would be much better for me to first learn the field at one of the Universities. Later on I found out how Joe treasured a good basic education. (A. Oort 2007, personal communication)

Oort then studied under Professor V. P. Starr at MIT and completed his M.S. in meteorology by 1964. Oort continues,

> [In early 1964] Suki Manabe invited me to give a seminar at the General Circulation Research Laboratory in Washington on the energetics of the lower stratosphere (my MS thesis at MIT, and later part of my PhD thesis at the University of Utrecht). Joe and Suki apparently liked my seminar, and after my return to The Netherlands I received in September of 1964 the invitation to join their group. In the beginning I was rather hesitant to accept the invitation because GFDL was known for its modelling and theoretical strength, and my interest was more in observational-diagnostic work. However, Joe and Suki stressed that they just needed a person like me who had these interests to complement the theoretical and modeling work at GFDL. (A. Oort 2007, personal communication)

Other observationalists at GFDL were Ernie Kung (atmospheric kinetic energy generation) and Gene Rasmusson (hydrologic cycle).

Doug Lilly typifies the scientist attracted to GFDL under the following principle: *C (v): Don't be afraid to go after promising young people who haven't yet made their reputation; this is much better than getting senior well-known scientists who may be over the hill.*
As recalled by Lilly,

> Joe did in fact pursue me. I initially had no knowledge of him or his lab, and at the time I was not seeking employment, not having finished my dissertation [at Florida State University]. He apparently chased me down from one of my professors, probably Seymour Hess. In fact he came down to Florida to meet me. I can't say exactly why he decided on me, but I must have made a good enough impression. I did not give a seminar, perhaps some kind of written statement . . . In retrospect I should say that Joe had an unusually strong ability to evaluate people, as shown by several examples. Joe actually brought me up to Washington and put me on the payroll [in 1959] before my dissertation was completed [Lilly 1960]. I didn't realize how unusual that was until later. (D. Lilly 2007, personal communication)

Lilly would be encouraged to investigate the dynamics of convection and boundary layer processes (see Lilly 1962).

By the late 1950s, Smagorinsky was motivated to include ocean circulation modeling in the GCRS.[6] His reasons for pursuing this path were the following: 1) the anticipated need to couple ocean and atmosphere to study climate, and 2) the transfer of technology from atmospheric general circulation to ocean circulation modeling. One can only imagine that he had difficulty convincing the conservative USWB that ocean modeling should be a primary component of the GCRS. As he stated, "HW [Harry Wexler] agreed to look the other way, [oceanography is] not exactly part of [the] WB [Weather Bureau] mission" (J. Smagorinsky 1987, personal communication). He continues, "5 years later [1965] it was one of the prime examples justifying the uniting of oceanography and meteorology in the formation of ESSA [Environmental Science Services Administration] and then in 1970, NOAA [National Oceanic and Atmospheric Administration]." A concise summary of events that led to the creation of ESSA and NOAA is found in the transcript of a recent talk given by Robert M. White, former USWB chief and administrator of ESSA and NOAA (White 2005).

In Smagorinsky's search for an oceanographer, Kozou Yoshida was his primary target. He was a prized student of Koji Hidaka, chair professor of oceanography at the University of Tokyo. Yoshida was interested in coming to the GCRS, but could commit for only a year because of his wife's ill health. Smagorinsky felt that a two-year visiting appointment was the minimum he could justify for construction of a numerical ocean model. Yoshida could not make that commitment, and so Joe began to look elsewhere.[7] He gave serious consideration to Wolfgang

[6] This information is based on material in unpublished notes (henceforth referred to as J. Smagorinsky 1987, personal communication).

[7] Yoshida would later succeed Hidaka as chair professor of oceanography at the University of Tokyo.

Krauss and Pierre Welander. In the end, he was able to attract Kirk Bryan, a postdoc at Woods Hole Oceanographic Institution: ". . . Manabe and Lilly had gone to a GFD [Geophysical Fluid Dynamics] summer program course at Woods Hole. And they met a guy by the name of Kirk Bryan . . . a student of Lorenz I think . . . And we started talking and by 1960, he showed up." (Smagorinsky 1986).[8]

In his reminiscences, Bryan said, "I was excited about working with Joe on building a numerical model for the oceans . . . and luckily for me they [Yoshida, Welander, and Krauss] turned him down" (K. Bryan 2007, personal communication). Bryan has written an informative historical review of general circulation modeling of the ocean that includes the work that he and others (especially Michael Cox) accomplished at GFDL (Bryan 2006).

