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A Theoretical Basis for Intentional Organizational Change with Comments From a Thirty Year Perspective[1]

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ABSTRACT

Application of scientific methods to organizational behavior followed work done in the military and industry in World War II on team performance, group behavior, and organizational design. A new scientific discipline, **organizational psychology,** grew to fill the gap between psychologists studying individual behavior and sociologists and cultural anthropologists interested in large group behavior. New approaches had to be devised for applying traditional applied research designs and measurement techniques to the formation, growth, success, and failure of human-centered organizations. At about the same time, a holistic or systems science was emerging. This science, when coupled with the tools of conventional applied research, brought a fresh new perspective. During our lifetimes we have come to view organizations as living organisms ("living systems," as James Miller calls them) that grow, adapt, evolve, interact with other organizations, self-organize, self-regulate, and, based upon feedback from the environment, modify behavior to succeed or fail.

General systems and cybernetic theory provided a framework of constructs, models, and points-of-view that was helpful in organizing and formalizing thinking about intentional organizational change. Using a general systems paradigm of Organization (O) Relating (R) to Environment (E) through Time (T), this paper both discusses ways of first distinguishing and then synthesizing structure and process and articulates criteria of organization effectiveness and efficiency. It then suggests ways of synthesizing formal logical problem-solving and human problem-solving processes in the context of concrete situations faced by organizations. Such a paradigm serves to help clarify hypothesis formulation and evaluation using conventional scientific methods. However, the methodological focus on organization objectives, goals, and missions has revealed to me that individual and collective options arising from values lie at the root of organizational effectiveness and efficiency.

General systems/cybernetic theory can provide useful scientific paradigms when coupled with those of specific scientific disciplines, common sense, or intuitions, in the sense that it offers an interdisciplinary theoretical framework for analytical investigation of highly complex human-centered organizations. However, there are bounds. In any real sense, scientific investigation of human centered organizations cannot be value-free. The general systems paradigm cannot, of itself, explicate root values which arise from somewhere outside that framework.

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[1] Based upon a paper, "Management Systems," presented to the Mid-Atlantic Conference of the Society for General Systems Research, September 1973, and an article by E. J. Burns and J. H. Proctor, "Sistemi e Administrazione," appearing in *Paradigmi a Societa,* (A Systems paradigm), Franco Angeli, Editor, George Braziller, Publisher, Milano, Italy, 1978, pp. 87–101. E. John Burns, my friend and colleague, and I discuss these issues to this day.

I. Introduction

Contributors to General Systems theory/cybernetics have increasingly recognized that despite the advances and the useful insights achieved by theory, the nature and role of values remain paramount in every intellectual construct. As Sutherland (1974) states:

> . . . any information any scientist acquires, by whatever means, will owe at least some portion of its substance to non-empirical, non-inductive imagination, such that a value-free science is both logically and practically impossible (and perhaps not even desirable).

To reach a similar conclusion, this paper describes in some detail an application of general systems theory/cybernetics to organization improvement and discusses why in human social systems one always faces values that transcend and direct the construction of all formal structure-process sets one usually denotes as "system."

In my experience as an organizational psychologist consulting with clients in government and industry, a large number of problems and situations are seen more clearly when viewed from a general systems/cybernetics perspective. I am a technologist who attempts to apply that perspective whenever possible. My work, performed in a highly pragmatic context rather than a research or academic setting, has led me to certain conclusions concerning the nature of general systems theory/cybernetics as a paradigm for scientific approaches to understanding and organizing human social systems.

Chief among these findings is that human social systems at any point in their multi-year life cycle present a hierarchy of phenomena that, while inherently interactive, has a precedence order of relationship and influences among levels. John Burns and I have been asserting for over twenty years that beyond the familiar systems concept of levels within levels, there are qualitatively different levels—perhaps we should call them Levels—such that there are not only levels within levels, but Levels above or, better yet, Levels beyond Levels. This result is suggested when one poses the now familiar question: "Where do goals or objectives for management by objectives come from?"

In attempting to answer this question, we view human-centered organizations a little differently, through a conceptual lens of general systems/cybernetics theory. The result of using such a lens is to try to step beyond a description of the separate notions of structure (management systems) and process (systems management) or the semantic distinction between systems of management and the management of systems. However interesting and compelling the extensions of

these notions might be, this paper explores the interactive connection between structure and process in terms of the missions, goals, and objectives of organizations and the measures of their individual unit and total organization effectiveness and efficiency.

A General Systems Paradigm

In the broadest sense, institutions, companies, corporations, agencies, and churches are viewed as "organized complexities." (Weaver, 1948) The organized complexities are certainly systems according to Bertalanfy's (1956) definition, "sets of elements standing in interaction." They are complex in the sense of Simon (1956): "Roughly, by a complex system I mean one made up of a large number of parts that interact in a non-simple way. In such systems the whole is more than the sum of the parts . . ."

In terms of the elements themselves, two basic classes can be distinguished. First, there are those elements united for a specific purpose in a controlled and bounded arrangement, as distinguished from those elements that operate outside the purposes and the boundaries of control. These internal elements (with bounds) can be called collectively the **Organism** (O), and those supportive and constraining elements external to O can be called the **Environment** (E). I acknowledge that we are on dangerous ground when we select and classify elements, for as Hall and Fagen (1956) pointed out some time ago: "[I]t is no mean task to pick out the essential from the nonessential; that is, specification and subsequent dichotomization into system and universe is . . . a problem of fundamental complexity." Stafford Beer's "viable system" contributions directly attack this problem. His thoughts on "Divisio" (1960) are particularly relevant. The interactions, of a dialectic nature, both among the internal elements of O and between the internal and external elements, we call a **Relationship** (R). At any given moment these relationships have a present and future weighted existence which constitutes the Organism's actual and preferred states in Time (T). (Fraser, 1975)

This thinking about interactive relationships among the internal elements of O and between those elements and external elements of E coincides with an analysis by Mark Braham (1973) who, in developing a general theory of organization, has perceived that the fundamental process ". . . is cyclic, involving alternative periods of divergence and convergence."

Certainly organizations develop structural subelements with differing functions, and considerable individual and collective effort is expended attempting to orchestrate these different groups so that they will play together within time phases and over time intervals. But interaction between divergence and convergence also manifests itself in particular ways. It arises because of the practical

necessity to make the heads of organizational elements responsible for producing at maximum rather than optimum levels. This is a practical necessity wherever optimum levels are not explicitly known, which in my experience is generally the case. The interaction also appears as the conflict between change and the status quo. A particular manifestation of this dialectic (for that is what we are really dealing with) is encountered in corporate planning, especially in advanced planning, where a means is sought for performing the transition between present and future states of a company where the guardians of the organization's present state can be distinguished from the exponents of change despite the daily singing of the corporate anthem (Morris, 1974).

Improving the performance of a system or parts of that system begins with either the acceptance of the client's organization (O) as given or the articulation of O in the client's terms, specifically welding the one and the many into a dynamic whole which has both individual unit flexibility and holistic cohesion. Then the environment (E) is specified. This is usually a problem at best, but what is especially intriguing is that case in which the basic character of the company itself is the subject of close scrutiny. In these cases the basic question surfaces: What is the function or mission of the total organism? This question must be settled before the relevant bounds of E and the relationship (R) between O and E can be determined. Answering this question is by no means as straightforward as it might appear, even for consultants and clients working within highly structured societies that encourage such well-defined institutions as corporations. It becomes an extremely difficult task when dealing with governmental, educational, and other similar institutions which operate largely within self-interpreted and changing boundaries, established and justified with too little debate and fewer empirical facts and measurements.

In essence, I am employing an abstract and admittedly idealized general systems model—that of Organism (O) relating (R) to Environment (E) in time (T)—to help articulate and implement with a client an interactive divergence/convergence process which will achieve optimum rather than simply maximum states of output. The next section describes the O-R-E-T model in more detail. After using this model to approach organizational development for some 20 years, I am persuaded that despite appearances of sophistication, we are shortly led to certain hypothesized assumptions which cannot be deduced from the O-R-E-T model or its method of employment. These considerations form the basis for the remainder of the paper. They lead to the conclusions on general systems theory/cybernetics not only as a paradigm for conventional science but also as a currently conceived vehicle for assisting organizations in solving problems of efficiency and effectiveness.

The Model

In the development of the O-R-E-T model, major emphasis is placed on the relationships among and between the constituent elements. These relationships manifest themselves through the statements of goals, objectives, and missions of the organization. It is the articulation of these relationships that is at the root of this approach to management systems and systems management of person-machine combinations in human-centered organizations. The principal reason for using this approach with clients, stated in its simplest form, is that relationships among elements is the essence of system. Unless we focus upon and build upon an understanding of relationships, we can have no concept of the cohesion underlying the convergence and divergence processes of the many organization elements which are required to operate in a unified fashion. We would fall prey to maximizing the product of subsets, rather than, as far as possible, optimizing the outcomes of the whole.

Working with a variety of institutional management forms, I have observed that there are a number of subsets of attributes within O, i.e., organisms within the Organism called *o*s, which stand in a variety of relationships (*r*s) to different aspects of the Environment. These aspects describe E and are subsets of E, which is itself a set of *e*s. These *o*s within O have a variety of relationships to one another and constitute elements of environments of a second type, internal to O but external to each *o*. Stated simply, these are intra-organizational relationships. Hierarchical structures and processes, over time, could produce innumerable distinctions between internal and external environments peculiar to any of the *o*s. In other words, a particular subsystem has relationships with other subsystems of the organism as well as with those attributes of the environment that lie wholly outside of the organism itself.

Recognizing these orders of complexity and choosing to work with a whole institution as the O, one is forced to define very carefully the boundaries or constraints upon the activities of O, which involves taking an "outside-in" view of the particular corporation or agency.The first order of business is usually to assist the client managers to identify or define the system of *r*s as a set of purposes and related states to be achieved by O with reference to E. First, we define the overall R in terms of what is called the mission of O, which is O's own guideline for the type of R over time that client managers would like to have. This mission definition results from the best thinking the client can do about interactions between E and O, making assumptions about both. Since E induces constraints and even a set of values that restrict the "rationality" of O's mission, the consultant's job is to test the assumptions about E and O logically and, if

possible, empirically. Client managers are presented with decisions or trade-offs, in which choices can be made about the kind of Rs and Es desired. This is what happens when we arrive at a statement of product line, market, and raison d'être (such as maximizing return on investment). In effect, these statements have put in a general way the combination of desired E and R, and assumed much about what kinds of Rs are possible, proper, or good and what kinds of Rs are impossible, hurtful, or bad.

A pragmatic mission statement is developed in terms that facilitate its attainment. This task requires the client to construct a set of sub-missions, goals and interim objectives that permits a definition of the organization structural elements through which objectives will be attained. Both structural questions (who, where) and procedural questions (what, when, how) are answered narratively and graphically in terms of necessary primary functions. The functional network of subgoals in essence defines either the structure (though not necessarily the form) of the organism or its constraints. It does not necessarily bound the possible process alternatives. Since we are attempting to specify a goal-seeking dynamic condition over time, we speak of **goals** as configuring future desired system states and **objectives** as configuring immediately desired system states.

We are essentially constructing a model of interrelated goals and objectives, a $\Sigma(r)$ system which is stated in such a way that it defines, at all levels of aggregation, the desired r with a specific e which concurrently ensures that the rs are consistent with the overall relationship of O to E. Such a structural description is called the **functional structure.** In recent years, various organization development (OD) techniques have been developed for setting organization objectives. The methodology is generally referred to as **management by objectives** or MBO (Beck and Helman, 1972; Roeber, 1973).

Functional Analysis

Within this functional structure $\Sigma(r)$, we very carefully distinguish functions from activities. Activities are the mechanics of function performance. They derive their right to exist only with respect to the efficiency by which they allow the function to be achieved effectively. Functions are goal- or objective-defined collections of activities and the functional structure articulates the web of interrelated goals and objectives. When involved in developing procedural, organizational, and information solutions to management problems, we first devise the functional structure through **functional analysis.** All activities that are not amenable to goal-oriented articulation in a network culminating in and consis-

tent with R itself are carefully excluded so that we can devise measures of effectiveness for O and each subsystem *o*.

Such a view provides a perspective for operationally distinguishing between effectiveness and efficiency. **Effectiveness** is defined in terms of the quality and correctness of an organization's stated mission, goals, and objectives; it is a measure of their relevance. **Efficiency** is a measure of the cost per unit time of achieving an output that constitutes achievement of the mission, goal, etc. Hence, efficiency can be measured by itself without respect to effectiveness, even though such a measure is surely restrictive. Effectiveness cannot, from a dollars-and-cents viewpoint, be defined without reference to efficiency. Given limited resources, an organization cannot be effective unless efficient, but an organization can be highly efficient without being at all effective.

However, the operational problem is to define both types of measures and establish their relationship. By defining an organism O functionally in terms of *r*, we have constructed the means of defining measures of effectiveness. When measures of efficiency are developed, the two types of measures may be joined into an operative scheme for management. In my experience, the failure to approach the problem from a functional (*r*) point of view is a major cause of untold confusion in institutions of every kind because the distinctions between effectiveness and efficiency are not clear.

Further, the use of functional analysis as a means of capturing divergent and convergent interactions is fundamental to a correct employment of general systems/cybernetics models for the management of organizations. To better develop the idea, let us use an example of a function. In a large transportation company, maintenance is a complex, important, and expensive organization subsystem requiring a large staff and an equally complex organization. The transportation company has labelled major elements or subsystems as production, production control, engineering, reliability, administration, planning, etc. Each major element performs many activities. However, one of the company's overall objectives can be defined as the concurrent reduction of maintenance costs and improvement of safety and services by means of the modification and improvement of spare parts design. As such, this desired way of relating O to E is a function that induces a convergence/divergence coupling of all the above organizational elements of O. That is, the role of each major element in this function clearly differentiates certain elements of the organization from others and at the same time relates them in that function to the overall organism O. In other words, the whole point of the $\Sigma(r)$ system defined functionally is to construct a holistic description of all the functions of an organization such that each is related to the final mission of O in a clear way but they are carefully distinguished from one another. This same form of description unifies the various

activities under the functional objective, facilitating review of the necessity for this or that activity to exist, and thereby distinguishing between issues related to the efficiency of an activity and issues related to its relevance to the whole, i.e., its effectiveness.

As shown in Figure 1, from a methodological perspective, we (the consultant and client) are beginning with some configuration of people, equipment, facilities, material, and money. First, as mentioned earlier, we attempt to define the mission and to specify or bound E and R. Logically we would specify the present E (E_p) and the future E (E_f) as well as the present R (R_p) and the future R (R_f). Often these are not compatible on the first definition cycle in the functional analysis and some iteration is required.

Next we decompose the E (both E_p and E_f) into various constituent elements for which some set of relationships (*r*s) are required. These *r*s define the objectives and goals of O. We group these *r*s in a manner that we hope will facilitate a definition of the processes by which they may be performed. Such groupings vary from case to case. Their formation is largely an art form, although the O-R-E-T model provides us with certain guidelines and checks.

These groupings are examined for interrelationship: How does the achievement of one function impact the achievement of others? These interrelationships determine the network of *r*s (in terms of goals and objectives). Finally, we construct the levels of feedback and aggregation for the functions. Levels of feedback are defined in terms of how the function is evaluated and modified. Levels of aggregation specify subfunction or subgoals as objectives whose accomplishment determines the achievement of a larger, more inclusive, goal or objective.

Functional Process

The next question is how the functional structure is activated—how it is articulated in terms of process. The dynamic aspects of a function are viewed as process, a transformation from an existing *r* to a desired *r*. From this viewpoint, structure may be seen through time as stable process. In other words, if processes exhibit some critical degree of stability, they will form an observable pattern which I label **structure.** The key to identifying and examining process is the specification of the information and communication required for an O to produce an output. It is by means of information that processes are initiated, sustained, and redirected and that objectives are accomplished. It is fundamental to describe the character of processes from a mission-, goal-, or objective-oriented point of view. As Sir Peter Medawar (1973) has remarked, "[I]t was not the

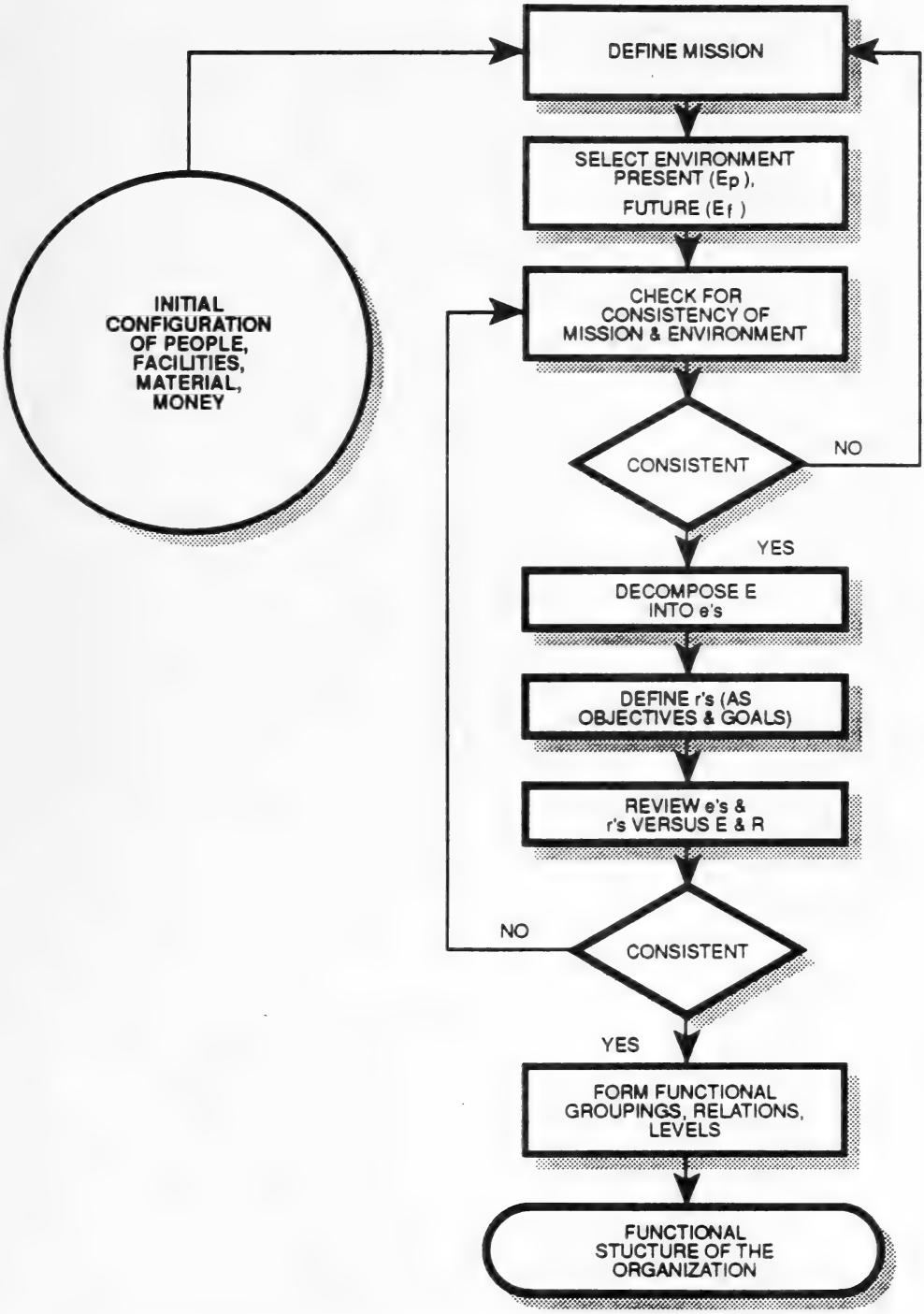

Fig. 1. Development of the functional structure of the organization through functional analysis.

devising of the wheel that was distinctly human, we may suggest, but the communication to others, particularly in the succeeding generation, of the ways to make one."

We initially present information descriptions in terms of transforms. A **transform** describes the logic of a content or meaning change in information. Then we develop a description of the information transfer. A transfer involves the movement of information-bearing symbols from one place to another. A description of the flow of information in terms of transfers and transforms, within and across functions, within and between levels of O (indeed throughout O), and between O and E through time (T) completes the functional process description. In practice, available time and money prohibit such a complete description.

We can now examine the existing elements of O and our initial configuration of resources (see Figure 1) to determine modified or new configurations of people, equipment, facilities, and money to satisfy measures of organization effectiveness and efficiency. I term these new resource combinations **enabling systems** in that they are necessary to move the O from an "as is" state in O-R-E-T to a "should be" state in O-R-E-T. Only at this point, after functional analysis and functional process description, in my judgment, can one design optimum configurations of resources and recommend to client management resource allocation alternatives in terms of numbers of people, size of budgets, amounts of inventory, and organization form and shape. This thinking appears consistent with the broader view of employing quantitative methods or operations research techniques (Saaty, 1972). One may deal concurrently with process in the organization development sense, but I am convinced that the information and procedural logic must be correctly designed as a proper setting for organization development.

Testing Assumptions and Crafting Recommendations

Herbert Simon (1956) has pointed out:

> How complex or simple a structure is depends critically upon the way in which we describe it. Most of the complex structures found in the world are enormously redundant, and we can use this redundancy to simplify their description. But to use it, to achieve the simplification, we must find the right representation.

And I have not found any single approach that invariably leads to the "right" representation. To test assumptions concerning the functional structure and process descriptions and to suggest feasible alternative enabling system designs, John Burns and I have adapted simulation and gaming techniques. With a

general systems perspective, problem identification and solution can be depicted as shown in Figure 2. As previously discussed, each function relates to an environment and the environment has been defined in a manner which permits comparison of the present relationship (r_p) with a desired relationship (r_d). It should be noted that there may be two kinds of differences, with various attendant measures of organization effectiveness and efficiency. The desired state is different from the "as is" state in kind or degree or both. In either case, the difference presents a problem and the resolution of the difference involves problem solving.

The information character of problem solving begins by developing for a particular *o*, *r*, and *e* a **problem environment** (PE). There may be different problem environments associated with one function of O or any of its constituent *o*s. For example, performing a printing plant scheduling function is different for job shop environments and self-contained shop environments. This example is fairly obvious, but in practice these differences are often overlooked, especially when the functions or environments are more abstract. Moos (1973) discusses important considerations in conceptualizing human environments. The problem solving process begins by defining the problem environment PE—that is, by selecting appropriate segments *e* of the overall environment E and defining the desired relationship, r_d that the organization (O) or an element of the organization (*o*) should have with those segments. The present relationship r_p is then compared to r_d. The comparison is first articulated in terms of the differences Δr. After the various factors causing the differences are determined, the various strategies sketched in Figure 2 are used. The solution must then be communicated to various organizational elements *o* (labeled in Figure 2 as *a*, *b*, and *c*) and implemented; this causes a new relationship r_n to exist between O and E. That relationship must be evaluated. If it is found not adequate, the solution must be appropriately modified, which may require iteration of part of the problem solving cycle.