The rough statistics of hiring practice at the GFDL follow:

- typically 25 scientists applied each year (including applicants for fellowships and visiting scientist appointments);
- of these 25, 5 were typically selected for temporary appointments; and
- every four years ~1 permanent appointment was made or ~1 permanent appointment for every 100 who applied.

In his notes (J. Smagorinsky 1987, personal communication), Joe looked back over his tenure as director of the GCRS/GCRL/GFDL and took pride in the low attrition rate of the Ph.D. scientists. Only five left the organization during this period: Doug Lilly, Gene Rasmussen, Ernest Kung, Wayne Sangster, and William Holland [order of names follows Smagorinsky's notes (J. Smagorinsky 1987, personal communication)]. Lilly recollects events related to his departure from GFDL:

> I'm always a little fuzzy on the exact dates, but in 61 or 62 I visited NAR, met and was impressed with Phil Thompson, and obtained a visiting appointment for an academic year. Joe encouraged me to do that on gov't money, presumably on the assumption that I would then owe it to him. I kind of missed the point, and shortly after the end of that year I let Phil and Joe know that I wanted to go to Boulder permanently. That caused a terrible ruckus with Joe, and of course he had a legitimate grievance . . . As I said, Joe was kind of a "godfather" . . . very loyal to his staff and expected great loyalty in return. The few of us who left fairly early would probably be considered somewhat more rebellious than those who stayed. (D. Lilly 2007, personal communication)

Rasmusson's departure was certainly not related to any rebelliousness. In his case, Joe exhibited magnanimity in allowing Rasmusson to leave GFDL. As recalled by Rasmusson,

> Like Bram [Abraham Oort], I was studying under Victor Starr, pursuing a PhD which would lead me to a research career. In 1964, I received word that my scholarship [granted by the USWB] was now being supported by Smag, and therefore I had become a member of GFDL temporarily assigned to MIT. After completing my PhD, I joined GFDL in DC. Bram and I worked closely until I left in 1970 [See Oort and Rasmusson (1970)]. Unlike some others, my departure in no way reflected dissatisfaction with Smag or the organization but rather was an unhappy response to some arm-twisting by NOAA headquarters [HQ]. Smag called me into his office [at GFDL/Princeton] one day and informed me that NOAA HQ wanted me to join a group being assembled in DC to analyze the newly acquired BOMEX [Barbados Oceanography and Meteorology Experiment] data. I respectfully declined the "offer." A couple of weeks later Smag informed me that he had again received the same request but this time he said, "There are times you should not say "NO." I grudgingly agreed, with the knowledge that Smag had seriously attempted to shield me from this "request". As a consequence, my relationship with Smag and the lab remained cordial and mutually supportive throughout the years of his directorship, and I will always consider myself a proud alumnus of that organization. (G. Rasmusson 2008, personal communication)

MANAGING THE TEAM. *The groups.* Viewed macroscopically, Smagorinsky insisted on teamwork across the boundaries of the major groups in the laboratory that were tasked to investigate the problems mentioned at the end of "Smagorinsky's vision for the GCRS." As recalled by Jerry Mahlman (J. Mahlman 2007, personal communication), one of the group leaders from the mid-1970s until he replaced Smagorinsky as director in 1984:

> GDFL was built on projects that were designed to solve major problems in atmospheric and oceanic science. Specifically:

[8] Bryan confirmed that his advisor at MIT was Edward Lorenz (K. Bryan 2008, personal communication).

- Suki Manabe's group focused on creating better climate models
- Kiku Miyakoda's group was designed to pioneer the mega-challenges of modeling extended-range forecasts—typically 10–40 day forecasts
- Kirk Bryan's group was focused on creating a dynamically consistent ocean model
- Yoshi Kurihara's group focused on hurricane forecasting
- Isidoro Orlanski's group worked on matching larger-scale climate models with regional mesoscale models
- My [Jerry Mahlman's] group worked on atmospheric dynamics and chemistry modeling.

The Japanese. There is little doubt that Smagorinsky had an affinity for the Japanese scientist—he attracted Manabe (as mentioned earlier), Miyakoda, and Kurihara in the early 1960s. And as we have noted, he had hopes of attracting Yoshida. The characteristic of the Japanese to place the "whole" [the country, the household ("ei"), corporate family] above the individual fit Joe's philosophy. Further, the complementary aspect of the Japanese, self-discipline and drive in the spirit of Bushido (the traditional code of the samurai), undoubtedly impressed him (see Nitobé 1914).

Kirk Bryan saw the advantages of having the Japanese contingent in the lab: "Joe did an excellent job of fostering camaraderie and team work in the lab. He never gave people tasks that pitted two scientists against each other [see principles B (iv) and E (ii) in the appendix]. In this he was greatly assisted in the fact that the core of the lab were Japanese scientists [in the 1960s] who were naturally great at working together" (K. Bryan 2007, personal communication).

And consistent with the nature of the Japanese, Joe placed the organization above the individual leaders as captured in his tenet that had the flavor of a mathematical inequality: *E (iii): The interactive whole organization should be stronger than the sum of its parts.*

Protection from bureaucracy.

C (x): Protect the time of the scientists

- *Assign a minimum of administrative tasks*
- *Shield them from demands from the outside which are unreasonable*
- *Discourage unnecessary or excessive travel.*

These adages represented Joe's desire to keep the scientists on the "research trail" without distraction. Further, he felt that he had a gift for dealing with bureaucracy, and indeed he did. As viewed by Lilly,

> Certainly he was tolerant of and interested in his scientific staff's views and directions, although he always let us know what was his view of the truth. I would assert that his most unique administrative ability was not downward, but upward. Somehow he was able to convince his senior management, who were also unusually capable, that he should have pretty free rein and a fairly generous budget . . . Joe apparently thought it best to represent us and not waste our time in bureaucratic maneuvers . . . (D. Lilly 2007, personal communication)

Abraham Oort echoed Doug's sentiment:

> Joe had a powerful presence. He could and would, if necessary, defend the laboratory and its people from outside interference. He was a sort of father figure for us all. Many of the scientists had come from foreign countries, and would not have functioned so well in a different, less protected environment . . . Joe made it clear that our main duty was to do good work at GFDL; so our traveling was limited, but enough. (A. Oort 2007, personal communication)

Monasticism. To some in the meteorological world outside GFDL, there was a perception that this lab exhibited aloofness and monasticism, a cloistered environment of sorts. Joe did not deny it. He practiced "constructive snobbery" to use his phrase (Smagorinsky 1998). In no way would he "open the gates" of GFDL, that is, he would not have it viewed as a community scientific institution as was the mandate and policy of the National Center for Atmospheric Research (NCAR). He admitted that there was some merit and advantage to NCAR's practice, but it was not for him and his lab.

The transcript of a conversation between Norman Phillips and one of his protégés, Tony Hollingsworth, points to the perception that GFDL practiced some form of insularity. The conversation is excerpted from Norman Phillips' oral history (Phillips 1989) and it follows:

> **Hollingsworth:** . . . it always seemed curious to me that GFDL at that time [early 1970s] was somehow monastic in its mode of working. It didn't seem to have a whole lot of contact with your group [at MIT] . . . with you or Charney for example. I just thought it would have been in the interests of both of you to have had a lot of contact.
> **Phillips:** I think Jule was able to get computing done at GLAS [Goddard Laboratory for Atmospheric Science] that he could not have gotten done at

Princeton because, well first of all, the computing time at Princeton was well spoken for and secondly, Joe Smagorinsky was strong enough a character that he wasn't about to submerge himself in anybody.

When GFDL moved to Princeton University in 1968, the cloistered nature of the work was ameliorated to a certain degree. As remembered by Oort,

> Later on [after we got to Princeton], the visiting scientist program and Princeton students at the Graduate Program in Atmospheric and Oceanic Science were a great asset and opening to the outside world, opening our individual ivory towers (A. Oort 2007, personal communication).

One of the students who worked with Oort was Dennis Hartmann. His recollections add substance to Oort's statement:

> Bram Oort was my advisor [beginning in 1972] and he assigned me some data sets and an FFT [Fast Fourier Transform] program and suggested I do some spectral analysis of radiosonde data. This I worked at assiduously and it resulted in my first publication [Hartmann 1974] . . . He did not ask to be a co-author, even though he set up the problem for me totally . . . Bram is a wonderful person and I can say that about many people at GFDL at that time . . . Jerry Mahlman gave up a tenured faculty position [at the Naval Postgraduate School (Mahlman 2006)] to join the lab to work on stratospheric modeling. He became a great source of advice and inspiration also, and his youthful attitude made it easy to approach him . . . my committee of Bram, Jerry, and Suki made arrangements for me to become familiar with satellite data by spending time at Bill Smith's laboratory [Development Lab of National Environmental Satellite and Data Information Service (NESDIS)] where Kit Hayden served as my tutor. I then returned to Princeton and finished my dissertation on the dynamics of stratospheric circulation in the southern hemisphere. (D. Hartmann 2007, personal communication)