Problem-identification or problem-solving teams of client personnel organized by function and level, interdisciplinary teams of client specialists, and games or exercises involving consultants and client managers can be used anywhere from the initial organization mission specification through the steps of assumption testing to the suggestion of alternative enabling system designs. For example, I have frequently encountered client "communication" subsystems which consist only of information transfer description. To recommend alternative ways to improve operations optimally rather than maximally in terms of communications, consultants have to work with clients to specify both information transforms and transfers. Transforms and transfers arise on both the purely logical and the interpersonal levels of communication. Transforms are the cor-

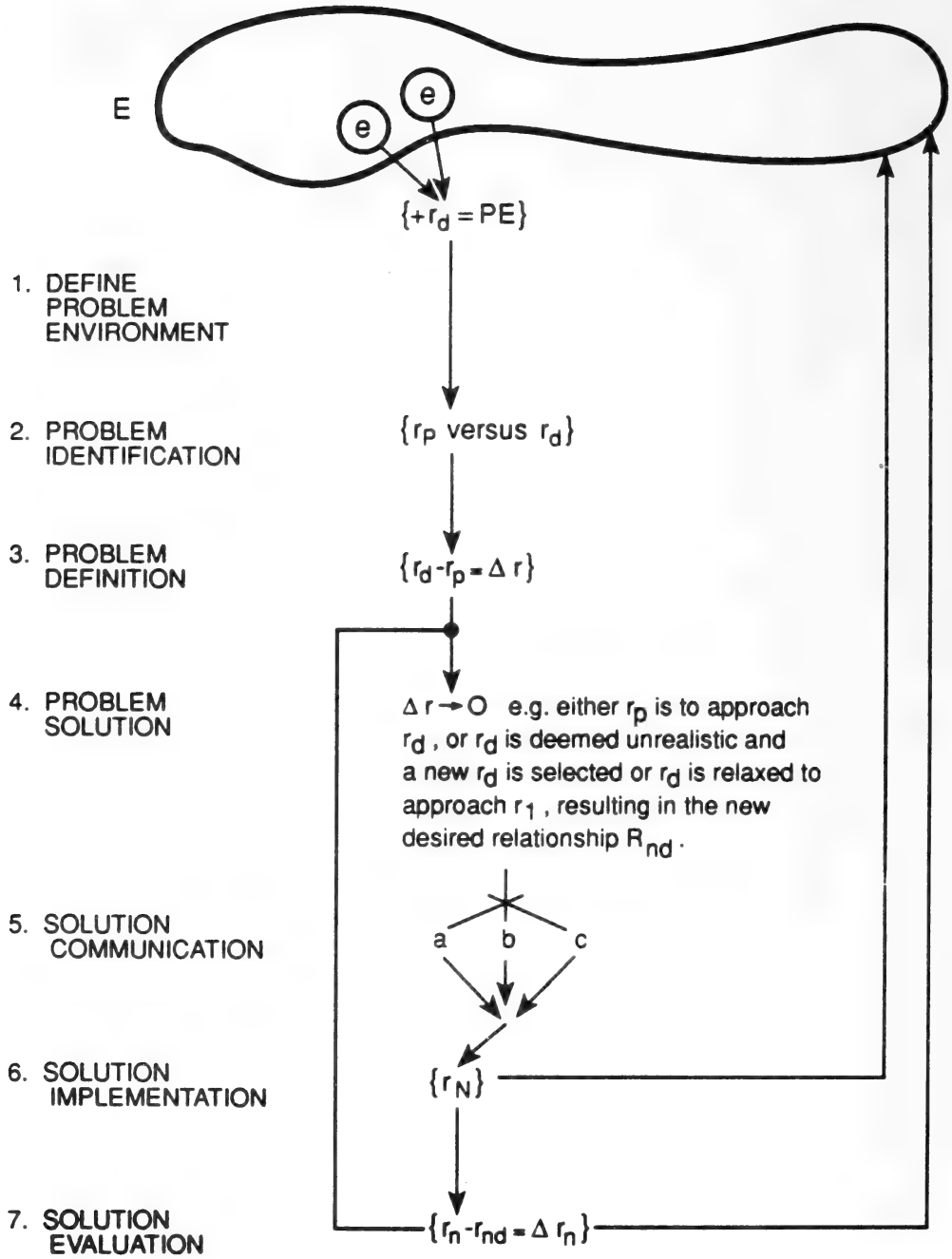

Fig. 2. Problem solving process in terms of relationship, r, analysis and modification.

nerstone—the necessary (but not sufficient) condition of ensuring effectiveness. Transfers support transforms in the same sense that activities support functions. Transfers must be measured in terms of effectiveness in the same sense—they exist not for their own sake but only to accomplish and disseminate transforms. One method I have found workable, using the O-R-E-T model to define and develop transforms and transfers, is to combine operations research (OR) and organizational development (OD) techniques in the design, staging, and development of manual or computer-assisted simulations called **controlled exercises** (Proctor, 1963). This combination of techniques takes the form of personnel team building within a context of problems of a concrete nature specifically designed in such a way that the preferred problem solution is explicit. Deviations from interim and final problem solution steps can be observed and processes can be quantitatively analyzed and evaluated.

O-R-E-T Summary

This O-R-E-T model presentation is undoubtedly highly abstract and mechanistic; it even appears to be a static model for improving organization structure and process. I fully appreciate the inadequacies of this form of presentation. In practice much more is considered, but this simplified preparation helps me set the stage for certain specific conclusions. Clients and consultants are heading for trouble by beginning to devise the enabling systems, or use the O-R-E-T model even with this simple formulation, without a basic statement of primary mission: What are we trying to do? What business are we in or would we like to be in? Without that formulation, one can't find an E or an R or even a hint of the formulation of effectiveness and efficiency criteria. Organization improvements proceed from this more fundamental goal, whose values precede rather than follow the O-R-E-T model because those values must be articulated before one can select a specific E from the universe of potential Es. I have come to believe that these values, which many appear to assume are predictable, "given talented people using modern techniques," are by no means certain (Beer, 1979 and 1981). The questions "What business are we in?" or "Who are we?" are discussed and debated, resolved or left unanswered because of values, or what more modestly could be termed **options.** That is, what managers perceive and desire causes them to select options which precede and determine the directions taken by their systems. One could argue that these options ("opts") are in turn constrained by larger systems, but a general system approach suggests that these larger systems are themselves functions of perceptions and desires which promote **super options** ("super opts"). So it seems reasonable to assume that under-

lying and directing all formal or technological systems are apparently a set of prior values or options.

Opts and Super Opts

Human social organizations have a "structure of mutual expectations" (Vickers, 1957), the articulation of which has been discussed here in terms of setting goals and objectives. However, there are no guarantees that these humans, acting themselves as goal-directed systems within higher order systems, necessarily share a set of such expectations at the same or different levels. Nor is it sure, as this discourse may have led the reader to believe, that they interact only as formal information-processing problem-solving elements. As a practitioner using operations research and organization development techniques, I have observed that interactions often have a thrust of their own, independent of the institution and of their given problem-solving role within that institution. In other words, from a goal-changing point of view, we are confronted not only with various types of interpersonal interactions, but also with the possibility of self-defined and non-mutually-supportive interpersonal interactions. Concurrence of purposes, as idiosyncratically conceived at various levels of aggregation, is not guaranteed. It is not a question of "logic" so much as a question of premises.

The articulation of these premises is crucial for those of us engaged in the design, operation, combination, and renewal of organization structures and processes. Our assumptions about why "they" behave as they do, individually or in the aggregate, are critical. The predictability of either collective or individual performance on which we all count to feed, fuel, heal, worship, and govern is profoundly impacted by the phenomenon of collective (**convergent**) and conflicting (**divergent**) options.

We are faced, it appears, with a system of values or collection of options which produces various priority orderings of organization objectives—a system gaining increasing attention within and across scientific disciplines (Laszlo, 1973; Vickers, 1973). At the highest decision making level in any organization, the top manager, acting either as an individual or on behalf of the owners or power possessors, chooses an option (Lundberg, 1968), i.e., "opts" for a specific relationship between O and an E. The overall selection of goal or purposes may or may not coincide with the prevailing climate of social values at some level of aggregation. A dynamic and changing relationship between institutional goals and social values may begin.

One might argue that the environment produces constraints that drive the selection of the desired ends. In a cosmic sense, over an eternity, this may be absolutely true. In a relative moment, however, the decision maker has a consid-

erable apparent range of choice, within definite constraints. An individual within an organization may not opt for the success of some portion of the organization, even his own. The impact of choice which defines O, R, and E becomes generally greater as we move up what might be called the "power scale" toward some point which may be termed the culture itself (Boulding, 1956; Ackoff and Emery, 1972). At that level we encounter super opts. It is important to recognize their pervasive character.

Serge Moscovici (1972) has addressed the importance of these options for any methodology of social understanding and has devised a provocative set of examples of the radical nature of social values for research in social psychology. Studies in group dynamics are made from the viewpoint of work efficiency and increased productivity, not job satisfaction. Studies of change have been undertaken whose aim has been to reduce the resistance of one group to the goals of another (labor and management respectively in the example discussed). Strategies of conflict resolution are based on the value of clashes between nation-states, not on ideologies or even material interests such as food. (This is a political question which assumes the values of social units.) Some economists see rate of return on investment as the fundamental value, while others select full employment.

In the species of goal-seeking human social interrelationships called management systems, the general system/cybernetics approach has led me to adopt a stance compatible with that expressed by J. Dennis Nolan (1974), who said, in speaking of the functional analysis of behavior, "Since functional analysis is neutral with respect to the desirability of behavior, it necessarily cannot specify the alternatives ('for the products of behavior')". Analyzing B. F. Skinner's approach, Nolan concluded that, rather than creating a value-free science, it represents a technological strategy that begs the question of values.

In short, when we consider intentional organizational change in goal-seeking human social systems, we are confronted with the questions "Who am I?" and "Where am I going?" on a level which transcends the construction of our technological systems model. And dealing on higher levels of aggregation does not provide the answers. For whatever the O, the questions remain, and furthermore exhibit themselves as a layer upon the Os.

Conclusions

During three decades, I have found that general systems theory and cybernetics provides a conceptual framework or point of view which is most helpful in organizing and formalizing intentional efforts to change organizations (Proctor, 1985). There are ways of first distinguishing and then synthesizing process and structure as well as dealing with related questions of effectiveness and efficiency.

In addition, there are ways of synthesizing formal logical problem-solving and human problem-solving processes in the context of concrete problem-solving situations faced by the organizations with which we deal.

As a paradigm for a scientific, operational approach to consulting with organizations, general systems/cybernetics continues to clarify hypothesis formulation and evaluation capabilities. However, the necessary methodological focus on objectives, goals, and missions has led me to believe that individual and collective options arising from values lie at the root of these organisms. "While management is a discipline—that is, an organized body of knowledge and as such applicable everywhere—it is also 'culture.' It is not value free science." (Drucker, 1974; Rifkin, 1987)

These values are real. To fail to articulate both the structure and process of interacting values presages failure for consultant and client. To study them in a laboratory or controlled experiment is important. But, if utility (or "get results") is the name of the game, these laboratory experiments are transferable to the external world only to the extent to which they are empirically "correct" at their chosen level of human system aggregation. Geoffrey Vickers points out that "when we set out to shape our institutions—even to form a company—we are not creating order out of chaos. We are intervening in a dynamic situation already regulated by its own laws." (Vickers, 1957)

It seems to be exceedingly difficult to devise useful predictive models of these kinds of human-centered organizations without at a minimum explicating the ends and values involved. At least this is true for prediction of organizational effectiveness and efficiency and for classification of structure and processes as well as growth and decay. Controlled experiments involving the effects of choice are only valid if the existing world system operates in conformity with the controls placed by the experimenter. This is exactly the question which arose in management by objectives: Whose objectives?

The conclusion, then, is that general systems/cybernetics can provide a useful scientific paradigm with conventional scientific methods in the sense that it proposes an interdisciplinary theoretical framework for analytical investigation of highly complex human-centered organisms. However, there are bounds. In the end, scientific investigation of human social organizations cannot be value-free. The values of any theory or experiment must be stated and only in this sense is a dialectic possible between traditional science and general systems theory, between theory and practice, between the laboratory and the practical world (Caws, 1968).

Even using general systems theory, transferring models, research, or experiments from the laboratory to the real world is risky at best, and the subject requires extensive open debate. Models, computer-assisted or otherwise, which

cannot frame or account for the multiplicity of and convergence/divergence of values and their enormously complex interactions may be misapplied.

Scientists, philosophers, and practitioners constantly must take responsibility for articulating their own values, their conception of the ends and objectives of the social organisms which they perceive, theorize, or build models about and experiments upon, especially when they may possess perceptions which may be capable of directly influencing institutional values.

It would be nice for all, from any vantage point, if various values were mutually agreed upon so that acts achieved goals satisfying all. If not satisfying all, they would be judged to be at least minimally injurious, in some qualitative sense. Many legal, political, or moral "controls" are instituted toward that end, but the devisers and appliers of these controls are operating from a point of view. The question of whose objectives, whose values, and what values transcend what values becomes very real to consultants, theorists, managers, and modelers working for or with companies and government agencies, or other aggregates of human social organisms.

Practical improvement of certain organizations at certain times is possible. However, there are no guarantees of sustained future improvements and no guarantees that formal models can predict or formulate the nature of social organisms. In fact, scientific methodologies in this realm, to be of any lasting consequence, must articulate values and take a stand upon attendant matters of underlying ethical choice. If this is done, the world, even if never ideal, may see substantive improvement. People sense this. Hopefully, scientists and philosophers do too.

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Can Hydro-Reservoirs in Tropical Moist Forest Become Environmentally Sustainable?

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ABSTRACT

Today's polarization of society "for" and "against" big hydroprojects relates to environmental costs, particularly borne by vulnerable ethnic minorities and the poor; such costs include species extinctions and tropical deforestation. This counter-productive polarization can be reconciled by transparency of planning, pluralism involving the society and especially all affected people, and by engendering national consensus on the best project. Detailed criteria for consensus are discussed. These include promotion of energy efficiency and conservation, ranking of alternatives to the next hydro project, and environmental ranking of potential sites. Environmentally well designed hydro can be preferable to alternatives (coal, nuclear), and most environmental costs can be prevented, thus making hydro renewable and sustainable.

To observers such as ourselves, society in an increasing number of tropical forest-owning countries seems to have become polarized into two

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extremes—supporters and opponents of big hydroprojects.[2] The media inform us about opponents brandishing machetes at confrontations with power engineers in the Amazon, thousands of demonstrators opposing dams in several countries, hundreds of thousands of signatures on petitions to the United Nations received by the Secretary-General, and even an international celebrity, Baba Amte, starving himself to death on the banks of the Narmada river in India. One government is alleged to have fallen partly because of a hydroproject proposed for a valuable southern rainforest, and several projects slated for tropical forest areas have been canceled or indefinitely postponed partially or entirely on environmental grounds.[3]

This tropical forest dam controversy transcends helping the power utilities win consensus and defuse polarization. We are concerned with global sustainability.[4] One of the most effective ways to achieve sustainability is to accelerate the transition to renewable energy. Hydroprojects that have lower impacts and higher benefits than alternatives may play a substantial part. We postpone to another occasion the debate on large versus small projects. We are certain that the world cannot afford business as usual. This is doubly true for big dams: there is not enough capital available at affordable cost to meet projected demand for power (Imran & Barnes, 1990; Moore & Smith, 1990; World Bank, 1989). The political polarization for and against hydropower seems most extreme in countries with tropical forests. This is understandable because tropical forests are often associated with major untouched rivers and are also the world's richest source of biodiversity. Such countries exist in Latin America, Africa, and Asia. So this is very much a global debate; it is not restricted to one or two countries. Both poles could be perceived as adopting extreme positions and unwilling to explore any middle ground. The most promising approach to reconciliation is to build on the progress in hydroproject design (Table 1), broadening the constitu-

[2] The most comprehensive documentations of this polarization are Goldsmith and Hildyard's (1985–1991) three-volume opus, and Williams (1991).

[3] Recent costly dam fights, mainly over environmental issues, include the following: India's 240-MW Silent Valley hydroproject in Kerala's remnant rain forest was cancelled in 1980. Thailand 1986 Nam Choan: 2000 MW was lost after feasibility stage. Thailand 1991 Pak Mun: the dam was relocated and its height lowered; delayed but now proceeding. Brazil 1988 Babaquara: 6000 MW was lost, due to campaign by rock singer Sting. India Narmada: 5 year delay after feasibility, new investigation (1991–2) awaited. Australia's 180-MW Franklin River in Tasmania's World Heritage Rain Forest was shelved in 1983. (Commissao Pro-Indio, 1989; Margulis, 1990; Paiva, 1977, 1982; Rosa, 1989; Rosa et al., 1988; Santos & de Andrade, 1988).

[4] Sustainability as a concept has been formally endorsed as an official priority of the United Nations system, and by the World Bank. Although the Bank knows more about the concept than it is comfortable admitting, it is difficult to operationalize the concept in all work. As they depend on the hydrological cycle, hydroprojects are theoretically renewable indefinitely. Sustainability here refers to two levels. First, the environmental and social costs—often not fully internalized—must be valued and clearly outweighed by the benefits. Second, the life of the project must not be damaged by environmental abuse, such as rapid sedimentation due to lack of watershed management upstream. Daly and Cobb (1989) have thought through the concept of sustainability the furthest. Other references include Adams (1990), Goodland et al. (1991), and Goodland & Daly (1991, 1992).

Table 1.—Broadening the Design Constituency of Hydroprojects

Design Team	Approximate Era
1. Engineers	Pre-WWII Dams
2. Engineers + Economists	Post-WWII Dams
3. Engineers + Economists + EIS[5]	Late 1970s
4. Engineers + Economists + Environmentalists	Late 1980s
5. Engineers + Economists + Environmentalists + Affected People	Early 1990s
6. Engineers + Economists + Environmentalists + Affected People + NGOs (Non-Governmental Organizations)	Late 1990s
7. Engineers + Economists + Environmentalists + Affected People + NGOs + National Consensus	Early 2000s ?

Note: These dates hold more for industrial nations than for developing ones, although meaningful consultations with affected people or their advocates and local NGOs, and the involvement of environmentalists in project design are now mandatory for all World Bank-assisted projects.

ency. The aim is to promote a national debate to ascertain whether there are criteria under which some acceptable and sustainable reservoirs could be developed in tropical forest regions.

We believe that both the transparency in decision-making and the pluralism necessary for success in such a debate will themselves significantly contribute to consensus-building. The whole process, including access to consolidated budgets, must be transparent so that the identities of the recipients of subsidies will be known to all. To achieve pluralism, academia, NGOs, the private sector, and the government must be included. This requires a certain amount of decentralization, especially of mitigatory measures. Full participation, especially of affected people and their advocates, also is essential. This brings responsibility: all groups must be held accountable for objective performance standards. In addition, environmental standards for development projects are improving. Therefore, because a reservoir may take twenty years from investigation to completion, today's best practice is the minimum acceptable standard. Let us assume that national criteria can be agreed upon and that they can be substantially met. Are there conditions under which such a reservoir could be justified? Our opinion is yes. Many hydroprojects are fraught with impossible environmental problems, but for others, such problems can be solved—although with much more effort than is expended today. On the other hand, hydroprojects with large reservoirs also may have major side benefits, such as flood control, improved water quality, and fisheries. Under certain conditions, recreation, tourism, irrigation, and navigation can be made compatible with hydropower. Even without the added benefits of hydropower, the environmental problems of coal (e.g.,

[5] EIS: Environmental Impact Statement, which was started when the design was complete—a recipe for confrontation and waste.

Table 2.—Environmental Ranking of New Energy Sources (Simplified)

	BEST
1. SOLAR (+ hydrogen)	
2. PHOTOVOLTAICS	
3. WIND	RENEWABLE & SUSTAINABLE
4. TIDAL & WAVES	
5. BIOMASS (+ alcohol)	
6. HYDRO	POTENTIALLY SUSTAINABLE
7. GEOTHERMAL	
8. GAS	
9. OIL	NON-RENEWABLE & UNSUSTAINABLE
10. COAL	
11. NUCLEAR	
	WORST

Note: As energy efficiency and energy conservation are becoming recognized as supply options in tropical nations, they would top this ranking.

carbon dioxide, greenhouse effect) and of nuclear power[6] are much less solvable (Table 2). Now that economic development is being decoupled from energy consumption, damping of electricity demand need not constrain economic development for many developing countries. On the contrary, the less investment needed in energy, the more becomes available for job creation elsewhere.

What might such national criteria be? Producers, consumers, and government should compile their own lists and their own ideas on the criteria they judge necessary. The present list is suggestive only. The criteria-setting process must be widely transparent in order to engender national consensus. *The purpose of this paper is to present the case that tropical forest reservoirs meeting such national criteria could be made environmentally acceptable.*

Assume that a hypothetical nation, some of which is tropical forest, has reached national consensus that more energy is needed. Agreed-on criteria have been met. Efficiency and conservation have been substantially achieved. Brownouts, load shedding, and rationing are unavoidable in the near future. The choice is among coal, nuclear, and hydro. All gas and oil has been exploited or is not economic. The scope for interconnections with neighboring countries

[6] The nuclear industry has spent about 75% of total R & D budgets over the last four decades, but even now only generates 3% of global commercial energy. Rather than earning a profit after all these subsidies, the industry has abandoned nuclear plants, causing $10 billion in losses for shareholders in the US alone. As 10 000 to 20 000 new nuclear plants, or one new plant every three or four days, would be needed over the next 40 years to replace coal, the use of nuclear power is highly inadvisable. The enormous number of victims of the 1986 Chernobyl accident, a number likely to exceed 4 million, will postpone any recrudescence of nuclear projects. If even the skilled and disciplined Japanese can be crippled by the "very serious" 9 February 1991 accident in Mihama, the possibilities in 10 000–20 000 new plants are not reassuring. If the problem of radioactive waste storage is solved, and, in addition, if "inherently" safe designs are achieved, then prospects would improve.

has been exploited or is not feasible. (This is important because interconnections enable a more acceptable site in a neighboring country to be taken up before a worse site in the country in question. Cooperation between Uganda and Kenya is a case in point.)

The crucial national question urgently needing resolution is: *Is there a set of criteria that could justify reservoirs in tropical forest regions?* Although this would apply to tropical forest dams in general, important country-specific criteria also would be necessary. Thus trade-offs are being faced in many countries, such as those between massive increases in coal-burning on the one hand and constructing dams—such as the world's biggest dam, Three Gorges in China, and the Narmada dams in India—on the other. Global common property issues such as carbon dioxide accumulation and biodiversity conservation should not be compromised by country-specific criteria.

Sector Criteria

For the purposes of this discussion, we assume that the following conditions prevail: The price of electricity must substantially have already reached long-run marginal cost. Practically all consumers must have been metered; meters are substantially precise; major arrears cause prompt cessation of service. The financial stability of the power utility has to be ensured.

Most energy conservation and efficiency measures must substantially be in place, both in generation and transmission, as well as inside homes and factories (Shepard, 1991). That is, the marginal economic cost (including environmental externalities) of saving an additional kilowatt-hour through conservation must be as high as the marginal cost of a new one produced and delivered to the consumer. This follows from the assumption above. Because conservation measures are always advancing, implementation will always lag behind savings potential. The goal is to minimize this lag. Also, conservation cannot reap results overnight because of restraints on the pace of replacing capital stock, plus other factors. A long-term, least-cost energy services perspective is needed. And "least" cost here must fully include environmental and social costs borne in the future (inter-generational costs). Because markets are not perfectly efficient, decoupling of profits from sales is in the utilities' interest, so that they can make money on margin and not on volume. Progressive utilities have started selling conservation to consumers. Utilities should be rewarded for efficiency. (Anandalingam, 1987; Chandler, 1985; Fickett et al., 1990; Flavin, 1985, 1986; Flavin & Durning, 1988; Gamba et al., 1986; Goldemburg, 1988; Guzman, 1987; Hagler, Bailley Co., 1987; Holdren, 1987; IEA, 1987, 1988; Januzzi et al., 1991; Johnson, 1989; Lovins, 1990; Moreira, 1989; Naviglio, 1990; Noguiera, 1991; Procel, 1990; Van Domelen, 1988; World Bank, 1989a,b.)

Discounts encouraging overconsumption by large consumers must have been repealed. However, the companies need not have been penalized to the extent that they start to generate their own power, with possibly worse impacts. Large consumers must have shifted to less electricity-intensive methods where economically feasible. (Aluminum smelting will always be energy intensive.) National energy efficiency equipment standards must be in place. Cogeneration potential must have been rationally exploited.

All economically perverse subsidies and other incentives must have been rescinded. For example, some electricity and fuel pricing policies mandate that electricity and gasoline/diesel prices be the same at the power plant, refinery, port, or capital city as they are at the farthest frontier outpost. Such policies promote excessive consumption of fuel, distort industrial, population, and agricultural-siting policies, raise prices in the main load centers, and discourage efficient energy production in remote areas.

All rehabilitation and expansion of existing sites must already have been accomplished. This is almost always achieved at much less environmental and economic cost than construction of new sites. The large number of hydroprojects completed in the 1950s and 1960s can be modernized to postpone the need for new projects. Owen's Falls, for example, only turbines 50% of the available water.

Dam Criteria

As outlined above for sector criteria, we assume all reservoir sites outside tropical forests either already have been developed or are not socially, environmentally or economically acceptable. The following six criteria are proposed (ordering does not imply priority).

First, *the proposed dam should have a high ratio of power production to area inundated.* Some hydroprojects have no reservoirs. On a ratio of kilowatts per hectare,[7] the reservoirs with the highest ratios, in the many hundreds, include Pehuenche, Guavio and Paulo Afonso: all exceed 100.[8] The lowest-ratio reservoirs include Brokopondo, Balbina, Sobradinho, Samuel, Babaquara, and Curua-Una, all under 5. Babaquara's low ratio contributed to its cancellation. A few, such as Suriname's Brokopondo and Burkina Faso's Kompienga, have ratios less than 1. One admittedly arbitrary criterion or cutoff point could be 30,

[7] This is a serviceable but arbitrary ratio. Both GWh and Kwh/ha would be better indicators.

[8] This paper focuses on hydro-reservoirs, which are increasingly common in tropical moist forest, rather than on irrigation reservoirs, which do not occur in tropical moist forest; some multipurpose reservoirs and those in tropical dry forest remnants (e.g., India's Narmada) are mentioned. Irrigation reservoirs may be slated for the tropical dry forest remnants; these would be even more problematic than those in tropical wet forest.

as in Tucurui (Table 3). Clearly, this depends on an expanded cost-benefit analysis in each case. If the ecosystem to be flooded is intact primary tropical forest, the ratio should be set much higher, say 100; if the ecosystem is agricultural or degraded land, then the ratio should be set lower. Economists are struggling to assign prices to intangibles, irreversibles, and intergenerational equity, such as those involved in the extinction of species.

Second, *the proposed site and surroundings should have had a thorough biotic inventory, and there should be no centers of species endemism, rich biodiversity, or other special features.* The ecosystem of the proposed site should be very well conserved in perpetuity nearby, as a compensatory area ecologically equivalent to or better than the flooded area. The biotic salvage will be effective.[9]

Third, *the reservoir water retention time should be brief*—days or weeks, rather than many months (i.e., a rapid circulation rate). The shorter the retention time, the less time there will be for anaerobic conditions to be created, and the better will be the water quality in the impounded area, as well as downstream for all uses.[10] The nearer to "run-of-river" the project is, the fewer will be the environmental problems. The tradeoff here will be between valuable inter-seasonal and over-year regulation, which can be less necessary in seasonless rain forest areas.

Less regulatory capacity means fewer benefits from flood control. This essentially means two types of sites are especially valuable. The first type is a canyon in which the reservoir does not rise above the top; these do not need large flows. Harnessing waterfalls that fish never ascend prevents migratory fish problems. The second type is no-head in-stream axial turbines which do not flood any forest. The best sites have such low volumes of biomass that decay will neither contribute significantly to greenhouse gases, nor impair fish and water quality, nor waste valuable biomass, nor clog turbine intakes. Removal of economically extractable biomass decreases greenhouse gas production and water quality risks. By inventing submersible chainsaws, Eletronorte (the federal power agency for the Amazon region of Brazil) utilizes already inundated trees in their Tucurui reservoir (Cadman, 1991). Brief retention time has to be balanced with storage needed for irrigation and navigation.

Fourth, *there should be no vulnerable ethnic minorities living in or using the general area of the proposed site.* No other settlements should be affected, unless

[9] Live rescue for release into biotically impoverished habitat or into zoos and arboreta has rarely been effective historically, although captive breeding and reintroduction merits invigoration. More cataloguing and preservation of seeds and dead specimens also is urgently needed.

[10] Decaying tropical forest generates massive volumes of greenhouse gases, especially methane, which is 32 times more damaging than carbon dioxide. Large, shallow reservoirs from which forest is not removed may generate vastly more greenhouse gas than a coal-fired thermal equivalent (Gupta & Pachauri, 1990).

the livelihood of the deported after resettlement is guaranteed to be better than it was before, as measured by systematic socio-economic surveys. Higher firm-Gwh/family-displaced ratio projects should have preference. But more significant is the subsequent improvement in livelihood. This means the proposed site has been thoroughly assessed by sociological and anthropological professionals, well before any decisions have been made. Direct internalization of costs needed for adequate resettlement may be acceptable for normal deportees. But for vulnerable ethnic minorities, experience shows that it has not yet been possible to achieve adequate resettlement. If roads, employee housing, and construction materials are available near the proposed site, that will reduce the impact further.

Sometimes it is not obvious who the beneficiaries are. For example, in tropical forest reservoirs used for export aluminum smelting, the beneficiaries are the industrial countries' aluminum consumers. The question then arises: For whom are the tropical hydroproject owners deciding? Similarly, in James Bay, the Cree claim the land flooded; the Quebec and Canadian governments also have claims. Is North America the beneficiary? These questions highlight the need to address explicitly the tradeoff between the beneficiaries and the people bearing the costs.

Fifth, *there should be no water-related diseases, such as malaria, Japanese 'B' encephalitis, or schistosomiasis anywhere in the general region.* Nor may they be likely to arrive. The risk of their arrival is reduced by destruction of the nearest foci. If water-related diseases are present, they must be eradicated, preferably before the impoundment creates more habitat. If this is impossible, the diseases should be controlled to the best extent feasible and a public health component should be integrated into project design.

Sixth, *the proposed dam should be sited above undammed tributaries,* to help minimize changes in flood regime (on which wetlands depend) and to provide alternative upriver sites for migratory fish. There is much uncertainty even in the relatively very simple, depauperate Northern fish biological systems and in their behavior related to impoundments. Certainly much more effort than at present is needed to increase the benefits and opportunities from fisheries.

Also, the dam should be proposed for an already dammed river. From the environmental point of view, dams should be concentrated on already dammed rivers, rather than siting one or a few dams on a larger number of rivers. Thus, a representative sample of the nation's rivers would remain in their natural, free-flowing state. This tradeoff with the risk of low flows curtailing power output should not be common to the extent tropical wet forested catchments are not usually seasonal. In multi-purpose dams, the enormous value of the annual flood restoring productivity downstream should be factored in.

Table 3.—Hydropower Generated per Hectare Inundated (Examples only)

Project (Country)	Final Rated Capacity (MW)	Normal Area of Reservoir (ha)	Kilowatts per Hectare
Paulo Afonso (Brazil) I–IV	3984	1600	2490
Pehuenche (Chile)	500	400	1250
Guavio (Colombia)	1600	1500	1067
Rio Grande II (Colombia)	324	1100	295
Itaipu (Brazil and Paraguay)	12600	135000	93
Aguamilpa (Mexico)	960	12000	80
Sayanskaya (USSR)	6400	80000	80
Churchill Falls (Canada)	5225	66500	79
Grand Coulee (USA)	2025	32400	63
Urra I (Colombia)	340	6200	55
Jupia (Brazil)	1400	33300	42
Sao Simao (Brazil)	2680	66000	41
Tucurui (Brazil)	7600	243000	31
Ilha Solteira (Brazil)	3200	120000	27
Guri (Venezuela)	6000	328000	18
Paredao (Brazil)	40	2300	17
Urra II (Colombia)	860	54000	16
Cabora Bassa (Mozambique)	4000	380000	14
Three Gorges (PRC)	13000	110000	12
Furnas (Brazil)	1216	144000	8
Aswan High Dam (Egypt)	2100	40000	5
Curua-Una (Brazil)	40	8600	5
Samuel (Brazil)	217	57900	4
Tres Maria (Brazil)	400	105200	4
Kariba (Zimbabwe/Zambia)	1500	510000	3
Petit-Saut (French Guiana)	87	31000	2.8
Sobradinho (Brazil)	1050	421400	2
Balbina (Brazil)	250	236000	1
Babaquara (Brazil)	6600	600000	1
Akosombo (Ghana)	833	848200	0.9
Kompienga (Burkina Faso)	14	20000	0.7
Brokopondo (Suriname)	30	150000	0.2

Note: This table is only partially indicative, since it does not reflect the value of the land inundated, which can vary significantly. Some of the land inundated is river bed. The more reliable ratio kwh/ha (instead of kw/ha) is being calculated. The ranking would be improved, but little altered, if river bed or normal annual flood areas were subtracted. Islands in the reservoir also could be subtracted in certain cases. Some of these figures are for non-forest reservoirs and most are hydropower, rather than irrigation reservoirs. Area inundated is the key issue. Less seasonal tropical wet forest reservoirs do not need to be large. Optimizing the tradeoffs at the margin of reservoir capacity is more important than choosing between having or not having a reservoir.

It is relatively easy to include bottom sluices at the design stage for such releases. Dams should not cause species extinctions, including those of migratory fish that would be denied access to breeding or feeding sites. This means the damming of the last few free-flowing rivers in a region will be even more difficult to justify.

In tropical forest areas, roads built to facilitate hydroproject construction or operation can "open up" significant areas to colonization and deforestation. Therefore, care must be taken during road planning and operation to reduce this effect.

Criteria on Small-Scale and Unconventional Power Potentials

Small-scale and unconventional potential alternatives are being examined or are already substantially exploited. Privately owned renewable energy generators sell surplus to utilities. Such alternatives include:

a) No-dam (or very low head) axial tube turbines within the river.
b) Small generating systems (including water wheels).
c) Solar power elsewhere in the country (including photovoltaics, tidal, wind and hydrogen from splitting water molecules).[11]
d) Biomass energy production (biomass plantations, garbage and sewage).

Many tropical forest countries contain dry, sunny, or even desertic regions where solar powered electric plants can be sited. They occupy 5% to 10% of the land of even the "best" (i.e., high head / low area) hydroschemes, and often can put otherwise unproductive land to sustainable use. We believe, and the World Bank is in process of calculating, that solar power is already economic in comparisons with hydro when the value of inundated forest is internalized, even imperfectly.

Population Stability Criteria

Human population stability is an essential precondition for all sustainable use of renewable resources, including use both of hydropower and of tropical forests. Human populations of tropical moist forest-owning countries annually increase by more than 2.4%, which means a doubling in 25 years. Sustainability criteria will be difficult enough to fulfill without having to double the electricity supply every 25 years. The situation is more severe in those countries in which the per capita electricity use also is rising. Average planned power demand growth is about 7% in developing countries—a doubling every ten years. (In Brazil per capita use is projected to rise 55% by 2000).

It is sensible to permit electricity companies to profit from their customers' investments in conservation. Utilities should not be penalized for investments in conservation. An increasing number of Northern utilities now find it more economic, rather than to generate more electricity, to provide free fluorescent light fixtures and to promote or even to subsidize other more efficient appliances for consumers. This suggests, as we noted above, that the pricing policy is wrong in these cases. Although this requires sensible action on pricing in the power market which does not yet exist in most developing countries, the preference is

[11] Hydrogen from splitting water molecules is likely to become economically and technically feasible very soon (Ogden & Williams, 1989). While it is difficult to generate 500MW from garbage and sewage now, a large number of smaller such plants reduces the need for large projects.

clear. Utilities now conduct free energy audits for consumers, showing where energy can be conserved most.

To the extent this holds for population, power corporations' support of governmental family planning goals will reduce the national controversies and project delays commonly experienced. Power corporations already help to the extent that televisions are contraceptive. We do not want to burden the power sector with righting all societal ills. However, population stability is so important for sustainability that family planning or similar activities should be components of all relevant projects, including those in the power sector.

Case Example of Brazil

The above suggestions are generic rather than specific to any particular country. However, Brazil, Eletrobras (its federal power agency), and Eletronorte in particular are deeply concerned with both energy conservation and environmental impacts. So is the citizenry—if not more so (Eletrobras, 1986, 1987, 1990; Goldemburg, 1987; Holtz, 1989; Juras, 1990, 1991; Lacerda, 1990; Serra, 1991; Zatz, 1990). Brazil has probably saved more than US $1 000 000 000 in new generation capacity avoided because of recent major improvements in the electricity tariff structure, which led to more conservation and efficiency (Geller, 1986, 1988, 1990). The World Bank has commended Brazil for moving towards a more appropriate tariff structure and in the direction of the difficult goal of raising the price of electricity towards the long-run marginal cost of production. The World Bank values the partnership with Eletrobras and has assisted in financing the federal electricity conservation program, under the direction of the Science and Technology Secretariat. The World Bank also is glad to be partners with the National Environmental Secretariat in the first and biggest loan solely for national environmental priorities and institutional strengthening (US $117 000 000 in February 1990).

The government, Eletrobras, Eletronorte, and environmentalists are adopting a new position on new Amazonian hydroprojects as a result of evolution of environmental awareness, specific legislation, and experience with Amazonian issues (Adam, 1988; Eletrobras, 1986, 1987, 1989, 1990; Goldemberg & Barbosa, 1989). Current construction rankings suggest that environmental criteria are most effective when applied proactively. This emphasis on environment, conservation, and efficiency is exceptionally well placed. The recent hiring of substantial numbers of environmental professional staff by all Eletrobras' concessionaires is encouraging. For example, Eletronorte's environmental staff rose from less than one at the time Tucurui was designed in the late 1970's to over 100 today (Goodland, 1978, 1990, 1991). Environmental training throughout the entire power sector has increased dramatically.

Capital availability is a major constraint on the power sector, which has been responsible for as much as 25% (US $30 000 000 000, 1973–83; now about 19%) of Brazil's foreign debt. Eletrobras may require of the order of US $7 500 000 000 to meet its 1991–2000 demand projections. In today's era of severely limited capital, such huge public investments in any sector, such as power supply, could force reductions in investments in other sectors, especially environment and the social sectors—education, nutrition, and health—as well as in poverty alleviation. Thus electricity, formerly a driving force behind social and economic development, could instead hinder vital welfare gains if improved pricing, conservation, and environmental precautions are not achieved. We could be entering an era in which power investments reduce investments in other sectors whose growth was the driving force underlying electricity demand projections.

Electricity rationing started during the 1985–86 Northeast drought and is projected to increase in the mid-1990s. Eletrobras projects that electricity demand will double between 1988 and 2000. This means that 37 000 MW needs to be installed by 2000. How can we best install the equivalent of three new Itaipus —the world's largest hydroproject—in this decade? How can we avoid repeating delays, confrontations, and wastage?

Reports are guardedly encouraging. One of the next Amazonian dams may be the 1328-MW Serra Quebrada project just upstream from Tucurui. This meets many of the criteria listed above, and contrasts starkly with the Balbina/Babaquara-type (Cummings, 1991; Dwyer, 1990; Fearnside, 1989, 1990; Gribel, 1990; Hecht and Cockburn, 1990; Moreira, 1987; Sao Paulo Energia, 1988; Visao, 1985). According to Eletronorte, there are no Amerindian settlements and little involuntary resettlement. The reservoir is small and practically run-of-river, and has a high ratio (31.5) of kW/ha of land flooded, which is slightly better than Brazil's biggest hydroproject, Tucurui. In addition, it is on the already dammed Tocantins river, rather than being the first on a hitherto undammed Amazonian river.

This presages well for the ranking of the next Amazonian dams potentially identified by Eletrobras' *Plano 2010* for the next twenty years. The range between the best and worst hydro sites is so wide that the least cost (after conservation) power investment program will include a full array of sources, such as gas, and imported power. Coal and (possibly at some time in the future) even nuclear (with best technology) may be found by Brazil to be better than the worst potential hydro site on future ranking on national criteria. A mixed hydro-thermal system implies fewer reservoirs.

Eletronorte has a massive challenge. Recent developments (PROCEL, cancellation of Babaquara, criteria of Serra Quebrada) suggest promising improve-

ments. National consensus on the kind of criteria suggested above will ensure that the trend is strongly positive. The World Bank wants to support this trend to the fullest extent possible.

Case Example of India

The case of India differs substantially from that of Brazil in the sense that India has not invested a substantial share of its power sector resources in hydroelectric plants. The main reasons for this are: first, that India has 148 600 000 000 tons of non-coking coal, and second, that development strategies have relied only to a very small extent on foreign borrowings. Even though the Indian economy has generally recorded a savings rate of over 20%, resource mobilization in the power sector remains severely constrained.

This has happened for three main reasons. First, power sector demand growth in recent years has been rather high (9–10% annually), with a growing peak demand relative to base load demand. Second, the electric utility industry has accumulated heavy losses on account of suppressed tariffs and operational inefficiencies. Third, high human population growth (1.8% annually) continues to impose onerous demands on investments in education, health care, welfare programs, and infrastructure. The power sector is thus one of many sectors competing for limited resources.

These factors have resulted in a preference for relatively short gestation thermal power plants rather than hydroelectric capacity. While hydro and thermal had almost the same share of power generation (45% and 55%, respectively) in 1965–66, the distribution is now 30% hydro to 70% thermal. Fortunately, Indian coal is low in sulfur, even though its ash content exceeds 40% in some power stations. As a result, the main environmental problems of thermal power stations are particulate emissions and ash disposal. Except in regions like the Rihand reservoir—now well known for the Singrauli thermal power plants—acid rain is not now a problem, nor is it likely to become much of one in the future.

India's main hydrosites are in the Himalayas, with a large share concentrated in the North-East. India has a land to population ratio of 0.004 km^2 per capita while Brazil's is 0.070 km^2 per capita. High population densities, land scarcity (particularly agricultural), and disappearing forests are three crucial factors in Indian hydro planning. For example, the major issue in the 1200-MW (US $1 130 000 000) Sardar Sarovar hydro and irrigation project on the Narmada river is the involuntary resettlement of people. These 90,000 deportees are not well equipped to adapt to new habitats, having a historically long intergenerational dependence on the land and its specific biota. In addition, the track record

Table 4.—Indian Energy Tariffs

Energy Source	Economic Price	Market Price
Domestic Cooking (Rs./Mcal)		
Kerosene	7.78	5.63
Electricity	20.43	8.49
Irrigation (Rs./ML of water)		
HS Diesel	109.64	142.02
Electricity	107.19	9.03

[Note: Rs. = Indian Rupees; HS Diesel = High Speed Diesel; Mcal = megacalorie; Pachauri 1991 pers. comm.]

of Indian involuntary resettlement is poor, so that hydroprojects are likely to run into heavy public resistance (Goodland, 1985, 1989; Pachauri, 1990a,b,c).

Capital constraints in the Indian economy are intensifying. Therefore the impact on the power sector is likely to become more serious in coming years. Typically, the power sector has accounted for less than 20% of planned public sector investments, but the targets for the current (eighth) five-year plan demand a higher share. Therefore, energy efficiency improvements become more urgent. Conservation is important here not only at the end-use level, but also in the energy supply industry itself. For instance, official transmission and distribution losses have risen to 22%, and are as high as 40% in some states. Similarly, coal thermal efficiencies are well below state-of-the-art levels, with some plants attaining only 20%. There are, therefore, tremendous opportunities for efficiency improvements in the power industry which would moderate new capacity growth without sacrificing electricity supply.

Irrationally low energy tariffs, far below long-run marginal costs, are the main reason for lack of energy conservation. This is particularly true in the power sector wherein some end-user subsidies are extremely high. (See Table 4.) Efficiency improvements must begin with adjustment of energy tariffs, in order to provide the consumer with appropriate signals. Improved efficiency may not significantly reduce available aggregate demand for power to the extent that overall quantity of power also constrains. Investments in physical capital have to be matched with investments in human capital, especially in power sector planning and environmental assessment. Such human capital investments would have larger returns than almost any other form of investment in the power sector.

Conclusion

Our conclusion devolves on the likelihood of a country fulfilling most of the above criteria. These criteria are stringent even for industrial countries. To what

extent will such criteria be fulfilled? We agree with skeptics who rightly claim not all these criteria will be fully met. But the process of agreeing and approaching the criteria will be salutary. Do sites fulfilling most criteria exist? The least bad site certainly exists. The decisions will be difficult in some cases, less so in others. Although difficult, this course is better than the alternatives, and much easier than damping demand until solar/hydrogen energy becomes feasible in the next decades. Mandatory rationing and other service interruptions are likely to be exceedingly painful to consumers and to the development of the country. The damping pain should be thought through and discussed with all interested parties, as part of the criteria-setting and consensus-building exercise. Proper pricing makes the choices more obvious. In sum, we need to compare costs and benefits much more rigorously and comprehensively than has been the case so far.

In our imperfect world, the reality is that not all these criteria will ever be totally met. Therefore, a national consensus is needed on whether or not conservation, efficiency, environmental precautions, and other alternatives are being pursued adequately. The national consensus is essential in order to agree on the threshold at which the second best—or least bad—site should be developed. National agreement on criteria will reveal where the thresholds lie. As soon as the various environmental impacts can be evaluated, the polarization of society will be defused. The need is to make uncertainty transparent and positive, rather than covert and manipulative.

As hydropower is exceptionally capital intensive and capital availability is a major constraint in nearly all nations, tropically forested or not, it is imperative to follow the least-cost (as defined above to include social and environmental costs) sequence of development. Of course, least cost specifically includes saving kilowatt-hours, not just generating them, based on consumer choice when facing appropriate prices. We urge the use of proper opportunity cost of capital. Arguments for simultaneous development of higher cost alternatives should be rigorously resisted. Power corporations are commendably making the transition away from sole focus on new capacity and towards conservation and efficiency. This is difficult for them because new capacity is under their almost total control, whereas conservation means they have to persuade other sectors outside their control.

Power corporations wanting to promote sustainability and to reduce national controversies and delays should follow a vigorous action program with serial steps along the following lines (Goodland, 1988, 1990a,b,c):

1. Promote fulfillment of agreed-on criteria.
2. Manage demand to the fullest extent justifiable.
3. Promote agreed-on valuation of impacts.

4. Seek sites fulfilling nationally-agreed-on criteria.
5. Sequence all sites in a national least-cost power program, under credible scenarios.
6. Rank all potential sites on the basis of these criteria.
7. Only then, develop the least bad new site fulfilling such criteria.

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Ten Reasons Why Northern Income Growth is Not the Solution to Southern Poverty

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"Those who make $200 a year should not pay so that those who make $10,000 a year can breathe clean air. . . . We are all in the same planetary boat. A few of us travel first class, while most are in steerage. But if the boat sinks we all drown together."

Ambassador Edward Kufuor, Chairman, Group of 77,
at the UN Assembly, 1991.

ABSTRACT

Decreasing Southern poverty is arguably today's main goal of economic development. The two main views on how this can be achieved are not fully compatible. The traditional view, is that rich Northern high-consumption societies should consume yet more in order to help the South by providing larger markets. This paper outlines the alternative view: that the North should stabilize its resource consumption, and reduce its damage to global life-support systems. Any higher consumption must come from productivity improvements, rather from increased throughput growth. If natural resources were infinite, then growth would be unreservedly good. Since resources are finite, then more Northern growth inevitably means less room for Southern growth. Productivity improvements must replace throughput growth as the path of progress for the North, and eventually for the South as well.

Divergent Views on How to Reduce Southern Poverty

Decreasing Southern poverty is arguably today's main goal of economic development. The two predominant views on how this can be achieved are not fully compatible. The traditional view, held by most economists and development agencies, is not working well. The traditional view is that rich Northern high-

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consumption societies should consume yet more in order to help the South by providing larger markets.

For example, the Bretton Woods Institutions were in part created because of macroeconomic market failure, both to maintain full employment and to bridge the income gap between rich and poor, and to intermediate between rich countries and poor. Their leadership role, apart from lending,[2] is as purveyors of ideas, as well as of capital, setting the development agenda. Regarding capital, the volume of concessionary lending declines relative to hard loans, while poverty increases.[3] Net transfers from South to North[4] show that the current system is not working as well as it should.[5] Regarding ideas, there is profound confusion, which development agencies could help greatly to clarify. Development agencies are not primarily responsible for this situation, but they did not use as much of their considerable potential influence to change the conditions that contributed.

Net transfers from South to North persist because of mature high-interest debt servicing, in spite of higher real interest rates in developing countries (averaging 17% during the 1980's) (Human Development Report of the UN (HDR), 1992), compared with the 4% rates in OECD countries. To avoid negative transfers, more loans are needed just to cover debt service, thereby increasing total debt. The projects the debt supported were not as productive as expected; therefore, growth in debtor countries in the aggregate was less than expected. Consequently this repayment transfer is not made from a larger income made possible by the productivity of the projects financed by the debt. Of course, not all projects were disappointing, but on average, development prescriptions have not worked as well as calculated. This suggests that traditional prescriptions of how the North should help the South merit overhaul.

The alternative view is that the North should stabilize its resource consump-

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[2] One need is to calculate what fraction of the $55 000 000 000 total annual ODA supports sustainable investments and to increase this expectedly small fraction (disaggregating the large armaments fraction). Similarly for the possibly $100 000 000 000 per year from philanthropic grants. El Serafy's sustainability method (1991) and environmental accounting (Ahmad, El Serafy, & Lutz 1989) should be widely used to unmask liquidation of natural capital assets.

[3] The 1990 World Development Report "Poverty" calculated that more than one billion people, about one-third of the total population in the developing countries, live below the poverty line, and that poverty is also increasing in relative terms (World Bank, 1990).

[4] For example, 1989's South to North financial flows largely debt servicing and loan repayment approximated $50 000 000 000, or $150 000 000 000 by lost trade, excluding brain drain costs.

[5] The reasons why forty years of North-to-South capital transfers have not been as successful as planned are: a) improper allocation of capital, including government expenditures; b) flawed governmental policies that promoted misallocation, inefficient industries, and urban affluent elites at the expense of the rural sector; c) large and corrupt bureacracy and military; d) neglect of peasant agriculture; e) social systems that doom three-fourths of the population, especially women, to an unproductive and stagnating existence, especially failure to disseminate effective family planning. We are grateful to Professor Raymond Mikesell for this clarification.

tion and reduce its damage to global life-support systems. Any higher consumption must come from productivity improvements, rather from increased throughput growth, a quantitative increase in size by the accretive of materials. If natural resources[6] were infinite, then growth would be unreservedly good. Since resources are finite, then more Northern growth inevitably means less room for Southern growth.[7] Productivity improvements must replace throughput growth as the path of progress for the North, and eventually for the South as well.