The extreme selectivity that Smagorinsky exhibited in choosing his permanent staff was also apparent in his choice of visiting scientists. He was not about to export the treasured general circulation modeling code to the outside user, no matter how impressive the credentials. In Joe's words:

> And our basis for cooperation is not the same as NCAR's. We bring somebody in only if somebody [one of our group leaders] has specific interests. In Bourke's case [Bill Bourke, Australian physicist/meteorologist], it was Miyakoda. And the question is, what do you do, export the model? Well, these models are so special that you just can't place them in somebody's hands. (Smagorinsky 1998)

This practice of exclusivity and of confidentiality, especially in regard to model development knowledge and the associated computer code, came under severe criticism.[9] From Joe's viewpoint, it was justified and consistent with his tenet [$C(x)$], ". . . *shield them* [GFDL scientists] *from demands from the outside that are unreasonable*." Of course, the key word in the tenet is "unreasonable." Where is the line drawn that separates a reasonable request from an unreasonable one? Abraham Oort reflected on this issue: "I feel it was very wise of Joe to let scientists only come to GFDL as visiting scientists to use the models, where they could work closely with scientists like Suki or Kirk . . . the results were reliable and have withstood the test of time" (A. Oort 2007, personal communication).

Doug Lilly viewed it differently:

> At NCAR, we had many arguments and discussions about the privileges and prior claims of scientists to both observational and computational discoveries. I pushed for as much freedom and openness as possible, as consistent with both academic tradition and public financial support. I think there was a kind of informal agreement that data and computer models belonged to their discoverers for about a year. Joe assumed that if he let anybody use the GFDL models, they would probably either take credit themselves or screw up the programs and blame GFDL for it. (D. Lilly 2007, personal communication)

Battling NMC. The original proposal that established the GCRS implied, if not demanded, that this research wing support the JNWPU [and its heir, the Development and Operations Divisions of National Meteorological Center (NMC)]. From the very beginning there appeared to be contentiousness in this relationship. Aksel Wiin-Nielsen, a visiting scientist at NMC in the late 1950s, reviews the situation:

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[9] See Bryan (2006) for elaboration on Smagorinsky's attitude toward exporting code and confidentiality.

I think it is well-known in the meteorology circles that Joe also was one of the pioneers in numerical weather prediction, having spent time at Princeton . . . It is also in evidence that both [George] Cressman and [Fred] Shuman [leaders within NMC] were at Princeton at a later stage and learned something about modeling. Now I think that Shuman and Cressman must be characterized as the conservatives in the area of numerical weather prediction at the time . . . they stuck to the proven. Joe, on the other hand, was going to integrate the primitive equations . . . he was going to have his own computer, and he was going to put physics into things. So I used to listen to these exchanges between Fred Shuman and Joe Smagorinsky. They were not friendly; they were really after each other. Cressman was perhaps more withdrawn, a little bit more of a gentleman than the others . . . but nevertheless Aksel Wiin-Nielsen used to be sent as a diplomat down to Joe and from Joe up to Fred Shuman so we could negotiate certain things. (Wiin-Nielsen 1987)

Chuck Leith, a premier general circulation modeler who worked at Lawrence Livermore Laboratory and NCAR, discusses the contentious relationship between these groups in his oral history (Leith 1997). He said, "I liked to talk to both of them [Cressman and Smagorinsky] when I was in Washington for various reasons, but it was sort of like visiting Israel and Egypt: you had to not tell one that you were going to see the other, because there was so much animosity between them . . ." Leith conjectured that some of the hostility stemmed from competition for the same computer resources within USWB/ESSA/NOAA.

From our distant vantage point, it is reasonable to conclude that the meteorological community suffered from the inability of these organizations to cooperate. As Leith argued, the operational NWP community could have improved their "climate drift" by paying attention to results from the general circulation model, and the GFDL would have benefited from testing their cloud/radiation interactions on a day-to-day basis from the operational analyses that made use of the satellite-derived observations. Rasmusson agreed with Leith: "The long running battle between Smag (GFDL) and Fred Shuman (NMC) stood in the way of constructive cooperation between their organizations until the two protagonists retired" (G. Rasmusson 2008, personal communication).