The purpose of this somewhat polemical paper is to contrast the two views, and to argue the case for more attention to the alternative view. The case merits a monograph, rather than a dozen pages. In being brief, we have not been able to deepen all the arguments. Our aim is to raise the alternative view higher on the agenda so that monographs by others will be commissioned, and that the controversy over current doctrines and confusions is reconciled.

Traditional View

The North must grow faster to buy ever more resources from the South; otherwise the South will stagnate. Northern income growth translates into more Northern consumerism. Northern foreign exchange paying for imports from the South will indirectly trickle down from the Southern elites to alleviate poverty.[8] UNDP's 1992 Human Development Report outlines the historic discrediting of the trickle-down theory. The South is supposed to be almost totally dependent on the North and incapable of transforming its own resources into necessities for its own people. It must export natural resources, whose world market prices have, in general, steadily declined over the last few decades. The increased flow of natural resources supports Northern consumerism. These exports are for foreign exchange used partly to import the latest consumer goods for its own elites, who are not content with locally produced basic wage goods. If the economy were unbounded by a finite ecosystem then this strategy would be possible and could be defended at least as the lesser evil. Although "trickle down" may not be thought the best means of achieving development, this view is widely held, and is espoused by development agencies and orthodox economists.

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[6] Resources include the environment as a **source** of raw materials, healthy air etc., and as a **sink** for wastes, such as carbon dioxide.

[7] For a discussion of environmental finitude and of sustainability in general, see Daly and Cobb (1989), Daly (1991), Goodland, Daly and El Serafy (1991), and Goodland and Daly (1992). The two views are contrasted best by Korten (1991), most pithily by Brooks (1991). For the most recent support for the alternative view, see Krabbe and Heijman (1992). Adams (1991) and George (1990) highlight the weaknesses of international development.

[8] For example, according to World Bank Vice President and Chief Economist Lawrence Summers (1991) ". . . rising tides do raise all boats." Rising Northern tides, however, imply ebbing Southern tides.

Alternative View

The North should stabilize its rate of consumption of resources to free them for the South, and to free up ecological space as well. The North has to reduce its overuse of global commons. Environmental sink capacity, and to a lesser extent environmental source capacity[9] (Meadows et al., 1974), has been preempted by the North, thus denying as much room for the South. The North can continue to develop, but must cease increasing throughput growth. If the expanding global economy is bounded by a finite inexpandible ecosystem (Figure 1), then this view becomes the realistic one, and the traditional view becomes impossible.

Foreign exchange generated by economic development, both from loans and exports, serves the desires of the rich more than the needs of the poor. Developing countries should be more capable of producing necessities for their own people than of producing luxuries for their rich. The foreign exchange is needed more for the latter than the former. This minority alternative view is held by Economics Nobelists Jan Tinbergen and Trygve Haavelmo, along with many, if not most, of the members of the International Society for Ecological Economics. Tinbergen and Hueting (1991) hold that ". . . continuing (with the) prevailing growth path is blocking (global) chances for survival. . . ." Hueting (1990) says, ". . . What the world needs *least* is an increase in national income," and ". . . the highest priority is to (halt) any further production growth in rich countries . . ."

Haavelmo and Hansen (1991) characterize the two views: "Policies for more equality invariably start off with the statement that the standard of the poor should be raised towards the level of the rich. In other words, lifting the bottom rather than lowering the top." The alternative view suggests adding "lowering or at least transforming the top", ie: reducing Northern throughput growth and decreasing Northern consumerism. Under current dependency arrangements, a sharp Northern recession would hurt the South while benefiting the global environment. We advocate loosening such dependency to help prevent damage to the South.[10]

Discussion

The traditional view and the alternative view cannot both be right. The alternative view leads us to emphasize the following overlapping elements, which

[9] Environmental limits to growth can be separated into **source** limits, such as depletion of petroleum, copper, etc., and **sink** limits, such as greenhouse effect, ozone shield damage, pollution, etc.

[10] We could add a more palatable modification of the second view, an attack on today's main environmental threat, namely pollution, by means of effluent charges, standards, etc. This would then be digested efficiently by the market and one indirect result would likely be a reduction in GDP and throughput. However, this falls into the obscurity of Brundtlandism: that the world needs a "5- to 10-fold increase in growth, but of a different kind." While we would support such a frontal attack, we prefer to be crystal clear and opt for a transition away from throughput growth and towards a stable or declining throughput per unit of final product, and for a stable or declining population.

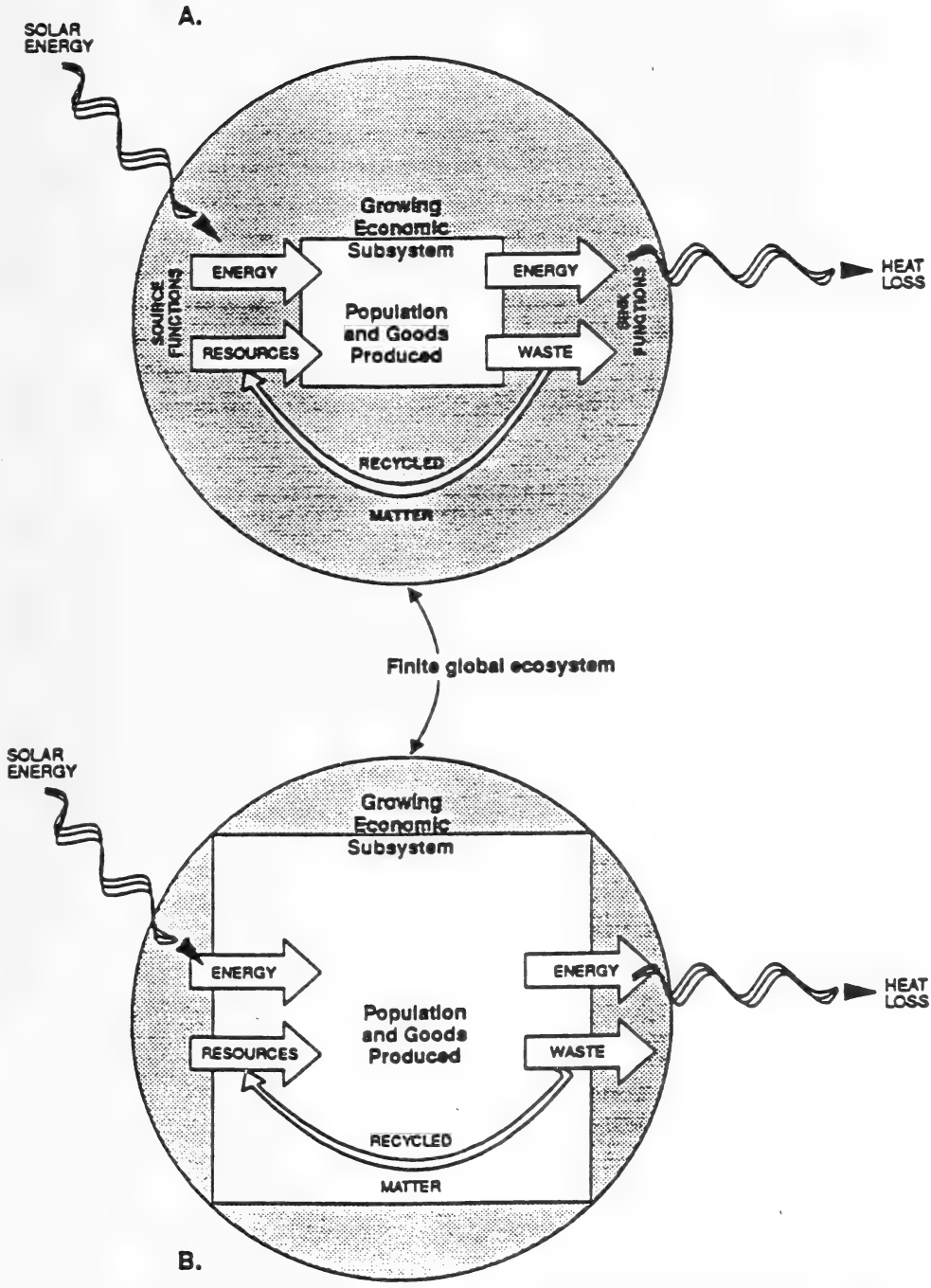

Fig. 1. The finite global ecosystem relative to the growing economic subsystem. The upper figure suggests the idyllic and long vanished era in which the economic subsystem was small relative to the then largely empty global ecosystem. The lower figure reflects today's situation in which the economic subsystem is large relative to the global ecosystem. It is now basically full and is being stressed by the scale of the economic subsystem.

together constitute our "ten reasons" why Northern growth is not the answer to Southern poverty.

1. GNP: A Flawed Measure of Human Well-Being

GNP as conventionally measured can be a misleading guide in two ways. First, GNP has little to do with human welfare, as well demonstrated by UNDP's 1992 Human Development Report. Second, economic sectors contributing most to GNP are those that are the most environmentally damaging (see below). Although GNP-maximization is unreliable for both prudent economic development, as well as for prudent environmental management, economic development takes GNP-maximization seriously as a general goal or yardstick. This should not condemn economic activity properly directed to pollution abatement, conservation, and reducing waste.

Recent work on environmental accounting by the Bank (Ahmad et al., 1989), Hueting (1990, 1992) and others shows that environmentally benign activities usually contribute a much smaller part to national income than do environmentally malign ones. On one hand, conventionally measured environmental damage and its rectification are "good" for GNP-boosting growth: for example, the Valdez oil spill clean-up boosted GNP. On the other hand, environmentally benign activities tend to be less costly than environmentally malign growth, consequently contribute less to GNP.[11] For example, walking, biking and mass transit contribute less to GNP than does automobile use; train contributes less than airplane; an extra blanket or sweater less than raising the thermostat; one-child families less than six-child families; eating legumes less than eating beef; recycling less than trashing. Reduction of GNP resulting from choice of these benign activities should be encouraged, not resisted.

Therefore, environmental protection is not, as commonly portrayed, an expensive choice, largely to be chosen when a nation becomes rich enough to afford such choices. The opposite is true. At the same time, rectifying environmentally harmful growth is indeed staggeringly expensive: for example, nuclear and toxic cleanups or greenhouse effect reversal. This strengthens the argument for prevention rather than cure, and for not repeating the errors of the industrial countries which passed through an environmentally damaging phase of economic development.

The contrast between Northern and Southern environments is that much local Northern environmental damage is pollution, hence reversible. For example, London's River Thames pollution and the "pea-soup" smogs of the 1950s

[11] For evidence and arguments supporting this important conclusion, see Hueting et al. (1992), appendix 3.

have been largely reversed. On the other hand, most Southern environmental damage is irreversible loss of biodiversity.[12] Irreversible damage cannot be cured; replacement costs are infinite. The North's unnecessarily expensive "damage the environment, then cure it" approach may be affordable (but imprudent) for the rich North. But cures cannot work for irreversible damage, and the South could not afford such expensive approaches in any event. The preventive approach is the only possible one for the South.

2. Importance of Relative Incomes

The traditional view emphasizing global income growth will exacerbate inequality while scarcely denting poverty. An annual 3% increase in global per capita income translates initially into annual per capita increments of $633 for the US, but only $10 or less for China, India, Bangladesh, or Nigeria. After a decade, the US income will have risen by $7257, whereas such income growth will have raised Ethiopia's income by only $41. Therefore, advocates of the traditional view, prioritizing global income growth, should at least state that an unwanted side effect will be to worsen income disparity. When dealing with market competition for finite resources, relative income is more influential than absolute income in determining whether some individuals are excluded from access to available resources. Since markets need at least some social equity, the traditional view will gradually exclude the poor from domestic and international market economies. We emphasize equity within countries as well as between countries.

3. Differential Utility of Needs and Wants

For Northern consumers, self-evaluated happiness is more a function of relative income than of absolute income. Therefore, since aggregate growth increases absolute but not relative income, it contributes little to actual happiness in the North (Hirsch, 1976). So, although our main concern is alleviation of absolute poverty, we recognize that above the poverty level, relative income is a more important determinant of satisfaction than absolute income. Now that Northern income growth yields sharply declining marginal utility, the North should question whether raising its incomes will not increase environmental costs faster than it increases production benefits. Raising Northern incomes not

[12] This generalization stems from the orders-of-magnitude-richer biodiversity in tropical countries and by the related lack of tropical winters. The four main tropical environmental impacts—deracination of jungle dwellers, deforestation, extinctions, and topsoil loss—are irreversible. Water and air pollution in the North are basically reversible. Pervasive global negative externalities (e. g., carbon dioxide accumulation) are probably irreversible over most time frames. The operational distinction between reversible and irreversible damage is that cure for the former is possible for the rich; prevention is the only choice for irreversibles.

only widens the gap between North and South, but may well be reducing Northern welfare absolutely. In the North's choice between consumerism and saving, the quest for relative standing based on visible commodities has biased the North towards consumerism. With less consumerism, more saving in the North could be invested in much needed poverty alleviation and growth in the South. Production to meet basic human needs produces relatively high utilities, frequently with relatively low environmental costs. Wants or luxuries generate relatively lower utility, often with with higher environmental costs.

4. Misplaced Technological Optimism

New technology is often adopted in order to improve productivity, which in turn can raise material standards of living. The impact of a particular technology depends on the nature of the technology, the size of the population deploying it, and the population's level of affluence. In the I = PAT identity, impact equals population times affluence times technology.[13] Accept here as given that world population is projected to double in 40 years and that the rich countries' per capita income ($18,330) is 23 times that of the poor and middle income countries ($800).[14] Therefore, to raise Southern affluence to today's level of the North (holding both impact and Northern incomes constant) means technology must improve 2×23 or 46 times. Since historical technological improvement rates never have exceeded a fraction of the needed 46 times, it will be exceedingly difficult for poor countries to catch up with rich countries in 40 years even if the North maintains current levels of income. It will be that much more difficult if the South is to catch up with a moving target, as prescribed by the traditional view.

Furthermore, this 46-fold increase must be in resource efficiency, and not just in capital or labor efficiency. Historically, much of the increase in capital and labor efficiency has been at the expense of resource efficiency. In agriculture, for example, the increase in labor and capital productivity has required an enormous increase in the complementary resource throughputs (energy, fertilizer, biocides, water) whose productivity has *fallen.*

5. The Value of Economic Self-Reliance

The poor can be helped far more, and with much less environmental damage, by a pattern of development which promotes employment in developing coun-

[13] **Impact** here means impact on or damage to environmental sources or sinks; **affluence** means per capita consumption of resources; **technology** refers to technological efficiency defined in terms of the number of units of human well-being produced per unit of environmental cost. Thus, where I is impact, P is population, and Y is total production, then $I = P \times Y/P \times I/Y$.

[14] Data from The World Bank's World Development Report 1991, Table A.2.

tries—as recently advocated by the World Bank's 1990 "Poverty" WDR, rather than by increasing Northern consumption and relying on "trickle down," as advocated by the traditional view. Poverty alleviation needs employment and self-reliance strategies aimed at using local resources to produce for domestic needs. This translates partly into promotion of value-added and domestic processing, and partly into employment creation. True, developing countries may at first waste a large fraction of raw materials during processing because of using obsolescent technology commonly transferred to the South. For example, modern sawmills waste considerably less wood than obsolescent sawmills do. But this argues for accelerating transfer of up-to-date technologies, rather than the old colonialist approach of exporting raw materials to be more efficiently milled in the North. The most needed such technologies are renewable energy generation and contraceptive methods. Waste prevention, recycling methods, pollution prevention, efficiency increases (e. g., of sawmills), low-input, organic and recycling agriculture, and methods reducing material- and energy-intensity in manufacturing also are priorities. Duchin (1992) argues for supplementing technology transfer by the practice of **industrial ecology**: life-cycle engineering for reducing pollution.

6. Throughput Growth as a Source of Both Income Growth and of Environmental Damage

If the activities contributing to national income are disaggregated into two components, environmentally friendly (e. g., most government services), and environmentally burdening (e.g., industry, agriculture, utilities), then about one quarter of the activities (measured in labor volume) generate about 65% of increases in national income. "Unfortunately, that 25% is precisely the activities which impair the environment." (Hueting et al., 1992) Increases in productivity generated by a relatively small part of the economy spread over the whole society via labor supply demand linkages. For example, a barber's labor volume and real output have not appreciably increased over the last 40 or 100 years, but his (deflated) income or value added has risen by a factor of four. The barber's increased real income has been generated by activities other than his own. These other activities are much harder on the environment than his own activities. Average Northerners now consume vastly more than they did 40 years ago all the way up the income scale—more than twice as much in the case of the US and Japan. For example, 88% of US households now own at least one car (up from 55% in 1935), and the average number of vehicles per household is two—even for barbers.

7. Subsidized Resource Pricing

The poor can be supported more directly and with less environmental damage by "getting the price right," or at least getting the price better than at present.

Today's severe undervaluation of Southern raw material exports means the South is subsidizing the North, in the sense of both externalized environmental costs, as well as governmental incentives, such as logging road construction. Cheap tropical log exports are a case in point. Stupendous subsidies, in the form of unpaid environmental costs, are only beginning to be recognized. Eastern Europe's pollution is a case in point. In the absence of Southern cartels or of producers' agreements to limit production, unilateral price changes are unlikely. We therefore advocate that international organizations, such as the World Bank, the IMF, or the UN, should foster and promote more economically realistic full-cost pricing. A caveat: the North advocates removal of subsidies, but this may hurt the poor more than the rich because in developing countries, the rate of removal of subsidies to the poor exceeds the rate of removal of subsidies to the rich.

Full-cost pricing should also be used to encourage the South to exploit its comparative advantage in agriculture, labor-intensive industry, and raw material processing in order to increase its employment, modernize its subsistence sector, and raise its per capita incomes.

8. Inequitable Trading Systems

"The structure of trade . . . is a curse from the perspective of sustainable development." (Haavelmo and Hansen, 1991) The writers conclude:

> . . . Much Northern growth is based on depleting Southern resources for a price far below the cost of sustainable exploitation. The adoption by the North of the "full cost" principle for pricing Southern resources would help the South more than would Northern growth. Exports only serve a purpose if they finance useful imports. The North should not (tell) the South to export what it cannot afford. Strategies to enhance exports of many staple agricultural products should be critically revisited. Such goods face low demand elasticities in world markets. Individually each exporter takes the world market price as given. In the aggregate, however, the simultaneous implementation of such strategies by many drives the price down dramatically as they all reach their production targets. In the end the export revenue might fall short of paying for the imported machinery, implements, pesticides etc., required (to produce) for export.

9. Dysfunctions of Imbalanced Trade

The traditional view tends to overestimate the virtues of free trade—that is, deregulated commerce across national boundaries. Financial imbalances from deregulated trade have led to debts that are unrepayable, and attempts to repay them by rapid export of raw materials can be environmentally destructive. Natural resource stocks are liquidated to meet debt servicing flows. Current efforts under GATT to include services under free trade will subject that sector

to international competition further pressuring existing payment imbalances. There is a conflict between the "free trade prescription" and the "get the price right" prescription. Countries following World Bank advice to internalize external environmental costs should not be expected to engage in free trade with countries that do not follow similar rules of cost internalization. Tariffs to protect an efficient national policy of cost internalization (not an inefficient industry) should not be ruled out as unwarranted "protectionism." Unpaid environmental costs, such as liquidation of natural capital, are subsidies reducing the price of exports—tantamount to dumping. User costs, from this point of view, should internalize depletion of natural capital. Rectification of the asymmetry of anti-dumping laws for manufacturers, but not for raw materials, would promote global sustainability. This refers to US Pacific Northwest logs exported to Japan as well as Malaysian rainforest hardwoods exported to Europe. At the same time, we acknowledge that Southern trade policies have limited intra-South trade in goods and services, which need to be expanded, and have contributed to real transfers from the rural to urban sectors.

10. The Insecurity of Inequality

From the ecological-economic point of view, our main concerns are that the prescription of raising Northern income will fail to alleviate poverty, will worsen inequality, and will reverse current trends towards sustainability. To these ecologic-economic aspects we append a final concern, that of global security. We believe raising northern incomes will decrease global security, and in HE (His Excellency) Minister Salim's Indonesian view tend to foment social stress and even revolution. Specifically, decreasing sustainability will increase "environmental refugees"—those people forced out of their homes and countries by environmental mismanagement, man-made disasters, and development-induced expulsions, including poisoned water or air, eroded soils, and desertification. A specific example is the environmental damage from Papua New Guinea's Panguna copper mine, which was a major cause of the recent civil war in that country.

The North bears an overwhelming responsibility for many environmental costs to both sinks and sources. As stated in the 1991 Beijing Declaration, the South has taken note that the North is responsible for practically all historical global pollution and continues to emit most of today's global pollution. Some Southern writers (Agarwal and Narain, 1991) argue that the North owes reparation payments to the South for historically disproportionate pre-emption of the global commons. Reparations are to restore base-line equilibria. Northern security will be enhanced to the extent the North reduces inequality in the South. North-South environmental linkages are growing. Two examples: While East-

ern Europe pollutes Scandinavian air, Scandinavia finds it more cost effective to improve its air quality by financing pollution abatement in Eastern Europe itself rather than in Scandinavia. Similarly, the Netherlands finds it more efficient to sequester Amsterdam-produced carbon dioxide from its coal-fired thermal electricity plants by financing tree plantations in South America. South America gets much employment created and more wood construction materials; the Netherlands buys time to internalize its own wastes within its own borders.

The North should see it as in its own direct security interest to invest in the South to reduce inequality, to alleviate Southern poverty, to protect and improve the global environment, and to avoid creating environmental refugees. The South can raise funds by taking on board some of the North's environmental concerns (such as carbon-sequestering in which the tropics have a major additional advantage) by means of tradeable quotas, and by selling the benefits gained from use of their environmental assets, such as intact tropical forest. The traditional view wants to appropriate more Southern resources for the North. The alternate view is that the North has to learn to live within its own means, to reduce its current reliance on global commons and on the environmental resources of the South.

Conclusion

A corollary of the above argument is that possibly the best way in which the North could help the South is by adopting the first oath of Hippocrates: "First, cease doing harm." The traditional view exacerbates harm; the alternative view is more likely to help. In the global approach to sustainability, the North has to adapt far more than the South. The South's contribution to global sustainability could be population stability and prevention of irreversible losses. The North's contribution to environmental damage in the South is clear: ozone shield damage, climate change, greenhouse warming, tropical deforestation, export plantations forcing the poor to marginal lands, indebtedness promoting drawdown of natural capital, and overuse of potentially renewable resources.

Recommendations

The ways in which the North can help the South, in priority order, are as follows:

First, the North must get its own house in order by transforming today's Northern consumerism and borrowing economy into a more sustainable model. An accelerated transition to renewable energy for a stable population is the major element. Internalizing environmental costs in energy prices would be a powerful start. This transition has to be faster than what would be suggested by the market at present. OECD countries are unilaterally phasing in carbon-based

or non-renewable energy taxes. The implications of such taxes for Southern development should be discussed. Part of Northern energy taxes could be allocated to promote Southern sustainable development. Such national taxes may later become global, as proposed by UNCED in 1991, or become tradeable pollution permits (perhaps with futures and options), as proposed by UNCTAD in 1992. Such arrangements would help protect global life-support systems.

Second, the North should internalize the costs of disposal of its toxic and other wastes within its own national borders, rather than exporting them in the name of "comparative advantage" to low wage countries. Internalization of costs to the nation of origin, as well as the firm, gives a stronger incentive to minimize toxic waste generation. This dynamic benefit is more important than the static allocation cost of neglecting "comparative advantage."

Third, the North should halt the harm to the South inflicted by present policies. This includes underpricing of Southern exports, warfare, and global pollution. Of course, the South has a bigger role to play in solving its own problems (as outlined in footnote 5).

Fourth, Northern governments, the private sector, and development agencies create much Southern debt, much of which is unrepayable. The North should address the current imbalance between commercial rate loans, subsidized investments, and grants to the South. The relative proportions of Northern transfers—as (1) loans, (2) subsidized, almost concessionary, IDA-type arrangements, or (3) grants such as that of free access by the South to the North's socially and ecologically beneficial technologies—should be improved. What are the proportions now? More grants and subsidized loans relative to hard loans. Reparations are mentioned above. Questionable loans—loans accelerating liquidation of natural capital, loans failing to internalize full costs, unrepayable loans, and loans clearly for unsustainable purposes—should be halted or cancelled. Global sustainability, poverty, equity, and security would be improved if debts in severely indebted countries were conditionally written off commensurate with progress towards environmental sustainability.

Fifth, because economic justification for foreign exchange loans for environmental investments is difficult, the conventional cost-benefit analysis needs to be broadened to internalize more environmental costs. Where international assistance is required for the South's global or transnational environmental priorities, it should be grant funded. Recent World Bank improvements in this respect are encouraging and need to be accelerated. Economists should begin to consider environmental investments as extended infrastructure investments—in other words, investments in the maintenance of the biophysical infrastructure that supports all economic activity, both public and private. Therefore, where conventional cost-benefit analysis is difficult to apply, as in some conventional

infrastructure investments, the Bank, UNDP and UNEP can now make environmental grants through the pilot Global Environmental Facility. This important facility urgently needs to be revised and vastly expanded if it is to help the South to approach sustainability.

Sixth, the North should focus on direct help to the South rather than on indirect "trickle down" help. Investments should focus more directly on the poor countries, and on the poor strata of society in those countries. Less ODA (Official Development Assistance) should be on commercial (tied) terms, and should include only the most essential projects, emphasizing domestic needs more than the export market. Suggestions to finance such investments have included reparations, conditioned debt relief (e. g., Brady Plan, Trinidad Terms), subsidized loans, and especially grants. The investments are to accelerate needed growth and employment-creation in small Southern economies. International assistance is needed to facilitate and possibly to help purchase rights to environmentally beneficial technology for the South. This will include improving the policy framework for commercial transfer of technology, and training and institution strengthening to improve absorptive capacities.

As old Northern assets depreciate, some may better be replaced in the South with appropriate technology. Thus, the North should accelerate export of advanced but appropriate technology for Southern processing of their raw materials. If the North finds cleaner technology easier to espouse than reducing population or overconsumption, then the North ought to be more willing to transfer technology on easier terms, even at the expense of other ODA. The traditional view would argue for more Northern growth in order to increase ODA. GATT needs to start addressing the environmental implications of trade and needs to take on board the concept of sustainable development.

Seventh, the priorities for sustainable economic development in the South are:

a) Accelerate the transition to population stability.

b) Accelerate the transition to renewable energy.

c) Human capital formation: education and training, employment creation, particularly for women.

d) Technological transfer to leapfrog the North's environmentally damaging stage of economic evolution; job creation rather than automation.

e) Direct poverty alleviation, including social safety nets and targeted aid.

Former World Bank President R. S. McNamara concluded his 1991 United Nations address by calling for official discussion of " . . . how the developed world, consuming seven times as much per capita as do the citizens of the developing countries, may both adjust . . . consumption patterns and reduce the environmental impact of each unit of consumption, so as to help assure a

sustainable path of development for all the inhabitants of our planet. It will be neither morally defensible nor politically acceptable to do less."

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Deviation Fractal Dimension

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SAY-C Software

There are many definitions (and estimates) of the fractal dimension of an object. Most of these definitions are equivalent in continuous domain, but some differ on discretized and digitized data (Barnsley, 1988; Dubic et al., 1987; Feder, 1988). As many researchers have discovered, the standard algorithms are somewhat awkward when it comes to processing actual experimental data (Knuth, 1992). We proposed another fractal dimension definition that for the case of an equally spaced time data series is conceptually simple.

Given that one has a set of date points x^i, i = 1 to N, which were captured at equal time intervals one can form the point-to-point-variance (PTPV) s^2, calculated with the formula

$$s^2 = (\sum (x^i - x^{i-1})^2[<i \leqq N)/(N - 1)$$

where we adopt Iverson's convention that [expression] has value 1 if expression is true, value 0 if expression is false (Moon, 1987). This concept is similar to the usual statistical variance, except that instead of the mean it uses the value of the previous data point. From the PTPV one can form a point-to-point-deviation (PTPD) s by taking the square root of PTPV.

The fractal dimension of an object is related to how that object behaves under scaling of some parameter. For example, in the a coastline case we have that L, the length of coastline, an island can be expressed as,

$$L = Kr^{1-D},$$

where r is length of the ruler used to measure the coastline, D is the fractal dimension, and K is a constant. In this case, the scaling parameter is the length

of the ruler used to measure the coastline. If the coastline has fractal dimension greater than one it follows that the length of the coastline becomes infinite as the ruler decreases in length. Mandelbrot's insight was that over many scales of magnitude the value D typically remains a constant, so that fractal dimension is a good characterization of the scaling nature of a phenomenon.

Using a similar approach, one asks how the PTPD will scale as a function of the time interval between data points. Now if the data behaved as if it were a straight line between the points, it follows that if you halve the time separation, that the PTPD will also halved. On the other hand, if each data points value is completely independent of previous data points, such as series formed from a set of dice throws, the value of PTPD will remain a constant. That is, if we throw the dice twice as often, we wouldn't expect that the new intermediate values would reflect the dice's previous value. That being the case we would expect that the PTPD for a series of dice throws to remain a constant.

These cases suggest a fractal dimension relationship

$$s = Kw^{2-D}$$

where w is interval time separation between points, and K is a constant, D is fractal dimension, s = PTPD.

In the limit as w approaches zero, if D approaches a constant value, then this value can be called the deviation fractal dimension of the data series. Typically, in the proposed method, one approximates the fractal dimension by comparing the PTPD calculated from data points two units apart. (Naturally, one can obtain whole series of variance values by using points in pairs that are three, four and so on time units apart.) In the case where the data values are independent, we would expect that D = 2, and in the case where the data values are smoothly changing in time, we would expect that D − 1. Thus, the deviation fractal is similar to other fractal dimensions and is related to the space-filling capability of the curve from which the data values are taken.

Similarity of the PTPV to the usual variance of statistics may suggest to researchers ways of determining the statistical significance of particular deviation fractal dimension for a given data set. As such, investigation of this statistics may lead to a further synthesis of statistical and fractal dimension concepts. Past research into the connection between statistics and fractal has been fruitful with concepts such as fractal brownian functions, fractal brownian motion, R/S analysis, and fractal statistics (Vicsek, 1992).

As a test of the method, two sets of data were obtained. One dataset consisted of the minute-by-minute measurements of RM-60 Radiation Monitor (product of Aware Electronics) which provided via software connection to a personal computer minute by minute readings in miroroentgens per hour. A number of

data samples were obtained including runs of up to 48 hours periods. The data was loaded into a spreadsheet and the PTPD was calculated using points in pairs 2, 3, 4, · · · 40 time units apart. The PTPV from this procedure remained fairly constant with some fluctuations as expected. Below is a table calculated from a 514 minute radiation file.

Minute separation	PTPD
1 minute	5.87
2 minutes	5.91
3 minutes	5.92
4 minutes	5.75
5 minutes	5.84

Other data samples showed different patterns of PTPD so it appears that PTPD is basically a constant, and that the radiation received in any minute has no predictive power for radiation that will be received in the next minute.

The second set of data consisted of daily weather temperatures (highs and lows) over a 27 year period. When we examined this data we found a PTPV for 1 to 10 day separation as follows (Weather Data provided by B. Farrer).

	PTPV			Fractal Dimension	
Sep.	Highs	Lows	Data Pairs Points	High Temp	Low Temp
1	139.2	106.7	9834	1.41	1.44
2	209.0	157.7	9833	1.57	1.56
3	248.5	188.8	9832	1.66	1.58
4	273.8	212.8	9831	1.65	1.65
5	296.2	229.8	9830	1.79	1.66
6	307.8	244.6	9829	1.73	1.77
7	321.5	253.5	9828	1.79	1.80
8	330.3	260.5	9827	1.84	1.78
9	336.5	267.5	9826	1.79	1.64
10	344.2	277.9	9825		

The fractal deviation dimension in the table were calculated using the formula

$$D = 2 - \ln (s_2/s_1)/\ln (w_2/w_1)$$

The fractal value of 1.4 for one day separation suggests that the next day's weather is somewhat dependent on today's weather. A value of 1.4 indicates roughly that 60% of the next day's weather is explained by today's weather. At the eight day separations point, the variance is not dropping as fast as the time separation interval which can be interpreted as saying roughly that weather 7 days from now and 8 days from now are not that dependent on each other.

The PTPV were also calculated (using C) all the way from w = 1 day to w = 26 years. The PTPV showed *as expected* minimums on *each yearly boundary.* The yearly minimums themselves also showed a 4 year cycle pattern which is nicely explained by leap year phenomena.

Does the deviation fractal dimension yield in the limit as $w \to 0$ the same fractal dimension as other standard definition? As yet, the author hasn't found a proof or a counter example for this conjecture. The proposed definition has some resemblance to the variational method of Dubic, being roughly based on the concept that a curve with fractal dimension is not differentiable. Thus any value which measures the rate of growth of the local derivative should be related to the fractal dimension. This suggests that perhaps a proof similar to what was used in the variation method of Dubic might work.

Some numerical studies were done. Curves were generated with the fractal interpretation method of Michael Barnsley (Feder, 1988). The known fractal dimension of these curves was then compared with the calculations gave results that showed a strong correlation with known fractal dimensions. The differences most of the time ranged from 0 to around 0.15. The deviation fractal dimension was closer in the case of curves with higher fractal values and consistently gave high results for curves with fractal dimensions close to 1. Other computer trials were run in which the data points were brought closer and closer together. As expected the deviation fractal dimension showed a convergence towards the actual values. This suggests that the deviation fractal dimension is indeed a new way of obtaining fractal dimension of data.

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Nonlinear Dynamical Formulation for Describing Growth of Cancer Cells

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ABSTRACT

A set of equations characterizing the interactions between RNA, DNA and proteins is postulated to describe the growth of tumor cells. From this set of equations, a method to determine the fixed points of the system is presented including the use of the Jacobian matrix. Assessment of the nonlinear dynamics around these fixed points is provided.

Introduction

It is widely recognized that the common and perhaps only methods to treat cancer are radiation treatment (Hall), chemotherapy, surgery, microwave hyperthermia (Carr, 1991), and combinations of these. All of these modalities have relative merits and disadvantages. But an overriding concern is that they all fail to reduce the number of tumor cells below a level at which they cease to replicate. Thus, enough tumor cells seem always to be left to form the starting population for another nonlinear growth process. There are hundreds of types of cancer (Golumbek et al., 1991) in a dozen general categories, including lymphoma, mammary (Friedenthal et al., 1984), prostate (Mendecki et al., 1980), myeloma, brain (Borok et al., 1988), lung, leukemia, and melanoma. The author is particularly disturbed by the intractable nature of some of these cancers in terms of treating them, preventing them, and explaining why they are on the increase. Breast cancer (Rosen, 1991; Wallis, 1991) is one of the most publicized cancers and one of the most difficult to understand and treat effectively. In contrast, lymphoma may be treated relatively effectively by surgical removal, followed by a secondary modality if warranted (ie.; chemo- or radiation therapy).

To both understand cancer cellular growth and prevent it in humans, it is

necessary to have a comprehensive research and clinical program using the latest developments and breakthroughs in fields which appear at first to appear wholly unrelated. These fields are microbiology, DNA engineering, molecular structure and crystallography, metabolic biology (May, 1987), nonlinear mathematics-physics-engineering, and clinical oncology.

It is the intent of this paper to outline qualitatively and quantitatively the possibilities of nonlinear mathematics (Mackey and Milton, 1990; West, 1990) in explaining cell growth from a point of view of physics (Tomsovic and Heller, 1991; Pecora and Carroll, 1991; Corcoran, 1991; Scholl and Hupper, 1991; Chi and Vanneste, 1990; Ditto et al., 1990) and engineering (Krowne and Skorupka, 1992; Skorupka et al., 1991; Carroll and Pecora, 1991; Parker and Chua, 1987; Matsumoto et al., 1985; Chua, 1984). A specific set of equations characterizing the interactions between RNA, DNA and proteins is assumed to describe the growth of tumor cells. From this set of equations, a method to determine the fixed points of the system is described including the use of the Jacobian matrix. Assessment of the nonlinear dynamics around these fixed points is provided.

Metabolic Cell Growth Behavior

Increase in the number of living cells, malignant or normal, requires a number of biological processes to happen between two successive cell divisions. Cell mass should approximately double, and the genetic material encoded in the cellular DNA must be provided to the two daughter cells. The time between cellular divisions, the period T, is often divided into three unequal intervals; the G_1 interval, the S interval, and the G_2M interval. The G_1 interval involves cellular events which are preparatory to DNA production in the following S interval. Molecular events during the G_1 interval are not as well understood as those occurring inside the S and G_2M intervals.

A number of models have been proposed which suggest that it is in the G_1 interval that the cell decides to synthesize DNA or enter a quiescent state. And it may be at a particular development level (call it a critical point) in the G_1 interval that the decision whether or not to synthesize DNA occurs. The mechanism for DNA triggering may be delocalized in space with a stochastic flavor caused by random molecular collisions at numerous cellular sites. This may also suggest that the critical point is spread over the G_1 interval with a function describing that characteristic.

The G_1 time interval T_1 is dependent upon the nutrient environment, including growth factors. Some research information indicates that the sequence of

events in the G_1 interval are related to proto-oncogenes, units of genetic information that code for growth-factor-like proteins. Oncogenes are expressed one after another during the cell cycle. Expression of a gene begins with the production of a string of mRNA (messenger RNA). This suggests that protein generation results from RNA presence.

After the G_1 interval and before the G_2M interval is the S interval during which DNA is actually produced. Some consider this the most vital part of cell life. The last interval, the G_2M, constitutes the time span during which the cell prepares for cell division, doing this by separating the DNA into two identical copies and then physically splitting the cell.

Sometimes the cell grows and divides into two unequal sized or constituent filled daughter cells. This is called asymmetric cytokinesis. Both RNA and protein are unequally distributed among the daughter cells. Cells which have more RNA in the earlier part of the G_1 interval, which immediately follows cell division, traverse the cell cycle faster than those that inherited less RNA. It appears that RNA is unequally divided in a random way between daughter cells.

Formulation of Nonlinear Cellular Growth Equations

Following other work (Kimmel, 1987), it is reasonable as a first approximation to describe the linear interactions between the concentrations of RNA, DNA, and protein by the autonomous-like equations (no explicit time dependence)

$$\frac{dR(t)}{dt} = -a_{11}R(t) + a_{21}P(t) \tag{1}$$

$$\frac{dP(t)}{dt} = a_{12}R(t) - a_{22}P(t) \tag{2}$$

$$\frac{dD(t)}{dt} = \begin{cases} 0 & t < T_1 \\ b[P(t - T_1) - P(0)]; & T_1 < t \leq T_1 + T_2 \\ 0 & T_1 + T_2 < t \leq T \end{cases} \tag{3}$$

Here $R(t)$, $D(t)$, and $P(t)$ are respectively the RNA, DNA, and protein concentrations inside the cell. These equations describe the concentration changes within the cell over time in relation to the total time T it takes for a cell mitosis or division leading to replication. The first part of the process, taking time T_1, is an interval involving no DNA synthesis. The next interval of time, of length T_2, involves DNA synthesis. The last interval again involves no DNA synthesis.

This DNA behavior reflects the widely accepted view of what occurs during cellular growth and mitosis. Mitosis is that process whereby the cell duplicates the genetic information to split or divide. The other cellular organelles, proteins, and RNA are also replicated.

Coefficient a_{11} gives the reduction of RNA over time and a_{21} gives the increase in RNA due to the ambient protein concentration $P(t)$. Coefficient a_{12} gives the increase in protein concentration due to the ambient RNA concentration $R(t)$. Coefficient a_{22} gives the reduction of protein over time.

The boundary conditions under which equations (1) through (3) are solved are

$$R(0) = P(0) = D(0) = 1 \tag{4}$$

$$R(T) = P(T) = D(T_1 + T_2) = 2 \tag{5}$$

Boundary conditions (4), (5) indicate that at time $t = 0$ the normalized concentrations of RNA, DNA, and protein are unity, that is, there is one cell with its concommitment of constituents, including the required amounts of RNA, DNA, and protein. After the period T elapses, the cell has undergone exactly one cell division, doubling the quantity of constituents including its RNA, DNA, and protein content. More exactly stated, just before period T, the cell has twice as much RNA, DNA, and protein, which will be used to produce the two new cells originating from the single cell in the mitotic process.

Equations (1)–(3) may be written in a more compact and general form for $T_1 < t < T_1 + T_2$ as

$$\frac{d}{dt}\begin{bmatrix} R(t) \\ P(t) \\ D(t) \end{bmatrix} = \begin{bmatrix} -a_{11} & a_{21} & 0 \\ a_{12} & -a_{22} & 0 \\ 0 & T_c & 0 \end{bmatrix}\begin{bmatrix} R(t) \\ P(t) \\ D(t) \end{bmatrix} \tag{6}$$

where T_c is the translation and subtraction constant operator

$$T_c P(t) = P(t - T_1) - P(0) \tag{7}$$

We may wish to call this system semi-autonomous because of the time translation behavior. Furthermore, the original set of equations (1)–(3) has the third equation giving $dD(t)/dt$ with different expressions in time. This amounts to an explicit time form. Thus the original system is in reality nonautonomous. Only in the interval $T_1 < t < T_1 + T_2$ is the system autonomous with a general form

$$\dot{\mathbf{x}} = \mathbf{f}_{\mathbf{u}}(\mathbf{x}) \tag{8}$$

where the dot above $\mathbf{x}$ indicates a total time derivative and $\mathbf{u}$ is the parameter space vector. $\mathbf{x}$ is the variable space vector given by

$$\mathbf{x} = [R \quad P \quad D]^T \tag{9}$$

and $\mathbf{f_u}$ is the nonlinear operator and $\mathbf{f_u}(\mathbf{x})$ is a vector. For the linear case of (6), $\mathbf{f_u}$ reduces to

$$\mathbf{f_u} = \begin{bmatrix} -a_{11} & a_{21} & 0 \\ a_{12} & -a_{22} & 0 \\ 0 & T_c & 0 \end{bmatrix} \tag{10}$$

which is a matrix operator. Clearly,

$$\mathbf{f_u}(\mathbf{x}) = \mathbf{f_u}\mathbf{x} \tag{11}$$

The vector representation of $\mathbf{f_u}(\mathbf{x})$ is

$$\mathbf{f_u}(\mathbf{x}) = [f_1 \quad f_2 \quad f_3]^T \tag{12}$$

where

$$f_1 = -a_{11}R(t) + a_{21}P(t) \tag{13}$$

$$f_2 = -a_{12}R(t) + a_{22}P(t) \tag{14}$$

$$f_3 = b[P(t - T_1) - P(0)] \tag{15}$$

We can postulate nonlinearities of a fairly general nature and modify (6) accordingly. Writing these equations out in an open form like (1)–(3),

$$\frac{dR(t)}{dt} = -a_{11}R(t) + a_{21}P(t) + b_1R^2(t) + b_2P^2(t) + b_{12}R(t)P(t) + \cdots \tag{16}$$

$$\frac{dP(t)}{dt} = -a_{12}R(t) - a_{22}P(t) + c_1R^2(t) + c_2P^2(t) + c_{12}R(t)P(t) + \cdots \tag{17}$$

$$\frac{dD(t)}{dt} = \begin{cases} 0 & t < T_1 \\ b[P(t - T_1) - P(0)] + B_1[P(t - T_1) - P(0)]^2 + \cdots; & T_1 < t \le T_1 + T_2 \\ 0 & T_1 + T_2 < t \le T \end{cases} \tag{18}$$

For the interval $T_1 < t < T_1 + T_2$, (16)–(18) can be put into compact form (8) where

$$f_1 = -a_{11}R(t) + a_{21}P(t) + b_1R^2(t) + b_2P^2(t) + b_{12}R(t)P(t) + \cdots \quad (19)$$

$$f_2 = -a_{12}R(t) - a_{22}P(t) + c_1R^2(t) + c_2P^2(t) + c_{12}R(t)P(t) + \cdots \quad (20)$$

$$f_3 = b[P(t - T_1) - P(0)] + B_1[P(t - T_1) - P(0)]^2 + \cdots \quad (21)$$

The Jacobian matrix operator $\mathbf{J}_o$ on $\mathbf{f}_u(\mathbf{x})$ produces the Jacobian $\mathbf{J}$

$$\mathbf{J} = \mathbf{J}_o\mathbf{f}_u(\mathbf{x}) = \begin{bmatrix} \frac{\partial f_1}{\partial x_1} & \frac{\partial f_1}{\partial x_2} & \frac{\partial f_1}{\partial x_3} \\ \frac{\partial f_2}{\partial x_1} & \frac{\partial f_2}{\partial x_2} & \frac{\partial f_2}{\partial x_3} \\ \frac{\partial f_3}{\partial x_1} & \frac{\partial f_3}{\partial x_2} & \frac{\partial f_3}{\partial x_3} \end{bmatrix} \quad (22)$$

Using the specific forms for the components of $\mathbf{f}_u(\mathbf{x})$, (22) becomes

$$\mathbf{J}\,\mathbf{f}_u(\mathbf{x}) = \begin{bmatrix} (-a_{11} + 2b_1R + b_{12}P) & (a_{21} + 2b_2R + b_{12}P) & 0 \\ (a_{12} + 2c_1R + c_{12}P) & (-a_{22} + 2c_2R + c_{12}P) & 0 \\ 0 & \frac{\partial f_3}{\partial x_2} & 0 \end{bmatrix} \quad (23)$$

to second order in the nonlinear terms. Let us evaluate $\partial f_3/\partial x_2$

$$\partial f_3/\partial x_2 = \frac{\partial\{b[P(t - T_1) - P(0)] + B_1[P(t - T_1) - P(0)]^2\}}{\partial P(t)} \quad (24)$$

Notice that equations (19) and (20) are independent of D, the DNA concentration. This presumably makes sense because DNA is finally constructed from protein P and RNA R. Thus for the system as posed, we really have a 2×2 sized problem coupled to a third nonlinear equation. The 2×2 Jacobian $\mathbf{J}$ for this system can be extracted from (23) as

$$\mathbf{J} = \mathbf{J}_o\mathbf{f}_u(\mathbf{x}) = \begin{bmatrix} (-a_{11} + 2b_1R + b_{12}P) & (a_{21} + 2b_2R + b_{12}P) \\ (a_{12} + 2c_1R + c_{12}P) & (-a_{22} + 2c_2R + c_{12}P) \end{bmatrix} \quad (25)$$

Once the 2×2 system is solved, the nonlinear dynamical behavior of DNA can be found from (21).

One could also suppose, since $\dot{D} = f_3(P)$ in form, that $\dot{P}$ has terms in D. Maybe, even $\dot{R}$ has D terms. Finally, $\dot{D}$ may not be independent of R and D.

Nonlinear behavior can be determined locally by finding the fixed points of the system, linearizing about those fixed points, and studying the stability characteristics of the particular system under consideration. A fixed point $\mathbf{x}^*$ occurs at

$$\dot{\mathbf{x}} = 0 \tag{26}$$

so that (8) becomes

$$\mathbf{f}_u(\mathbf{x}) = 0 \tag{27}$$

Fixed points occur when the system variable motion is zero as (26) states. It is possible to have only part of the variable space motion zero, in which case (26) will only hold for those appropriate components.

$$\dot{x}_i = 0 \qquad i = 1, \ldots, k \leq N \tag{28}$$

where the system is N dimensional.