Despite the estrangement between GFDL and NMC, GFDL connected well with operational NWP at the European Centre for Medium-Range Weather Forecasts (ECMWF) and the Australian Numerical Meteorology Research Centre (ANMRC). Wiin-Nielsen, the first director of ECMWF, recalls the events leading to the first medium-range (10 day) forecasts at ECMWF in late 1979: "We had the luxury of taking a few years to decide on our plan of attack. We didn't have to invent, rather gather. We got help from GFDL—in particular, we benefited from their physical parameterization schemes" (A. Wiin-Nielsen 1993, personal communication). Although Smagorinsky was supportive of the linkage between these two organizations, he had some doubt about ECMWF's emphasis on satellite observations. As recalled by Pierre Morel, Smagorinsky's politico-scientific colleague in matters related to the Global Weather Experiment,

I still remember distinctly an occasion when Dr. Lennart Bengtsson (then director of research at ECMWF) and myself presumed to write down our views concerning the uncertainty margin and relative statistical weight that could be attributed to in-situ measurements by radiosondes versus remote-sensing temperature profiles obtained by satellites. Like most dynamical meteorologists of his generation, Smagorinsky favored in-situ observations and told us (especially me) in no uncertain terms "to mind our satellites and leave numerical prediction to the real experts". On this particular point, Smagorinsky's judgement did not serve him well. Only a few years later [mid-1980s], ECMWF achieved significant advantage in prediction skill over both NMC and GFDL by assimilating marginally accurate but densely distributed satellite observations together with in-situ Raobs in its forecast initialization process. (P. Morel 2007, personal communication)

Smagorinsky made a Herculean effort to help Australia advance into the NWP age. GFDL hosted two long-term visits by Commonwealth Scientific and Industrial Research Organization (CSIRO) scientists to explore the feasibility of extended-range NWP in the Southern Hemisphere (B. Bourke and D. Gauntlett 2007, personal communication). The visitors were Reginald Clarke (who visited in 1965) and Doug Gauntlett (who visited in 1968). Clarke demonstrated the viability of 10-day forecasts in the Southern Hemisphere by using the GFDL model (Smagorinsky et al. 1965). Against the backdrop of Clarke's work, Doug Gauntlett was sent to the United States to assess the comparative merits of the NMC and GFDL models. He sided with the GFDL model and spent 12 months adapting the code for use in the Southern Hemisphere. The "Southern Hemisphere" version of the GFDL model was operationally implemented at ANMRC in 1972.

EPILOGUE. If Joe Smagorinsky was not aimless, he was uncertain of his path in meteorology until he heard Jule Charney set out the rationale for dynamical weather prediction at the AMS national meeting in 1949. In one day, his life changed and from that point onward he dedicated himself to a career aimed at weather prediction and climate modeling by dynamical process. Again Charney influenced him by suggesting a dissertation topic that bridged from synoptic prediction into prediction of the general circulation—a subject thought impossible a decade earlier. Albeit a "small step" in that direction as stated by Sutcliffe, the direction would not change throughout Smagorinsky's lifetime.

Through the excitement generated by Phillips' bold numerical experiment—an experiment that set the theoretical meteorological world abuzz because of its promise for extended-range forecasting—John von Neumann and Harry Wexler advocated the formation of a General Circulation Research Section (GCRS; later renamed GFDL) within the Bureau. This research effort would build on the scientific base that led to success with short-range NWP, and Joe was chosen to lead the GCRS by Bureau Chief Francis Reichelderfer and Chief of Scientific Services Harry Wexler, Smagorinsky's champion.

Joe's vision of the GCRS was adventuresome, far from "the proven" to use Wiin-Nielsen's phrase. Nevertheless, he was impressive by his deliberation, spending 4–5 yr developing a robust primitive equation model with help from excellent meteorologist programmers. By 1960 he advocated the development of an ocean circulation model, realizing that climate modeling would demand ocean–atmosphere coupling. With his vision and modeling tool in place, he began to assemble the team of researchers. His vision demanded a "balanced approach," one where observational studies would stand beside theory and numerical experiment.