Linearizing (8) about the fixed point $\mathbf{x}^*$ gives

$$\mathbf{f}_u(\mathbf{x}) = \mathbf{f}_u(\mathbf{x}^*) + \left.\frac{\partial \mathbf{f}_u(\mathbf{x})}{\partial \mathbf{x}}\right|_{\mathbf{x}=\mathbf{x}^*} (\mathbf{x} - \mathbf{x}^*) + \cdots \tag{29}$$

Truncating this equation after the second term and noticing that the coefficient in the second term is just the Jacobian matrix evaluated at the fixed point $\mathbf{x}^*$,

$$\mathbf{J} = \left.\frac{\partial \mathbf{f}_u(\mathbf{x})}{\partial \mathbf{x}}\right|_{\mathbf{x}=\mathbf{x}^*} \tag{30}$$

(29) can be written as

$$\mathbf{f}_u(\mathbf{x}) = \mathbf{f}_u(\mathbf{x}^*) + \mathbf{J}(\mathbf{x} - \mathbf{x}^*) \tag{31}$$

Defining a new variable $\mathbf{y}$ in reference to the fixed point

$$\mathbf{y} = \mathbf{x} - \mathbf{x}^* \tag{32}$$

noting that (27) holds, and that

$$\dot{\mathbf{y}} = \dot{\mathbf{x}} \tag{33}$$

we find that

$$\dot{\mathbf{y}} = \mathbf{J}\mathbf{y} \tag{34}$$

Linear solution of (34) allows the determination of the types of stability behavior about the individual fixed points. All of the mathematics of linear matrix analysis can be brought to bear on the solution of (34). Consider only the two equation system first, namely, (16) and (17), (19) and (20), and (25). Then apparently

$$\mathbf{x}^* = 0 \tag{35}$$

is seen to be a fixed point satisfying (27) by inspection of the first five terms of (19) and (20). By (25), the Jacobian about the fixed point $\mathbf{x}^* = 0$ is

$$\mathbf{J} = \mathbf{J}_o \mathbf{f}_u(\mathbf{x})|_{\mathbf{x}^*=0} = \begin{bmatrix} -a_{11} & a_{21} \\ a_{12} & -a_{22} \end{bmatrix} \tag{36}$$

Eigenvalues and eigenvectors can be found setting the ith eigenvector solution for $\mathbf{y}$ as

$$\mathbf{y} = \mathbf{y}_i = \mathbf{v}_i e^{t\lambda_i} \tag{37}$$

where $\mathbf{y}_i$ is the ith eigenvector of $\mathbf{y}$, $\mathbf{v}_i$ the nontime part of the eigenvector, and λ_i the time eigenvalue. Placing (37) into (34) gives the equation

$$[\lambda_i \mathbf{I} - \mathbf{J}]\mathbf{v}_i = 0 \tag{38}$$

where $\mathbf{I}$ is the identity matrix. A nontrivial solution to (38) only holds if

$$\det [\lambda_i \mathbf{I} - \mathbf{J}] = 0 \tag{39}$$

Putting (36) into (39) yields

$$\begin{vmatrix} (\lambda_i + a_{11}) & -a_{21} \\ -a_{12} & (\lambda_i + a_{22}) \end{vmatrix} = 0 \tag{40}$$

Eigenvalue solutions λ_i to (40) are

$$\lambda_i = -\tfrac{1}{2}(a_{11} + a_{22}) \pm \tfrac{1}{2}[(a_{11} + a_{22})^2 - 4(a_{11}a_{22} - a_{12}a_{21})]^{1/2} \tag{41}$$

Because R, P, and D are real physical quantities, a_{11}, a_{12}, a_{21}, a_{22} must be real also in order to make the rate of change of RNA, DNA, and protein real, as a cursory inspection of (16) and (17) indicates.

Based upon the fact that there are two eigenvalue solutions λ_i, let us delineate the possible states of the system. Define

$$A = -\tfrac{1}{2}(a_{11} + a_{22}) \tag{42}$$

$$B = \tfrac{1}{2}[(a_{11} + a_{22})^2 - 4(a_{11}a_{22} - a_{12}a_{21})]^{1/2} \tag{43}$$

where A is half the system trace and equivalently for B

$$B = [A^2 - \det \mathbf{J}]^{1/2}$$

where the determinant of $\mathbf{J}$ is found from (36) so that (41) becomes

$$\lambda_i = A \pm B \tag{44}$$

Define

$$C = A^2 - \det \mathbf{J} \tag{45}$$

so that B can be rewritten as

$$B = C^{1/2} \tag{46}$$

There are three fundamental cases. One is when

$$C > 0 \tag{47}$$

and the eigenvalues are real, meaning that the possibilities available for the system are decay and growth. The second is when

$$C < 0 \tag{48}$$

and the eigenvalues are complex, meaning that the system may be in decaying oscillation or growing oscillation. The last case is when

$$C = 0 \tag{49}$$

when the system has two degenerate eigenvalues and the system is either in decay or growth.

Consider case (47) first. If the determinant of the Jacobian for the system is

$$\det \mathbf{J} > 0 \tag{50}$$

then (47) is satisfied when

$$|A| > \det \mathbf{J} > 0 \tag{51}$$

This places the eigenvalues in the interval

$$0 < \lambda_i < 2A \tag{52}$$

and tells us that the system must be unstable in time and has what is known as an unstable node (Scholl, 1987), i.e., the system undergoes growth if

$$A > 0 \tag{53}$$

and the system phase space trajectories diverge from the fixed point. For the half trace of the system being

$$A < 0 \tag{54}$$

the eigenvalues exist in the interval

$$0 > \lambda_i > -2A \tag{55}$$

and the system must be stable in time and has what is known as a stable node, that is, the system undergoes decay and the system phase space trajectories converge on the fixed point.

If the determinant of the Jacobian for the system is

$$\det \mathbf{J} < 0 \tag{56}$$

then (47) is always satisfied and

$$\lambda_1 > 2A \tag{57}$$

for an unstable solution, and

$$\lambda_2 < 0 \tag{58}$$

for a stable solution, and the system has a saddle point, provided (53) holds. For the half trace of the system obeying (54),

$$\lambda_1 < -2A \tag{59}$$

for a stable solution, and

$$\lambda_2 > 0 \tag{60}$$

for an unstable solution, and the system still has a saddle point.

The second fundamental case occurs for (48) when the system is found to possess oscillations. Equation (48) comes about when (50) holds and

$$A^2 < \det \mathbf{J} \tag{61}$$

leading to a system with a stable focus if the half trace A of the system obeys (54). The phase space trajectory of the system is a converging elliptical spiral. The system has an angular frequency equal to

$$\omega = |\mathrm{Im}[\lambda_1, \lambda_2]| = \{\det \mathbf{J} - A^2\}^{1/2} \tag{62}$$

and a decay or damping factor equal to

$$d = \mathrm{Re}[\lambda_1, \lambda_2] = A \tag{63}$$

and the two complex conjugate eigenvalues λ_1 and λ_2 are such that

$$\mathrm{Im}[\lambda_1] = -\mathrm{Im}[\lambda_2] \tag{64}$$

$$\mathrm{Re}[\lambda_1] = \mathrm{Re}[\lambda_2] \tag{65}$$

If the trace of the system is greater than zero obeying (53), the system has an unstable focus and the phase space trajectory of the system is a diverging elliptical spiral.

The third fundamental case occurs for (49) causing degenerate eigenvalues which are real and take the sign of the half trace of the system A. According to the sign of A, see (56), an unstable or stable system node occurs.

Conclusion

We advocate the importance of a more holistic approach to cancer research. We strongly suggest that the effort be restructured so that several critical fields simultaneously interact and collaborate in order to develop a much more comprehensive program. These fields include microbiology, DNA engineering, molecular structure and crystallography, metabolic biology, nonlinear mathematics-physics-engineering, and clinical oncology.

Because the fundamental underlying growth behavior of tumor cells is nonlinear, this paper focused on studying a particular set of equations describing the interactions of DNA, RNA, and proteins in the context of a linear system and its generalization to a nonlinear system. We hope our work will lead to further

research into nonlinear dynamics of cancer cellular growth. There have been many studies which have dealt with the growth of cancer cell population dynamics from an intercellular point of view (Duchting and Vogelsaenger, 1981; Suh and Weiss, 1984; De Boer and Hogeweg, 1986; Sluyser and Hart, 1983; Blum, 1974; Kimmel and Axelrod, 1991; Marusic et al., 1991; Chover and King, 1985; Merrill, 1984; Xu and Ling, 1988; Duchting and Dehl, 1980; Stein and Stein, 1990; Diekmann et al., 1984; Lasota and Mackey, 1984), but little work has been done to date to derive models for intracellular behavior (Freeman and Wilson, 1990; Gallez, 1984; Brooks, 1977; Baserga, 1984; Riddle et al., 1979; Traganos et al., 1982; Binggeli and Weinstein, 1986) which describe tumor growth.

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How Seriously is the US AIDS Population Understated?

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ABSTRACT

The work documented in this paper is an attempt at a realistic assessment of the true flow of AIDS cases entering the health-care system of the United States. Understatement in the numbers of AIDS diagnoses clearly causes serious problems for policy makers and analysts trying to understand the scope of the HIV/AIDS pandemic. From the perspective of the modeler and data analyst, the major problems would appear to result from *lags in the reporting* and *nonresponse*. Our focus is therefore on the development of approaches for dealing with these problems. The major source of input data for our analysis is the nationwide database of the CDC.

Despite the fact that the United States and the world are struggling to deal with HIV/AIDS. Yet, to date, there has been little agreement on the level and the sweep of the epidemic. We believe that much of this confusion results from the very nature of the disease, from how affected people and the medical community have been responding, and, most importantly, from the manner in which HIV/AIDS data are collected and become available to the public over time.

Most specifically, all such HIV/AIDS forecasting systems must convert reported AIDS cases back in time into a flow of HIV cases using what is often called backcalculation or backcasting. The systematic understatement of AIDS cases by year of diagnosis, however, compromises the very quality of the main statistical input stream for the analysis systems. In order to use such data in a valid way, there must be some sort of rational means for correcting underestimates of data and trends caused by reporting delays. Furthermore, it is well accepted that some data are likely never to be captured at all by the national reporting system, although the medical needs of this hidden subpopulation will still burden the health-care system.

Our work, then, focuses on the development of rational procedures for analyzing lags in data reporting and the subsequent development of a suggested model for extrapolating current AIDS statistics into a more useful data series. This is accomplished by applying multiple regression and goodness-of-fit techniques to the CDC data. In addition, a subgroup analysis is performed to determine what effect the shifting composition of the HIV+ population might have on future trends. We also examine the related question of underreporting.

Introduction

There are already strong indications that the HIV/AIDS epidemic will develop steeper growth levels in the period 1992–94, and will continue such in-

creases for the subsequent several years. The CDC has projected 58,000 to 85,000 new cases of AIDS for 1992 (CDC Centers for Disease Control, 1990b; Centers for Disease Control, 1990c). There have also been, of course, numerous estimates of the total size of the HIV-positive population. For example, (Dondero Jr. et al., 1987) provided an estimate in 1987 of 1 to 1.5 million. The CDC's own estimates have been for approximately 750,000 by January 1986; approximately 1 million by June/July 1989; and still 1 million by December 1991 (Centers for Disease Control, 1990b; Centers for Disease Control, 1990c; Centers for Disease Control, 1992a). Additional discussions on key CDC methodological issues are in (Brookmeyer and Gail, 1988; Centers for Disease Control, 1989b; Centers for Disease Control, 1990a; and Centers for Disease Control, 1992b).

The original objective of our project beginning in 1988 was the development of an innovative approach to the problem of forecasting the pace of the HIV/AIDS epidemic. In part, this stemmed from the Dondero et al. 1987 assessment which sought firmer estimates/forecasts of prevalence and incidence (Dondero Jr. et al., 1987) and successive parallel statements emerging from the 1988 Leesburg Workshop (Office of Science and Technology Policy, 1988), as well as from the more recent report of the October 1989 CDC Workshop (Centers for Disease Control, 1990c). However, all of these key conferences over the years have led neither to a consensus nor to a resolution of conflicting concepts among the participants of how to reconcile outstanding issues. Our forecasts and model development work are most completely documented in (Harris et al., 1992).

No matter what approach seems in favor at any time, there are major methodological problems still open. We do not think that any of those active in this problem area were surprised at the strong criticism by the GAO of the quality of the major forecasting systems in June 1989 (General Accounting Office, 1989), even if one might differ with some of the specifics in that report. Through all of this, it appears that there is a persistent problem of converting CDC's reported data to a more accurate picture of the status and evolution of the epidemic. The most profound issues revolve around the well recognized problems of lags in reporting and complete nonresponse (see, for example, CDC (Centers for Disease Control, 1990c), Table 4).

For example, questions of estimating HIV prevalence and incidence from prevailing data sources were the subjects of (Heyward and Curran, 1988; Curran et al., 1988; and Trafford, 1988). In addition, an Israeli paper by (Siegman-Igra et al., 1988) revealed some universal aspects of HIV/AIDS compromising the quality of data extrapolation, such as the role of mobility in the spread of the disease and possible AIDS longevity variations between various risk groups.

In our studies, the cohorts have been time-based, and the focus of our work has been on projecting a time series that would anticipate the historical data

series evolving for the epidemic over the coming decades. Our analysis begins from CDC reports of *AIDS cases,* from which we estimate the sizes of emerging morbidity-cohorts for each of the years of the US experience with the epidemic, thus the name Cohort Cascade for this major part of our modeling system. From the estimated development of the disease, we follow the progress of each annual cohort through the range of years of interest, and then aggregate the individual cohorts into an annual dataset of the expected numbers in various stages of illness for each of the years to the turn of the century. We have already made some first attempts at critical data adjustments, largely based on preliminary analyses of lags in reporting and on nonresponse. It is precisely because of our evolving attention to these data issues that we have prepared this current paper. The Walter Reed Institute of Medicine has formulated a series of stages to describe the progression of the disease (Brodt et al., 1986; Cowell and Hoskins, 1987; Redfield et al., 1986; Society of Actuaries, 1989). The first stage (abbreviated as HIV+) refers to the initial asymptomatic phase during most of which the patient is in apparent good health. This is followed by the LAS (Lymph Adenopathy Syndrome) stage which includes symptoms of lymph system dysfunction, and then by the ARC (AIDS-Related Complex) stage which includes a specific set of additional ailments of greater severity. Finally, AIDS (Acquired ImmunoDeficiency Syndrome) is the terminal stage of HIV illness and is marked by an additional set of opportunistic diseases, one of which is associated with the patient's death. Remember throughout this work the important distinctions between onset of HIV-positivity and the ultimate transition into AIDS.

Other staging classifications are in use, e.g., based on T4 lymphocyte counts. These staging categories have not yet played an important role in forecasting system development; instead, they serve the same functions that any categorical structure serves in statistical analysis and reporting, as entity identifiers.

Nonreporting of AIDS cases, intentional or not, is a particularly important problem in estimates and projections of the epidemic. The findings of a study on the increased level of reporting of pneumonia deaths among younger adults indicated that many AIDS cases have been erroneously attributed to pneumonia (Stoneburner et al., 1988; and New York Times, 1988). More on this matter later.

Designing An HIV/AIDS Prevalence And Incidence Model

An effective modeling system for understanding the societal impact of HIV/AIDS should provide several types of projections:

- National estimates of HIV death by year and by risk category;

- National estimates of HIV morbidity, detailed by year, by risk category, and by stage; and
- The national (and local) resources (dollars, hospital days, physician-times, etc.) required by HIV demands, detailed by year, by risk category, and by stage.

The essential components of the incidence and prevalence portion of this modeling system would consist of two major linked sequential parts:

1. an Infection Model, which would generate the estimated annual (newly-infected) cohort for each year, 1976 to the present and for several future years. Such a model would estimate what type of person (and how many of that type) would move from HIV-negative to HIV-positive during a given span of time (e.g., the number of male heterosexuals who acquired HIV in 1989; the number of female IVDUs who will acquire HIV in 1993; etc.)
2. a Progression Model for translating the time cohort estimated by the Infection Model and its disaggregated major risk groups into specific levels of morbidity and mortality over time. The Progression Model would use estimated experience probabilities to obtain a plausible description of an annual HIV+ cohort's movement through the several morbidity stages.

The driving element of such a structure would be the number of newly infected (HIV-positive) persons in a given year, the infectivity base. When a person contracts the virus, a biological clock affecting his or her immunity system begins to tick; while the incubation times are recognized to vary greatly among individuals, each infected person is assumed to pass through each of several stages that have been defined by medical researchers.

The precise dataset for HIV/AIDS prevalence and incidence we have developed required the following techniques and assumptions:

1. Estimating the expected size of the newly infected HIV population for each year from 1976–86 by backcalculation using reporting lag-correction factor (i.e., numerical adjustments for data bias due to severe lateness in case reporting), without which the size of the seropositive population (past and future) would be systematically and greatly understated;
2. Projecting annual growth rates for the HIV-infected population for the years 1986 through 1995 by a log-linear smoothing; and
3. Application of a nonresponse or hidden-case adjustment factor of 25% (which can be readily changed to any higher or lower assumed nonresponse level).

Our modeling system is an aggregated one. Ideally, however, each of the newly-infected cohorts should be amenable to disaggregation. Each year's newly infected (time-) cohort contains an increasingly heterogeneous population. Initially, the overwhelming majority of HIV/AIDS seropositive were male and gay. In recent years, IV Drug Users and heterosexuals have become larger proportions of the newly-reported AIDS cases. Increasingly, IVDUs have begun to exceed Gays/Bi-s among new AIDS cases being reported in several states.

The HIV+ cohort for a given year, the general aggregate number of that year's newly-infected HIV cases, is a major element in estimating stage incidence levels

in subsequent years. The desegregated risk groups, subcohorts, are an even more meaningful statistical entity, since they can be the basis for estimating shifts among key demographic groups (particularly the gays, IVDUs, and heterosexuals), by ultimately using a more complete Markov chain model. Undetected compensating shifts within the aggregate HIV+ cohort can obscure important behavioral changes among the key risk groups.

Infectivity Issues and Backcalculation

Remember that there are no data for the HIV cohort sizes in 1976, 1977, and succeeding years. Our Cohort Cascade structure provides a feasible basis for estimating those cohorts, but it is just one of a number of potential approaches for approximating incidence and prevalence through the years. However, such modeling systems necessarily start from data inputs on AIDS onset. Our model, just as many others, recreate the flow of new HIV cases through a range of years by backcalculation, which employs an estimate of the natural history of the progression of HIV/AIDS working in reverse from estimated numbers of new AIDS cases. Movement through the various stages of HIV disease is assumed to be linearly proportional to stage-population size.

In addition to the staged proportionality of the Cohort Cascade portion of our model, we assume that infection is spread at a (piecewise) constant rate. That is to say, if the cumulative total number of surviving infectees by the beginning of year t is represented by N(t) and the number of deaths in year t by D(t), then the net increase of cumulative infected people in year t would be

$$N(t + 1) - N(t) = \alpha_t N(t) - D(t) \tag{1}$$

or

$$N(t + 1) = [\alpha_t + 1]N(t) - D(t). \tag{2}$$

Henceforth, we say that α_t is the growth rate or "multiplier" for year t; we do permit some of the $\{\alpha_t\}$ to be equal for purposes of illustration.

In the years, 1976 to 1988, starting with "one" infected individual, an annual constant growth rate of about 2.5 would have produced a cumulative total of about 3.38 million infected persons by the start of 1988. But if the epidemic started among an ultra-high risk group within the homosexual community, then spread to the remaining (lesser-high-risk) homosexual males, the growth rate in the first few years must have been considerably higher (than 2.5) with an implied lower subsequent growth rate, particularly as the awareness of the cause and deadliness of the disease emerged. That set of facts and assumptions is the basis for a surrogate infection model.

The estimation of the growth of the infected population (the "survivors", or total of the subpopulations in each of the stages) is the vital estimating factor in all estimation of HIV/AIDS; the infection model (using the multiplier measure) is the pivotal input to the progression model. A model responsive to this scenario would start with a very high growth rate level, which would lessen year-by-year within the homosexual community, then track the passage of the epidemic into the heterosexual population to estimate the number of US HIV seropositives. Our actual estimates are derived using linear programming, with a full discussion of the procedure found in (Harris et al., 1992).

Estimation of HIV populations and time-cohorts is, therefore, anchored in two major factual sets of data, the progression factors and AIDS-onset data (AIDS-onset: the year that an HIV patient transitions into the AIDS stage). AIDS-onset data have been reported regularly by CDC in earlier years in the AIDS Weekly Surveillance Report (AWSR, Table G) and currently in the HIV/AIDS (Monthly, now Quarterly) Surveillance Report (HSR), and the progression factors can be estimated from the several cohort studies of seropositives (for example, see Cowell and Hoskins, 1987).

Backcalculating Past Cohort Sizes from Annual New-AIDS Cases Reported

How do these new-AIDS cases relate to infectivity? The key of our infection model are the values of the $\{\alpha_t\}$. Equations (1) and (2) may be manipulated to yield

$$\alpha_t = \frac{N(t+1) - N(t) + D(t)}{N(t)} . \tag{3}$$

It is clear that if the values of the {N(t)} and the {D(t)} are known accurately, then the $\{\alpha_t\}$ can be easily determined. However, there is even more uncertainty about the year-by-year size of the seropositive population than there is about the yearly counts of the number of people who have reached the AIDS stage. Therefore the above expression for α_t is not all that useful.

Since it is not feasible to determine the $\{\alpha_t\}$ directly using the {N(t)}, is desirable to estimate the unknown values of the multipliers using the more reliable counts of the number of people with AIDS. This can be done by exploiting the fact that, according to our conjecture, the yearly number of new AIDS cases is related to the sizes of the HIV+ cohorts via the proposed progression model. One way of doing this applies AIDS-to-COHORT equations in a backcalculation mode.

If AIDS numbers were perfectly reported and if the probability structure we

Table 1.—Infected-Population Multiplier Estimates

1976	1977	1978	1979	1980	1981	1982	1983	1984	1985
1.80	4.07	1.72	1.36	0.85	0.70	0.55	0.45	0.37	0.32

assume for disease progression were also precise, then we would be able to write down a simultaneous system of linear equations in yearly cohort sizes, with the reported numbers of new AIDS cases on the right-hand side. To get the exact coefficients of such linear equations, we would begin from an assumption that the typical HIV case was infected on a given day. Defining that event as the start of Year 1 (and conclusion of Year 0) of that individual's HIV-infection, that person's probability of AIDS-onset is set in Cohort Cascade as .022 by the end of Year 3. Stated in equivalent terms, according to the factors of the Frankfurt study, a cohort of 1000 is expected to have 22 members develop AIDS by the end of 36 months, i.e., the end of Year 3. By the end of the next year (Year 4), Cohort Cascade estimates that an additional 109 members will display AIDS, leaving 869 members AIDS-free by the end of Year 4; and so on.

Infection Multipliers

Each sequence of new AIDS cases relative to the unfolding numbers of seroconversions creates a set of infection multipliers. The pattern of these multipliers determines the estimated growth of the epidemic in the future, and, indeed, a major test of any HIV/AIDS modeling system is its ability to estimate these numbers well. We display in Table 1 the multipliers generated by the use of our linear-programming model applied to the CDC dataset of October 1990.

The first major observation from Table 1 is that the multiplier pattern has been monotone decreasing and convex since the early years of 1976 and 1977 when little was known about the disease. The pattern passed through the mid-40% range in 1983, when IVDUs became a significantly larger element of the newly-infected population. Since the IVDU subpopulation has been observed to be the most difficult to reach and seemingly the least amenable to behavioral changes, the declining multiplier rate is believed to be in a slowing rate of decline for the years after 1985.

The work of (May and Anderson, 1987) contains a related approach to the estimate of infection spread. They calculated a growth rate of 1.0 as the result of a serologically based approximate doubling of the epidemic in San Francisco and New York within 10–11 months over the period 1978–1980. The multipliers we have settled on for our work (by backcasting estimation) are approxi-

mately 1 for both 1979 and 1980. However, we do believe that the numbers had to be considerably higher (originally, likely 2–3) in the years 1976 and 1977 in order to boost the epidemic during that time (i.e., doubling was much quicker in the very early days). The year 1978 is a kind of boundary year (in the sense that prior years provided insufficient cases to permit statistical precision), and is often the base from which to calculate forward estimates of multipliers, as it is for May and Anderson's approach. But, we repeat, the ability to estimate disease spread is very much dependent on the manipulation of current data sources and the resultant extrapolations that they require.

Methodological and Validation Issues

Caveats, Confounding, and Data Biases

Through it all, it seems very clear that there are two major data issues which must be handled carefully when using AIDS reports, namely, lagged reporting of both cases and deaths, and nonresponse. For example, every month in 1991, the CDC reported as "new cases" not only 1991 AIDS cases but also cases diagnosed in 1990, in 1989, and earlier years. The most recent years are always the most underreported. One effect of this lagged reporting is to give an illusion of "trend-washout" in the uncorrected data. That is, the real trend level, which shows a consistent increase each year, is most significantly affected by a damping bias in the recent years.

Nonreporting is recognized also as a significant bias, since HIV and AIDS infectees are stigmatized in life as well as death. Denials of AIDS during the final months of life characterized the cases of several notables in recent years. Reporting in a number of cities reviewed by CDC indicates a higher-than-expected incidence of pneumonia deaths among younger adults, which implies that the opportunistic disease, and not AIDS, may be on the death report in many cases (Stoneburner et al., 1988; and New York Times, 1988).

Suicides during the latter stages of HIV are another likely source of misclassified HIV cases. Also, pre-AIDS deaths (e.g., ARC-related) would not appear in the AIDS reports, although they are HIV-related.

There are also some confounding elements in analyzing time-based cohorts. One such problem is the mobility (both travel and migration) of seropositives across regional and national boundaries. The mobility of homosexuals has been documented (e.g., see Siegman-Igra et al., 1988) as speeding the dispersion of HIV throughout the world. In estimating the growth of the epidemic, how should one handle the travel of seropositive, that is those already infected, from one locale to another? If a homosexual who acquires HIV in San Francisco returns home to Kansas City, is he then part of Kansas City's AIDS growth?

Alternatively, does that mean that San Francisco's AIDS growth has declined slightly? The National Opinion Research Center (New York Times, 1989a) has concluded that there is a great underreporting of Midwest cases, and suggests that it is a combination of avoidance (primarily by affluent whites) and a misattribution of Midwestern cases to either coast. This indicates that state and regional estimates will be subject to greater relative error than national projections.

Another, subtler, bias or dual-confounding factor is the outcome of an extended set of AIDS-determinant factors. These factors were supplemented in late 1987 and were scheduled again for additional factors in 1993. This has the effect of shifting the mean AIDS-onset "discovery" date to an earlier point, thus leading to two misperceptions, the mean AIDS incubation period will now appear to be shorter and the mean AIDS longevity will appear to be longer. While other causes may truly shorten the AIDS incubation mean (e.g., inclusion of other groups, women, children, etc.) or may truly increase AIDS longevity (e.g., AZT, earlier start of healthcare, etc.), the extended-set bias should be accounted for and factored out.

The introduction of intervention therapy to the HIV/AIDS population brings a potential bias to the estimates and projections of this model. Cohort Cascade is based on distributions for stages and years which derive from the natural history of the HIV/AIDS virus. PreAIDS intervention lengthens the time-in-stage for the seropositive, particularly those in the ARC stage. Intervention therapy after the onset of AIDS has been suggested as deferring death for about seven months, from 14 months to 21 months (Lemp et al., 1990).

There are some limitations on the impact of these treatments, however. Firstly, not all patients are able to tolerate the treatments. Secondly, cost and other considerations have make them available to only a fraction of those who could benefit from their application. Thirdly, since AZT on any significant scale was not made generally available until about 1987, the beneficial impact of intervention therapy would be applicable primarily to two subpopulations, the ARC and AIDS patients, respectively about 30% of the HIV/AIDS population. Assuming that tolerance and availability will reduce that to about 10% of the HIV/AIDS population, one means of minimizing the bias in using natural history distribution solely is to create alternative cohorts with modified ARC and AIDS stage distributions reflecting the new extended-life findings.

Lag Correction

We are certainly not the first research team to worry about making corrections in data before utilizing them in the exercise of a modeling system. The

papers by (Karon et al., 1988; Karon et al., 1989) discussed several methods for adjusting for lags in reported information, the Karon et al. Table 1 in (Karon et al., 1989) depicted data for 1984–1988 by quarters before and after adjustment. We derived our first empirical formula for lag correction, namely, $[1 + (1/m)]^2$ (i.e., at a point in time m months after the date of diagnosis, the expected number of cases to be logged for that month ultimately would be the current number being reported by the CDC multiplied by $[1 + (1/m)]^2$ (see Harris et al., 1992). That is, data in the June 1991 CDC report for the half-year ending June 1990 would have a value of m = 12, while m = 18 in the December 1991 CDC report for the same June 1990 data. That is, m equals the number of months in a given CDC report since the half-year data were "closed-out."

We did estimates for this 1984–88 period; the Karon et al. table 1 data are comparable to ours. Table 1/Cohort Cascade comparisons are: for 1984 AIDS (6019/6058), 1985 AIDS (11285/11297), 1986 AIDS (18590/18416), and for 1987 AIDS (27694/27502). (Later in this paper, we offer a detailed analysis of a more complete analysis of lag correction and the resultant development of a new set of correction factors.)

(Brookmeyer and Damiano, 1989) also developed a set of adjustment factors; when the three sets of factors are compared, you get (Karon et al./Cohort Cascade/Brookmeyer & Damiano) for 4–6 months old data (1.33/1.36/1.39), 7–9 months (1.24/1.23/1.22), 10–12 months (1.19/1.17/1.15), 13–15 months (1.16/1.14/1.11), 6–18 months (1.13/1.11/1.08), 19–21 months (1.11/1.10/1.07), and 22–24 months (1.10/1.08/1.05).

We have used our empirical adjustment factor, $[1 + (1/m)]^2$, applied the progression rates derived from the natural history data of the Frankfurt study and the Cowell and Hoskins paper to structure our set of simultaneous equations, i.e., our backcasting/backcalculation model. That model has produced estimates for each annual cohort-population of HIV-infectees. Each cohort's distribution through the successive HIV stages in each of the years following infection is aggregated to develop the ultimate dataset, a tabulation of cases by stage for the years 1976–93. There is an extrapolation component in the method used for the years beyond 1986, but it is an extrapolation based on the annual infectivity rates (i.e., the multipliers), and not on the annual newly diagnosed AIDS cases.

Cohort Cascade's (old) lag-correction formula (as discussed above), applied to all past reported cohorts of annual new AIDS cases, provides more accurate estimates of AIDS cohorts. The actual results of these sorts of calculations on past published (e.g., in July 1987 and November 1988) AIDS cohorts estimated that they would increase through time as shown in Table 2.

Table 2.—Validation of Lag-Correction Projections

Year of Cohort	Date Data Publ'd	Original CDC Data	First CohCasc Lag-C	2/90 CDC Update	Percentage Difference
1984	7/87	5619	5972	5837	2.31%
1985	7/87	9756	10743	10867	−1.14%
1986	7/87	13583	17277	17601	−1.84%
1987	11/88	23771	27502	25863	6.34%

Further support for our preliminary approach was offered by noting how (in Table 3) our (old) factors compared to the delay experience expected by the CDC (Centers for Disease Control, 1989a, Table 4). However, it is, of course, quite possible that both CDC and Cohort Cascade are using the wrong factors. It has therefore been a major target of our current work to examine this whole matter much more carefully, and the results of our investigation are found below in the "Construction " section. One of our findings is a revision of Cohort Cascade's lag-correction factors to a new set, Lag-Correction II: 3.3, 2.4, 2.1, 1.6, 1.4, which would replace line 3 of Table 3. The significance of these new factors is that they suggest a far greater impact on estimates of incidence, prevalence and trends for HIV/AIDS. More on this latter issue will be discussed in the "Concluding Remarks" section.

More On Backcalculation/Backcasting

No matter what approach is used in a modeling system for calculating backwards in time, it is necessary to establish a database of past populations from which further analysis can append subsequent HIV cohorts. It should be kept in mind that the progression factors of the Frankfurt study may in time be displaced by later studies of HIV natural histories. In such case, the new progression factors would be used to generate new projections. The Frankfurt factors were derived from a gay population; it would be desirable for subsequent progression factors to be subpopulation specific, e.g., a set of gay progression factors, another for IVDUs, and another for heterosexuals.

It should be kept in mind that although we are analyzing AIDS reporting issues applying some degree of mathematical sophistication, the definition of

Table 3.—Comparison of Lag-Correction Factors (CDC vs. Cohort Cascade)

Months after diagnosis:	1	2	3	6	12
(CDC) Approx. formula:	3.7	2.1	1.7	1.4	1.2
(CohCasc) Lag-Correction I:	4.0	2.3	1.8	1.4	1.2

what constitutes an AIDS case is subject to medically subjective modifications imposed by administrative decisions (e.g., definitional changes to the criteria-set of indicator diseases or symptoms, particularly in the Fall of 1987). The 1987 changes introduced several additional AIDS-indicator criteria.

The expected result of the enhanced criteria-set was a "blip", or sudden increase in the trend, which could expect a counterbalancing dip subsequently as the cases which were generated by the several new criteria move up in time for an earlier diagnosis. That is, these cases would indeed have been in later time included as they met one of the pre-1987 criteria. But having moved up, they depleted the stream of the later cases, which would result in a sparsity of candidate cases for later transition into the AIDS population. We do, however, believe that our analyses of lags and underreporting, as documented later in this paper, are compromised only to a very minor degree by these questions, primarily because these data discontinuities are smoothed out over time.

Our linear-programming method for backcalculation is actually dual in function: it establishes a past history of the HIV epidemic's growth through 1986, and it sets a bases for projecting infectivity multipliers to the years 1987 through 1993 and beyond. The past time series of multipliers, 1976–86, leads us to a consideration of the pattern for 1986 and beyond. If the pattern of decline continues, then the eight years 1986–93 would have infectivity multipliers declining gradually to about 12%. If the pattern levels off, the multipliers would instead average about 20%. Since we know that IVDUs are becoming a larger segment of the new AIDS cases, we might expect the multipliers to level-out to about a 20% average rate currently and in the near-future. Most analysts appear to accept the argument for a continuing, variable decline in the annual, aggregated infectivity rate through 1993 and beyond; but there is no consensus as to expected rate. In the end, the reliability of agreed-upon lag-correction factors will likely give all of us a better picture of how the epidemic will truly spread through the rest of this decade, particularly in light of the increasing focus on ameliorative therapies.

Construction Of A More Rigorous Lag Correction Function

The distribution of lags between the time of the diagnosis of AIDS and the time at which the case is reported to the CDC can be used to obtain an estimate of the number of cases that will ultimately be attributed to a specific diagnosis date. For instance, suppose it is known that 40% of all cases which will eventually be attributed to a given month of diagnosis have been logged at the CDC at the end of a two month period following the diagnosis month. Then, if N(2)

cases have been ascribed to the given month of diagnosis at the completion of the two month period following that month, the number of cases which will ultimately be attributed to the month is simply

$$N(2)/0.40 = 2.50 \cdot N(2).$$

In general, if the proportion of cases which have been reported within m months following a given month is F(m), and if N(m) is the number of cases which are known at that time, then the total number of cases which will eventually be reported for that date is given by

$$N(\infty) = N(m)/F(m).$$

That is, the inverse of the cumulative distribution can be used as a multiplicative lag correction factor.

The preceding expression for N (∞) is not useful unless F(m) is known, and unfortunately there is no way to directly observe or measure the distribution function since

$$F(m) = N(m)/N(\infty)$$

depends on the unknown value N(∞). However, by noticing and exploiting some key patterns in the available data one can develop a plausible approximation to F(m). In this section, we will describe a step-by-step procedure for constructing an estimate of the distribution function for the reporting lags, and the formula obtained from the method will be compared to the (implicit) estimates of the lag distribution reported by the (Centers for Disease Control, 1989a), Table 4.

Exploratory Analysis

The raw data on which the following analysis is based consists of the lags corresponding to all reported AIDS cases which have been attributed to the years 1982, 1983, . . ., 1988. A lag is taken to belong to the interval (0, 1] if the case was reported to the CDC in the same month it was diagnosed or in the following month. If a case isn't logged at the CDC until the second full month following the month of diagnosis, the lag is ascribed to the interval (1,2], and in general a case which is reported in the kth full month following the diagnosis month is assigned to (k − 1,k].

The data utilized for our work are the number of observed lags corresponding to cases diagnosed in 1988 for the intervals (0,1], (1,2], . . . , (29, 30], the number of observed lags corresponding to cases diagnosed in 1987 for the intervals (0,1], (1,2], . . ., (41,42], and the number of observed lags corresponding to

Table 4.—Various Ratios of the Cumulative Distributions of Lags for 1982–88 Indicating the Shapes of the Conditional Distributions for Lags No Greater than 30

Condit. distr.	1982	1983	1984	1985	1986	1987	1988
F(6)/F(30)	0.769	0.834	0.819	0.788	0.721	0.711	0.742
F(12)/F(30)	0.907	0.926	0.925	0.892	0.817	0.849	0.851
F(18)/F(30)	0.965	0.963	0.967	0.936	0.896	0.923	0.922
F(24)/F(30)	0.985	0.984	0.984	0.964	0.961	0.965	0.969

cases diagnosed in 1986 for the intervals (0,1], (1,2], . . . , (53,54]. The counts for intervals up to and including (65,66] for 1985, (77, 78] for 1984, (89,90] for 1983, and (95,96] for 1982 were also used. The data came from the CDC data base as reported through the second quarter of 1991. Consequently, even though some lags for the 1988 cases are reported in intervals up to (41,42], only the figures for the intervals through (29,30] represent complete counts. (For example, a December 1988 diagnosis having a lag greater than 30 months would not have been included in the second quarter 1991 CDC data base.) Thus, here we will only make use of intervals which are complete, and we will not employ projected counts for intervals to which additional cases may eventually be added.

Even though the cumulative distribution for the lags cannot be directly observed, one can learn something about the general shape of the lag distribution by examining the conditional distribution of all observed lags which are less than or equal to a certain value. For instance, of all the lags under consideration which belong to intervals up to (29,30], roughly 60% of them lie in the lowest three intervals. This fact, along with other information about the general shape of the distribution which can be gleaned from table 4, indicates that the distribution of the lags is highly skewed with the distribution mean and median both being relatively small compared to some of the larger lag values contained in the data.

Table 4 displays various values of the cumulative distribution for lags belonging to intervals up to (29,30] for the cohort years 1982–1988. While there are some similarities amongst the displayed values, particularly when values from adjacent years are compared, it is clear that the distribution of lags is not the same for each of the years, since if that were the case then along each row the displayed values would be identical.

Additional insight pertaining to the lag distributions can be obtained by looking at values of other ratios of the cumulative distributions. (Note that since

$$F(m_1)/F(m_2) = \frac{N(m_1)/N(\infty)}{N(m_2)/N(\infty)} = N(m_1)/N(m_2)$$

doesn't depend on the unknown value of $N(\infty)$, one can easily obtain the values of various ratios of the distribution functions even though the function values

Table 5.—Various Ratios of the Cumulative Distributions of Lags for 1982–88

Ratio	1982	1983	1984	1985	1986	1987	1988
F(6)/F(12)	0.848	0.900	0.885	0.883	0.883	0.837	0.872
f(12)/f(18)	0.939	0.962	0.957	0.953	0.912	0.920	0.923
F(18)/F(24)	0.980	0.978	0.983	0.971	0.933	0.956	0.951
F(24)/F(30)	0.985	0.984	0.984	0.964	0.961	0.965	0.969
F(30)/F(36)	0.985	0.986	0.986	0.973	0.978	0.976	
F(36)/F(42)	0.992	0.988	0.984	0.977	0.983	0.985	
F(42)/F(48)	0.991	0.989	0.983	0.986	0.986		
F(48)/F(54)	0.980	0.982	0.984	0.988	0.990		
F(54)/F(60)	0.962	0.987	0.991	0.989			
F(60)/F(66)	0.970	0.979	0.991	0.992			
F(66)/F(72)	0.967	0.990	0.992				
F(72)/F(78)	0.971	0.991	0.994				

cannot be observed.) Table 5 contains the values of certain ratios of the cumulative distributions for the years 1982–1988. One can see that the values along any of the rows are not identical, which indicates that the lag distributions are not the same for all of the years. However, one can note that there is a degree of similarity between row values of adjacent years, and that in some cases groups of three or more values in a row appear to be clustered around a central value.

For each year, the observed pattern of the distribution ratios can, for the most part, be closely matched by fitting a function of the form

$$F(m) = \frac{m + \alpha}{m + \beta}. \tag{4}$$

Functions of the form

$$F(m) = \frac{1}{1 + \alpha/m + \beta/m^2} \tag{5}$$

can also be fit to yield a good degree of agreement with the observed values. It should be noted that the preceding approximation formulas are very compatible with the data in the upper tail of the cohort distributions, but they are not uniformly accurate and tend to be rather loose in their agreement with the observed data in the first dozen or so intervals.

Fitting The Distributions And Estimating Cohort Sizes

The next step in construction an approximation to the cumulative distribution of the lags is to obtain estimates of $N(\infty)$ for each of the cohort years. Such estimates can then be used with the observed counts for the completed intervals to arrive at estimates of the cumulative distributions for each of the cohort years.

For the 1988 data, fits of (4) and (5) were obtained by estimating the unknown

parameter α and β using the method of minimum χ^2. The intervals (0,12], (12,13], (13,14], (14,15], . . . , (29,30] were employed and conditional probabilities were used since we had no data from the extreme upper tail of the distribution. The decision of not breaking the initial interval (0,12] up into smaller intervals was based on a desire to place more emphasis on the trend of observed values in the upper tail of the distribution. This is compatible with the goal of accurately estimating F(30) so that the value of $N(\infty)$ for 1988 can be estimated with N(30)/F(30), where F(30) is the estimate of F(30) obtained using minimum χ^2 estimates of α and β. Since the observed interval counts indicate that (4) and (5) will provide reasonable fits in the upper tail, but not necessarily in the lower portion of the distribution, utilizing the larger interval (0,12] will allow a good approximation to be found even though the fitted function may be somewhat incompatible with the data from the initial monthly counts.

Fits of (4) and (5) were also performed for the 1985, 1986, and 1987 cohorts, in each case using (0,12] as the first interval and then using all of the complete monthly counts. For each of the years 1985–88, fits of (4) and (5) were also done using the interval sets

$$(0, K-18], (K-18, K-17], (K-17, K-16], \cdots, (K-2, K-1], (K-1, K]$$

and

$$(0, K-12], (K-12, K-11], (K-11, K-10], \cdots, (K-2, K-1], (K-1, K],$$

where K is the upper endpoint for the last completed interval. These alternate sets of intervals place increased importance on accurately capturing the trend of the last 18 interval counts and the last 12 interval counts, respectively.

Of all the fitted functions obtained for each cohort year, it is difficult to determine which one is best. One criteria would be to perform a χ^2 goodness-of-fit test on each of the fits for a cohort, and select as the best fit the one which is most compatible with null hypothesis that (4) or (5) is the proper distribution function. However, this scheme is not necessarily ideal since it tends to favor the fits based on fewer intervals, and such fits can be more highly influenced (in a bad way) by quirks in the data.

For each year, the interval counts tend to decrease as one gets further out into the tail of the distribution, but the observed counts do not follow a strictly decreasing pattern. Since it is reasonable to believe that the simple function which will best capture the trend of the cumulative distribution and lead to the best approximation of $N(\infty)$ is a smoothly increasing function consistent with a decreasing sequence of interval counts, and since the fits of (4) and (5) generally

Table 6.—Estimated Number of Diagnosed AIDS Cases which will Eventually be Attributed to 1985–88 (obtained by fitting the data from the various years separately)

Year	$N(\infty)$
1985	12029
1986	20031
1987	29801
1988	37271

imply decreasing interval counts, a method which is more sensitive to quirks in the sequence of observed counts may not be as good as one which focuses on finding a function which provides a reasonable fit over a longer sequence of intervals. It should also be kept in mind that because we are dealing with such large sample sizes in our goodness-of-fit tests, the high power of the testing procedure will cause some strong rejections of the null hypothesis even though from a practical viewpoint the fit coincides with the observed data rather well.

Another way of arriving at a guess as to which fit may be best is to exploit the fact that Table 5 suggests that the cumulative distribution ratios are quite similar for adjacent years. Therefore, since we can observe that for 1987 we have $N(42) \doteq 1.040 \cdot N(30)$ (and noting additionally that for 1986 we have $N(42) \doteq 1.041 \cdot N(30)$), it seems reasonable to require that $F(42)$ be close to $1.040 \cdot N(30)$ for the 1988 data. Thus, the fit which provides an estimate of the 1988 cohort's growth from 30 months to 42 months which is most consistent with the observed growth for the 1987 cohort can, in one sense, be deemed to be the best estimating function for 1988's distribution. The various candidates for 1987 can be judged on how consistently they match 1986's observed growth from 42 months to 54 months, and other years can be handled in a similar manner. In every case it turns out that one of the fits based on the original set of intervals does as good as any fit based on the alternate sets of intervals which place more emphasis on the trend in the extreme upper tail of the distribution.

To arrive at estimates of $N(\infty)$ for the various years, we proceeded as follows. For each year, we found the fit that corresponded to the largest goodness-of-fit test P-value (which is indicative of the highest degree of compatibility with the null hypothesis) and obtained the associated estimate of $N(\infty)$. We then found the fit that most closely matched the observed growth rate for the preceding year and obtained the associated estimate of $N(\infty)$, and then averaged the two estimates to obtain the values displayed in Table 6.

Finding A Universal Fit For Recent Years

The estimated cohort sizes from Table 6 can be combined with the observed interval counts to arrive at estimates of the cumulative distribution functions for

the years 1985–88. Some values for the cohort distributions are shown in Table 7 below.

From Table 7, it can be seen that with the exception of the first eight or nine months the cumulative distributions of the lags appear to be very similar for the 1987 and 1988 cohorts. One can also observe other similarities between portions of the distributions, but in general no strong patterns linking the four estimated distributions seem to exist. Of course, inaccuracies in the estimated values of $N(\infty)$ for the various years could make the estimated distributions appear less similar (or more similar) to one another than is warranted. That being the case, it seems reasonable to see if a single (but perhaps with more than one piece) cumulative distribution approximation function can be found that is consistent with a large portion of the 1985–1988 data. However, it should be kept in mind that the values displayed in Table 7 (which are not based on any estimated parameters) indicate that the cumulative distributions are different for the various years, and so there exists no single formula which will completely agree with all of the observed interval counts.

To find a function which provides a decent fit to both the 1987 and 1988 data, one can first define $F_2(m)$ to be the sum of the N(m) values for 1987 and 1988 divided by the sum of the estimates of $N(\infty)$ for those two years. Then an appropriate function can be fit to the computed values of $F_2(m)$ using regression. It turns out that functions of the form

$$F^*(m) = 1.0/[1.0 + G(m)],$$

where G(m) is of the form

$$\frac{\alpha}{m} + \frac{\beta}{m^2}$$

or

$$\frac{\alpha}{m} + \frac{\beta}{m^2} + \frac{\gamma}{m^3},$$

are capable of providing good fits. One way of fitting such functions is to employ a least squares regression to fit the polynomial G(m) to the values of $[1.0/F_2(m)]$-1.0. Fits of this type were done using both the second degree and third degree versions of G(m), and fits were also done using values of $F_3(m)$ in place of $F_2(m)$, where the values of $F_3(m)$ are obtained by dividing the sum of the N(m) values for 1986, 1987, and 1988 by the sum of the $N(\infty)$ estimates for those three years. To insure a good fit to the tail of the distribution, the fits were done using only the values m = 12, 18, 24, 30, 36, and 42. (For m = 36 and m = 42, 1988

Table 7.—Estimated Cumulative Distributions of Lags for 1985–88 Based on the N(∞) Values Displayed in Table 6

Cum. dist.	1985	1986	1987	1988
F(1)	0.304	0.281	0.201	0.251
F(2)	0.468	0.425	0.373	0.422
F(3)	0.553	0.500	0.484	0.517
F(4)	0.604	0.551	0.541	0.574
F(5)	0.638	0.585	0.585	0.612
F(6)	0.665	0.613	0.618	0.642
F(7)	0.688	0.634	0.645	0.663
F(8)	0.705	0.649	0.668	0.682
F(9)	0.720	0.662	0.687	0.697
F(10)	0.734	0.673	0.708	0.712
F(11)	0.743	0.684	0.723	0.724
F(12)	0.753	0.695	0.738	0.736
F(18)	0.790	0.762	0.802	0.797
F(24)	0.814	0.817	0.839	0.839
F(30)	0.844	0.850	0.870	0.865
F(36)	0.868	0.870	0.891	
F(42)	0.888	0.885	0.904	
F(48)	0.901	0.898		
F(54)	0.912	0.907		
F(60)	0.922			
F(66)	0.929			

values were not available and so the formulas for $F_2(m)$ and $F_3(m)$ had to be adjusted appropriately.)

All things considered, the $F_3(m)$ values seemed to produce the more desirable fits. As expected, the third degree version of G(m) resulted in a nicer pattern of residuals; however, the second degree version of G(m) produced a fit that was actually superior in other aspects of being consistent with the observed data. It turns out that

$$F^*(m) = \frac{1.0}{1.0 + 4.83/m + 1.42/m^2 - 63.6/m^3},$$

which is obtained by averaging the coefficients of the least squares fit of the second and third degree versions G(m) to the values of $F_3(m)$, is an estimate of the cumulative distribution which exhibits remarkable consistencies with the observed data.

Table 8.—Preliminary Estimates of AIDS Cases Which Will Eventually be Attributed to 1985–88 (obtained by fitting the aggregated data from 1986–88)

Year	N(∞)
1985	11994
1986	19797
1987	30049
1988	37411

Table 9.—Estimated Values of the Cumulative Distributions of Lags for 1985–88 Based on the N(∞) Values Displayed in Table 8, With the Estimate F* Obtained from Fits of the 1986–88 Data

Cum. dist.	1985	1986	1987	1988	Estimate
F(6)	0.667	0.620	0.613	0.639	0.645
F(12)	0.755	0.703	0.732	0.733	0.727
F(18)	0.792	0.771	0.796	0.794	0.793
F(24)	0.816	0.827	0.832	0.835	0.834
F(30)	0.847	0.861	0.862	0.862	0.862
F(36)	0.870	0.880	0.883		0.882
F(42)	0.890	0.896	0.897		0.897
F(48)	0.903	0.908			0.909
F(54)	0.915	0.918			0.918
F(60)	0.924				0.925
F(66)	0.932				0.932

The values of N(∞) resulting from F*(m) are given in Table 8. These values are quite similar to the previously obtained ones displayed in Table 6. However, they differ enough from the previous values to yield noticeably different estimates of the cumulative distributions for the 1985–88 cohorts as can be seen by comparing the values from Table 7 with the values in Table 9.