He exhibited an uncanny ability to pluck the "rising star" from the research community—Manabe and Lilly were examples. He found excellent resources for his cadre of researchers through skillful and artful argumentation with agency bureaucrats—top-of-the-line computers and programming support. He was a father figure and he expected loyalty in return. There were constraints, a demand that there be teamwork and that any barriers to collaboration be demolished. It was not academia and there was a perception of monasticism by meteorologists outside GFDL. Joe exhibited a perplexing nature, full support for operational NWP efforts outside the United States (ECMWF and ANMRC), while distancing himself from NMC, the principal NWP organization in the United States. Further, he kept a tight rein on those precious modeling codes—he was not about to "export those models."

It is hard to argue against the success of GFDL over the period of Smagorinsky's tenure. Would his style of management and philosophy of science serve as a template to success in the typical scientific organization? Certain components of Joe's style and philosophy are enduring. One notes common threads in the success formula for other scientific organizations. For example, Bell Labs heralded technical excellence, independence of thought, and scrutiny by peers, as the values that led to their success (Mabon 1975). Beyond the values or rules, however, success of scientific organizations is generally linked to visionary leadership, lofty visions shared by the workforce. Joe celebrated the complexity of the atmosphere–ocean system and he was convinced that extended-range prediction of this complex system could be achieved through computer modeling and experimental design.

Joe frequently extolled GFDL's accomplishments to overflow audiences in the United States and around the world. As remembered by Morel, "We were always keen to listen to the latest findings from Jo's shop. His speeches were never dull. He always had some new breakthrough to report. It was like partaking in an exhilarating scientific adventure. Like the race to the moon, it was a discovery of uncharted capabilities created by human ingenuity" (P. Morel 2007, personal communication).

Joe never lost sight of the challenge that Reginald Sutcliffe left him: ". . . [T]here is a long way to travel from this paper [Smagorinsky (1953)] to forecasting based on the same principles. But it is always necessary to begin." Joseph Smagorinsky took that first step over 50 yr ago, and since that time he and the GFDL team have "traveled a long way" and significantly contributed to advances in extended-range forecasting.

ACKNOWLEDGMENTS. In the mid-1990s, Joe Smagorinsky wrote to me and supplied information for my study of the Japanese meteorologists who immigrated to the United States following WWII. At that time, he also sent me a nearly complete set of his scientific reprints. Upon surveying these reprints, I was inspired to examine his career more closely.

Those who took time to write letters of reminiscence or supplied information are the following, where the dates (month–year) are found in parentheses:

scientists who worked at GFDL—Kirk Bryan (6–07), Brian Gross (7–07), Dennis Hartmann (11–07), Leith Holloway (9–07) (via his brother George Holloway), Doug Lilly (6–07), Jerry Mahlman (7–07), Syukuro Manabe (4–97), Abraham Oort (10–07), Gene Rasmusson (1–08), Bill Shearn (7–07), Joe Smagorinsky (5–92 and 6–96), and Dick Wetherald (7–07); scientists outside GFDL—Bill Bourke (6–07), Fred Bushby (10–97), Doug Gauntlett (6–07), Jim Howcroft (8–07), Brian Hoskins (6–07), Akira Kasahara (10–07), Ed Kessler (8–07), George Mellor (8–07), Graham Mills (6–07), Pierre Morel (9–07), Olivier Talagrand (7–07), and Aksel Wiin-Nielsen (4–93 and 3–97).

Oral histories of Joe Smagorinsky were vital to this study. Oral histories of Chuck Leith, Jerry Mahlman, Norman Phillips, Robert White, and Aksel Wiin-Nielsen were also valuable. I commend the interviewers and Diane Rabson, archivist at UCAR, for helping me locate these oral histories.

Finally, I thank the Smagorinsky family (wife: Margaret, daughter: Teresa, sons: Fred and Peter) for supporting this historical study and for their generosity in making critically important documents available to me.

APPENDIX: SMAGORINSKY'S MANAGEMENT PRINCIPLES. On 24 April 1987, Joe Smagorinsky was invited to give a talk on the occasion of the foundation of the Institute of Naval Oceanography (INO) in Bay Saint Louis, Mississippi.

He used this occasion to give advice to the administrators of this new organization. The advice took the form of management principles, especially appropriate for scientific organizations. These principles were the product of Smagorinsky's experiences and were not associated with any particular texts on the subject. Joe had these principles typed, but he also included annotated comments. In the list that follows below, Joe's handwritten annotations are placed in brackets [ ]. Since reference is made to this set of principles in the body of the manuscript, we label the various subject headings by capital letters as follows: Program (A), Money (B), People (C), Organization (D), Personal Relationships (E), and Miscellaneous (F). Within each category, the subheadings are denoted by lower-case Roman numerals: (i), (ii), (iii), (iv), etc.[A1]

Before stating the principles, Joe wrote,

> I'd like to take a crack at indicating what I think are some worthwhile and perhaps even important principles of management. These principles you will find, do not necessarily follow conventional wisdom. But frankly, I think conventional wisdom is over-rated . . . I found that these principles require dedication, tenacity, and courage to implement. They demand keeping one's eye on distant objectives, and from time-to-time bucking superiors until such time as they understand that we are both on the same side.

A. Program
- (i) Maintain a stable and well-balanced program (including basic and applied emphasis)
- (ii) Do a few things well [That's what you'll be remembered for]
- (iii) Do only those things that you have the competence to be a leader in
- (iv) Bigger is not necessarily better; don't adopt a new initiative for the sake of novelty; more money is not necessarily the means to higher quality
- (v) Demonstrate feasibility and worth of new initiatives through bootleg resources; good management should make provision for serendipity through flexibility in programming and budget
- (vi) Management should insure continuity and commitment of program thrust; management must be careful to avoid arbitrary sharp changes in program policy
- (vii) Understand your place in the broader national and international research context
- (viii) Avoid becoming a job shop
- (ix) [Don't become complacent; it's easier to get to the top than to stay there]

B. Money
- (i) Minimize soft money; a lesser amount of hard money is almost always better in the long run
- (ii) Keep within your budget; don't be afraid to under-run
- (iii) Try to keep your budget structure as broad and flexible as possible
- (iv) Unlabeled resources should be controlled at the laboratory or institute level for a couple of reasons (to discourage divisive and destructive internal competition and to promote flexibility for unanticipated opportunities)

C. People
- (i) [There is no substitute for one very competent person]

[A1]Subject headings follow Smagorinsky while the labeling [A, (i), etc.] has been inserted by the author.

(ii) People are an organization's most precious commodity
(iii) Personnel decisions are the most important actions taken
(iv) It is better to not fill a vacancy than to compromise quality; patience always pays off
(v) Don't be afraid to go after promising young people who haven't yet made their reputation; this is much better than getting senior well-known scientists who may be over the hill
(vi) Get good people and give them their head within broad guidelines and constraints [and don't micro manage them—it's a sign of supervisor's weakness]
(vii) If you have to meddle in a project, then it's time to replace its leader or the responsible parties
(viii) Get rid of weak people; they will have no future with your organization; until they leave, place them where they can do a minimum of damage
(ix) Reward those who make effective use of resources, such as travel [funds], computer time, support staff
(x) Protect the time of scientists (assign a minimum of administrative tasks, shield them from demands from the outside which are unreasonable, and discourage unnecessary or excessive travel)
(xi) Avoid unnecessary meetings and reporting
(xii) In general, resist the temptation to misuse scientific talent
(xiii) The administrative structure [should] exist to serve the scientific staff which is responsible for the primary product of the organization
(xiv) Filter external perturbations (e.g., budget fluctuations and other demands); top management gets paid for quietly and effectively dealing with outer problems [making them transparent at the level at which the work actually gets done]

D. Organization
(i) Minimize (the size of) administrative and organizational structure
(ii) Make it easy for the organizational structure to accommodate necessary re-alignments in programs and priorities
(iii) [I find that] there is a maximum effective size of groups; beyond which office communication breaks down (5–25 scientists)
(iv) Decentralize wherever possible and economical
(v) [Lab directors usually outlive their bureaucratic bosses in their jobs]

E. Personal relationships
(i) Preserve free interaction among scientists; each scientist should be potential sounding board for the others, even though they may not be working on the same problem
(ii) Encourage constructive and friendly competition; the mode of resource management and allocation should be consciously designed not to encourage a splintering of groups into factions
(iii) The interactive whole organization should be stronger than the sum of its parts

F. Miscellaneous
(i) Minimize window dressing (e.g., crowing, unnecessary advisory committees, [over intrusive PR], etc.); good science is the most eloquent PR in the long run
(ii) Don't go crying to your supervisor on the least provocation; try to solve your own problems [and if you have to go to him, help him with suggestions]
(iii) [You never win a battle permanently, it always has to be re-fought]

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