Not only does this estimate of the cumulative distribution produce an increase degree of similarity amongst the four distributions, but it also yields ratio values that are in line with what one might expect from the values displayed in Table 5. This can be observed in Table 10, where the ratio values of the estimated distribution have been added to the information that was contained in Table 5 (and the ratios for 1982 have been dropped).

Even though the estimate just considered provides a more or less adequate fit of the upper tails of the 1986–88 distributions, it isn't all that accurate over the first twelve monthly intervals. To determine a function which provides a good fit to the lower portion of the cumulative distribution one can repeat the proce-

Table 10.—Various Ratios of the Cumulative Distributions of Lags for 1983–88 and of the Estimated Distribution Function

Ratio	1983	1984	1985	1986	1987	1988	Estimate
F(6)/F(12)	0.900	0.885	0.883	0.883	0.837	0.872	0.877
F(12)/F(18)	0.962	0.957	0.953	0.912	0.920	0.923	0.917
F(18)/F(24)	0.978	0.983	0.971	0.933	0.956	0.951	0.950
F(24)/F(30)	0.984	0.984	0.964	0.961	0.965	0.969	0.968
F(30)/F(36)	0.986	0.986	0.973	0.978	0.976		0.977
F(36)/F(42)	0.988	0.984	0.977	0.983	0.985		0.983
F(42)/F(48)	0.989	0.983	0.986	0.986			0.987
F(48)/F(54)	0.982	0.984	0.988	0.990			0.990
F(54)/F(60)	0.987	0.991	0.989				0.992
F(60)/F(66)	0.979	0.991	0.992				0.993
F(66)/F(72)	0.990	0.992					0.994
F(72)/F(78)	0.994						0.995

Table 11.—Comparison of Values of $F_2(M)$ and the Regression-Based Approximation

m	$F_2(m)$	Approximation
2	0.398	0.398
3	0.499	0.502
4	0.556	0.555
5	0.596	0.592
6	0.627	0.623
7	0.651	0.649
8	0.671	0.672
9	0.688	0.692
10	0.705	0.709
11	0.719	0.725

dure employed to fit the upper tail, only now using values of $F_2(m)$ and $F_3(m)$ for $m = 2, 3, 4 \ldots, 10, 11$. (The $m = 1$ values were deleted since including them substantially worsened the quality of the fit. It should also be noted that the $F_2(m)$ and $F_3(m)$ values used here are based on the new $N(\infty)$ values reported in Table 8.) For this part of the distribution, using the $F_2(m)$ values along with the third degree version of $G(m)$ seemed to provide the best fit. Table 11 compares the values of $F_2(m)$ with the following approximation formula obtained from the regression:

$$\frac{1.0}{1.0 + 5.0074/m - 10.3954/m^2 + 12.8785/m^3}.$$

Further Discussion

Upon putting all of the pieces together, we propose the following approximation formula for the cumulative distribution of lags:

$$F(m) = \begin{cases} 0.227 & m = 1 \\ (1.0 + 5.0074/m - 10.3954/m^2 + 12.8785/m^3)^{-1} & 1 < m < 12, \\ (1.0 + 4.83/m + 1.42/m^2 - 63.6/m^3)^{-1} & m \geq 12. \end{cases}$$

Our analysis exploited patterns noticed in the data which allowed us to obtain a good fit for the distribution of lags up to six and a half years with a relatively simple function. This function indicates that the distribution may have a longer tail than was previously thought. In the past, other researchers have focused on obtaining nonparametric estimates of the conditional distribution. Our formula yields results reasonably consistent with the conditional distribution values reported in Table 1 of (Brookmeyer and Liao, 1990), (who used the CDC database of cases reported before 10/1/89), with our sample almost three times as large.

Discrepancies between our results and those of others would seem largely due to the facts that reporting patterns have been changing slightly over time and that our fit is based on a more recent database.

For the diagnosis year 1987, information contained in (Centers for Disease Control, 1989a) can be used to obtain estimates of the cumulative percentages of AIDS cases reported within one month, two months, three months, six months, and one year. Respectively, the estimates are: about 23% or 24%, about 44%, about 56% or 57%, about 71%, and about 81% or 82%. These estimates are in disagreement with our approximation formula and are indicative of smaller $N(\infty)$ values than what we obtained. If the estimates obtained from our fits of the observed data prove to be more accurate, then all previous projections base on the information in (Centers for Disease Control, 1989a) will correspond to underestimates of the true population sizes. It is interesting to note that the estimates implied by (Centers for Disease Control, 1989a) seem to be inconsistent with the CDC data. For example, the value of the ratio F(2)/F(12) can be obtained from the CDC data without using any estimate of $N(\infty)$. From the CDC data we get $F(2)/F(12) = 0.506$, whereas the estimates inferred from (Centers for Disease Control, 1989a) yield a value of about 0.54 for this ratio.

Our schemes for obtaining estimates of the cumulative distribution of lags and values of $N(\infty)$ was based on an assumption that the trends apparent in the observed data would continue to hold in the intervals that future lags will be credited to. Of course, extrapolation is a dangerous practice and one should avoid it if at all possible. However, for this task there doesn't seem to be any good alternative to using data modeling combined with extrapolation. Nevertheless, the following suggestion probably has some merit: create a lag correction formula that can be used to predict the total number of report cases after eight years and then adjust upwards the nonreporting correction factor, which is little more than an educated guess anyway, to account for lags of more than eight years. That is, use

$$C(m) = F(96)/F(m)$$

as a multiplicative lag correction factor.

Here, F(m) is the three part formula given at the beginning of this subsection, and estimates of N(96) are to be obtained by multiplying N(m) by C(m). In conjunction with C(m), one would also employ a multiplicative nonreporting correction factor, γ, having a value somewhere between 1.11 and 1.43, to reflect an additional increase of 11 to 43 percent to account for lags longer than eight years and cases never reported to the CDC. This alteration to our scheme confines most of the uncertainty to the nonreporting correction factor. From the available data, one can certainly see that lags of up to eight years can occur but

Table 12.—Estimates of AIDS Cases for 1982–88, Corrected for Lags and Nonreporting

Year	$N(\infty)$	CDC: 3/92
1982	1346	1090
1983	3712	2924
1984	7559	5946
1985	14273	11159
1986	23559	18194
1987	35759	27159
1988	44521	33161

one has little concrete to work with in trying to come up with an estimate of the proportion of cases that will have a reporting lag of longer than eight years.

In order to get a sense of the changes in the lag distributions and of the growth of the number of AIDS cases, the values of $N(\infty)$ and the corresponding estimates of the cumulative distributions, corrected for lags and nonreporting, were computed for the years 1982–88. It is assumed that $\gamma = 1.25$. The values of $N(\infty)$ obtained are presented in Table 12 and some values of the estimated cumulative distributions are displayed in Table 13. Table 12 also shows how our estimates compare to the figures *reported* by the CDC in April 1992 for AIDS cases through the end of March 1992.

Analysis of Subgroups

If differences in reporting patterns among various subgroups are discovered, then it may be possible to develop more refined estimates of population sizes by analyzing the subgroups separately. (Brookmeyer and Liao, 1990), performed a regression analysis to model the conditional lag distribution as a function of various covariates. They observed some variation in reporting patterns across

Table 13.—Estimated Cumulative Distributions of Lags, Corrected for Nonreporting

Cum. dist.	1984	1985	1986	1987	1988
F(6)	0.590	0.560	0.521	0.515	0.537
F(12)	0.667	0.634	0.591	0.615	0.616
F(18)	0.697	0.666	0.648	0.669	0.668
F(24)	0.709	0.686	0.695	0.699	0.702
F(30)	0.720	0.711	0.723	0.725	0.724
F(36)	0.731	0.731	0.740	0.742	
F(42)	0.742	0.748	0.753	0.754	
F(48)	0.755	0.759	0.763		
F(54)	0.767	0.769	0.771		
F(60)	0.774	0.777			
F(66)	0.781	0.783			

Table 14.—Comparison of the Proportion of Cases Belonging to Key Subgroups

	Hetero	Gays	IVDU	Duals	Others
1986–87	0.031	0.625	0.198	0.074	0.073
1988–89	0.047	0.575	0.238	0.062	0.078
1990–91	0.063	0.543	0.244	0.054	0.095

geographic regions and between the risk groups. In our work, the analysis focuses on reporting delays for both the aggregate population and by risk group, having, of course, the advantage of a total sample nearly three times as large as Brookmeyer and Liao.

Table 14 displays the proportions of all AIDS cases reported for the diagnosis years 1986 and 1987 which belong to the following five subpopulations: heterosexuals, gays, IV drug users, duals, and others. Also given are the corresponding proportions for the diagnosis years 1988 and 1989, and also the ones for all cases attributed to 1990 and 1991 that are known of so far. It can be seen that the proportions of cases belonging to the heterosexual group and the IVDU group are increasing, whereas the gay and dual groups exhibit declines.

Since the subgroup composition of the AIDS population seems to be shifting somewhat, and is anticipated to exhibit more pronounced changes in the future, it is of interest to determine whether or not the reporting tendencies pertaining to the different groups are nearly the same. If the lag distribution or the nonresponse rate for one or more groups is appreciably different from what they are for other groups, then the proposed correction factors will have to be viewed as looser approximations than they would otherwise.

Unfortunately, at the current time there does not appear to be any good way to assess the nonresponse rates accurately for the various subpopulations (or for the aggregate population). However, by examining the distributions of lags some indication of how similar the reporting patterns associated with various group are can be obtained. Table 15 gives some values for the conditional cumulative distribution of lags up to 42 months for the cases attributed to 1986 and 1987.

It appears that the heterosexuals subgroup and the gays subgroup experience very similar behavior in regard to reporting lags, and that the IVDU subgroup has higher proportions of the longer lag times. The distribution for the duals group also shows a pattern of longer lag times than what is observed for the heterosexuals and gays, but the difference isn't as great as it is for the IVDU. It can be seen from Table 14 that if the two groups having longer lag times are combined, then the proportion of cases belonging to the combined group has remained relatively constant. For example, the proportion of cases which are in

Table 15.—Various Ratios of the Cumulative Distributions of Lags (by subgroups, for 1986–87 cases) Indicating the Shapes of the Conditional Distributions for Lags No Greater than 42

Condit. dist.	Hetero	Gays	Duals	IVDU
F(2)/F(42)	0.461	0.465	0.422	0.404
F(4)/F(42)	0.641	0.633	0.588	0.565
F(6)/F(42)	0.721	0.711	0.669	0.640
F(9)/F(42)	0.781	0.778	0.737	0.708
F(12)/F(42)	0.835	0.823	0.791	0.762
F(18)/F(42)	0.896	0.890	0.882	0.843
F(24)/F(42)	0.933	0.933	0.929	0.908
F(30)/F(42)	0.961	0.964	0.964	0.958
F(36)/F(42)	0.987	0.985	0.985	0.982

either the IVDU or duals groups is about 0.300 for 1988–89 and is about 0.298 for 1990–91.

It is also of interest to determine if there is any indication of changes in reporting practices in recent years. In order to assess whether or not there have been any substantial changes in the distributions of reporting lags, one can compare the cumulative distributions for the 1986–87 cases with the cumulative distributions for the 1988–89 cases. Since only the counts for lags up to 18 months are complete for the 1989 cohort, the cumulative distributions displayed in Table 16 and Table 17 are conditional distributions for lags up to 18 months.

The similarity between the lag distributions for the gays and heterosexuals subgroups noted previously is again evident from the values displayed in Table 16. Also, it can be seen that there are no major differences between the distributions for the 1986–87 cases and the 1988–89 cases (except for a slight tendency for a larger proportion of shorter lags for heterosexuals in the more recent cohorts). The values in Table 17 indicate that for the 1986–87 cases there is very little difference between the distribution for the IVDU group and the distribu-

Table 16.—Various Ratios of the Cumulative Distributions of Lags for the Gays and Heterosexuals Subgroups for the Diagnosis Periods 1986–87 and 1988–89

Condit. dist.	Gays '86–'87	Gays '88–'89	Hetero '86–'87	Hetero '88–'89
F(1)/F(18)	0.315	0.310	0.272	0.294
F(2)/F(18)	0.522	0.522	0.515	0.530
F(3)/F(18)	0.642	0.640	0.655	0.668
F(6)/F(18)	0.800	0.795	0.805	0.815
F(9)/F(18)	0.874	0.870	0.873	0.892
F(12)/F(18)	0.925	0.925	0.932	0.938
F(15)/F(18)	0.968	0.970	0.971	0.974

Table 17.—Various Ratios of the Cumulative Distributions of Lags for the IVDU and Duals Subgroups for the Diagnosis Periods 1986–87 and 1988–89

Condit. dist.	IVDU '86–'87	IVDU '88–'89	Duals '86–'87	Duals '88–'89
F(1)/F(18)	0.282	0.285	0.299	0.299
F(2)/F(18)	0.480	0.503	0.479	0.510
F(3)/F(18)	0.601	0.632	0.602	0.632
F(6)/F(18)	0.759	0.793	0.759	0.798
F(9)/F(18)	0.840	0.873	0.836	0.880
F(12)/F(18)	0.904	0.926	0.897	0.937
F(15)/F(18)	0.957	0.972	0.954	0.974

tion for the duals group, and these two distributions are very similar for the 1988–89 cases as well. However, for both of these subgroups the distribution for the 1988–89 cases is somewhat different than it is for the 1986–87 cases.

The distributions for 1988–89 are indicative of smaller proportions of the longer lag times, and in fact for the 1988–89 cases the distributions for all four subgroups are somewhat similar. One can summarize the results by noting that for the 1986–87 cases the lags for the IVDU and duals groups are somewhat longer than they are for the gays and heterosexuals subgroups, and for the 1988–89 cases the distributions for the gays and heterosexuals are similar to the distributions for the 1986–87 data, but that the distributions for the IVDU and duals groups have changed to become more similar to the distributions for the other two groups. These observations can perhaps be better seen in Table 18 where the distribution for the combined IVDU/duals group for 1986–87 cases is compared to the distribution for the combined gays/heterosexuals group for 1988–89 cases and the distribution for the combined gays/heterosexuals group (based on the data for the four year period 1986–89). We would conclude that, all in all, there is no strong evidence to indicate that separate lag correction formulas are needed for the various subgroups.

Table 18.—Ratios of the Cumulative Distributions of Lags for Various Combined Data Groups Indicating a Tendency Towards an Increased Degree of Homogeneity

Condit. dist.	IVDU/Duals '86–'87	IVDU/Duals '88–'89	Gay/Hetero '86–'89
F(1)/F(18)	0.287	0.288	0.310
F(2)/F(18)	0.479	0.504	0.522
F(3)/F(18)	0.601	0.632	0.642
F(6)/F(18)	0.759	0.794	0.798
F(9)/F(18)	0.839	0.874	0.873
F(12)/F(18)	0.902	0.928	0.926
F(15)/F(18)	0.956	0.972	0.970

Resolution Of Nonresponse

Every assessment of the CDC AIDS surveillance dataset recognizes the problem of nonresponse, whether from deliberate avoidance or systemic deficiencies in capturing 100% of the AIDS cases diagnosed. Various recent reports have enumerated social and career pressures which mitigate against candor in reporting AIDS by patients, by healthcare workers, and by institutions (National Commission on Acquired Immune Deficiency Syndrome, 1990; New York Times, 1990a; New York Times, 1989b; New York Times; 1990b; Washington Post, 1990).

The General Accounting Office (General Accounting Office, 1989), pp. 37–38) estimated that one-third of AIDS cases may not be reported (an implied nonresponse correction factor of 1.5). CDC's own estimates have ranged from "10% to 30%," a factor range of 1.11 to 1.43. A report on South Carolina's AIDS cases (Conway, 1989), comparing reported numbers vs. actual medical records, found only 59.5% of cases being reported, a factor of 1.68. A Chicago study (New York Times, 1989a) found that mid-America was greatly underreported on AIDS cases, particularly among affluent whites.

If you do not account for nonresponse in estimating HIV populations via CDC reported data, you implicitly set the value at 0%, i.e., a factor of 1.0. To date, we have used a factor in our work of 1.25, which is approximately at the center of CDC's "10% to 30%" assessment.

Concluding Remarks

Our results thus far suggest strongly that the HIV/AIDS epidemic will be more extensive and of greater duration than the public expects. The scope and persistence of the epidemic (worldwide and decades-long) has important implications for both the long-range and operational planning of our healthcare systems and related mechanisms of social support.

One of the more important results of the continual underestimation of the stream of AIDS cases is the concomitant underestimation of the incidence and prevalence of the HIV infection itself. Indeed, we believe that the public continues to be misled regarding the true scope of the epidemic. We take special exception to the oft-repeated quotation that there are 1 million people who have ever been HIV seropositive. We feel that the actual figure is considerably higher (possibly by an order of magnitude) and is still increasing.

But just how large is the pre-AIDS HIV-infected population? One million cases as of July 1989 is a widely cited estimate (embraced by the Centers for

Disease Control as a result of a workshop conducted October/November 1989, with subsequent publication in (Centers for Disease Control, 1990c). The 1 million quantity has obviously never been tallied—it is a statistical estimate with potential likely error in either an up- or downward direction. However, it has neither been revised since then nor subjected to subsequent re-estimation. There have been some minor differences over this figure (our own pre-AIDS estimate is 1.3 million (Harris et al., 1992); but the issue now is the repetitious use of a 1 million HIV population size since 1989, including its use at the International AIDS Conference in Amsterdam in July 1992.

Exactly what does "1 million HIV cases" mean? The CDC reported in January 1992 (Centers for Disease Control, 1992a), "Of the estimated 1 million HIV-infected persons in the United States, approximately 20% have developed AIDS." That is, the pre-AIDS HIV-infected population would have truly been about 800,000, when the highly publicized 200,000 reported AIDS cases, announced in early 1992, are subtracted. In an earlier report by CDC (Centers for Disease Control, 1990b) (see Table 1, page 111), total U.S. HIV prevalence was estimated as 750,000 (January 1986) and 1 million (June 1989); these figures excluded AIDS deaths. That report also stated CDC's opinion that 15% of all diagnosed AIDS cases are never reported to CDC.

We believe that recent reports in the scientific literature and public media regarding the possibility of a sharp decline in the incidence of HIV seropositivity are premature and may cause ill-timed erosion of public efforts to combat the epidemic. Furthermore, unanticipated impacts of HIV/AIDS on this nation's healthcare system through the remainder of the 1990s could well compromise our ability to deal with a broad range of competing public health problems. One of our primary concerns is recent work published by (Brookmeyer, 1991), which has been widely quoted in media.

In that paper, Brookmeyer claimed that there was a precipitous drop in HIV incidence to less than 25,000 infections in 1989. We strongly disagree with this estimate and the purported downward trend on two grounds. First, the drop is contradicted by published evidence and its use could lead to erroneous policy decisions. Second, we seriously question the validity of his application of a specific curve-fitting technique to a problem of forecasting future outcomes.

Separate analyses and modeling efforts performed at George Mason University (Harris et al., 1992) and at the Department of Veterans Affairs (Salzberg et al., 1991; Salzberg and MacRae, Jr., 1992) have both come to a contrasting conclusion regarding the probable path of the epidemic through 1995 and beyond. While we do concur with the emerging understanding that the rate of growth of the seropositive population has dropped sharply, we feel that there is significant evidence that the most optimistic estimates of a drop in the actual

numbers of newly infected HIV cases are too extreme and subject to misinterpretation. We must emphasize and repeat our conclusion that two seemingly contradictory factors both characterize the HIV/AIDS database simultaneously: (1) a decrease in the rate of new infections each year since 1977, and (2) an increase in the incidence of new infections each year since the start of the pandemic in 1976.

Outcomes of both the GMU and Salzberg HIV/AIDS modeling programs are in close agreement: while the rate of HIV growth is indeed likely decreasing, the number of new infections will almost certainly exceed 120,000 in 1991 and could well be as high as 280,000. Figures 1 and 2 depict the divergence between our projections of HIV prevalence and incidence and those of Brookmeyer. How can this difference be explained?

First, consider that an extrapolation of military data on HIV incidence in the general population yields an estimate of 40,000 new infections in 1989 (Salzberg et al., 1991). Since the military actively discourages homosexuals and IVDUs from service, this would thus represent a lower limit for the general population, which is already greater than Brookmeyer's estimate (at least 40,000 vs. 25,000).

Second, consider the following. There is some agreement among researchers that the HIV seropositive population was in the range 600,000 to 750,000 at end-1985 (CDC: 750,000; Brookmeyer, *Science:* 715,000; Salzberg et al.: 550,000; GMU: 588,000). It would, therefore, not seem unreasonable to assume that the total HIV population was approximately 1 million at start-1989. Thus, Brookmeyer's 25,000 new HIV cases represents an annual rate of 2.5 new infections per 100 existing cases. Furthermore, in order to maintain a stable population of about 1.5 million cases in 1991, the rate of new HIV infections would have to be enough to replace the number of HIV/AIDS deaths in the year, estimated currently by the CDC to be over 30,000. This represents approximately a 2% spread rate for the HIV seroprevalent population. Even if the total HIV/AIDS population had remained at 1 million, the spread rate for stability would be only 3%. However, we believe that these rates are too low and too great a departure from the rates recognized for recent years; in contrast, Harris, Rattner, and Sutton claim an estimated rate of 19% for 1989 and 15% for 1991, while the figure for Salzberg et al. is 16% for 1989.

Our respective analyses suggest that more likely profiles for new HIV carriers for the years 1986–91 would look as indicated in Figure 2, for example, giving a range of 142,000 to 258,000 in 1989 (where the lower value is based on a "highly optimistic" scenario by Salzberg). These would be compared to Brookmeyer's approximate figure of 25,000 for the four quarters of 1989.

Note, however, that the estimation of the pre-AIDS HIV population in recent years is complicated by the problem of measuring the impact of intervention

therapies, principally AZT, to moderate the rate of transition of pre-AIDS cases into AIDS. This contrasts with the previous natural history of the pace or progression of the disease. It is true that new therapies that have been administered in recent years to pre-AIDS patients have succeeded in lengthening the average incubation time-to-AIDS. Similarly, treatments provided to many AIDS patients have lengthened the average lifespan from less than 15 months to two years.

Clearly, there are no cures yet; it is hoped that these therapies may buy time needed to await the development of future, more effective treatments. This desirable interim situation in the progress of the epidemic has had the effect of slowing the rate at which the pre-AIDS population transitions into AIDS, by exhibiting one of the multiple indicator diseases. This lesser rate, as of now, means that the patient will live some months longer, after AIDS-onset, under a slower paced progression of the disease.

In order to combat the HIV/AIDS epidemic effectively, the bio-medical community must learn to know the enemy better. The search for practical treatments and cures requires the utmost of creative research and challenges our knowledge of the most fundamental properties of the behavior of this difficult virus. In order to respond to the health-care needs of the already infected, we must also come to grips with a more realistic assessment of the breadth and pace of the epidemic. If indeed the level of the epidemic is at or near the level shown in our analysis, then the degree of resources required will be far beyond what any governmental agency is currently prepared to support.

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Joseph Henry's 1842 and 1843 Out-of-Doors Electrical Transmission Signal Experiments

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ABSTRACT

Ever since G. Marconi first perfected his radio transmission apparatus in 1896, there has been controversy[a] as to whether Prof. Joseph Henry had transmitted and detected wireless electrical signals at a distance in 1842 and 1843. Evidence is presented to show that these signals were produced by a transient electromagnetic field alternating at about 3.5×10^5 Hz (i.e., cycles per second).

I discuss both the unpublished recorded and published evidence of Prof. Henry's out-of-doors electrical signal transmission experiments of October 1842 and October 1843. Mention is made of the destruction by fire of his records in January of 1865, and of the later published remarks about Henry's experiments made by Ernest B. Rutherford in 1894. I discuss the wiring of Henry's primary and secondary circuits, and the construction of his magnetic coil signal detector that he attached into the loop of the secondary wire.

1. *Introduction.* (a) Recorded and published evidence of signal transmission. On October 6, 7, and 8, 1842, Prof. Joseph Henry of the College of New Jersey (now Princeton University) conducted a series of outdoors electrical ex-

[a] Refer to:

(1) J. S. Ames, **The Discovery of Induced Electric Currents.** vil. I. Memoirs by Joseph Henry, American Book Company, New York, Cincinnati, Chicago. (LC: QC631.A51) See p. 107 for pertinent remarks:
(2) J. S. Ames, *Certain Aspects of Henry's Experiments on Electromagnetic Induction,* Science, 75, no. 1934 (Friday, January 22, 1932) pp. 87–92. See especially page 91.
(3) W. F. Magie, *Joseph Henry,* Reviews of Modern Physics, vol. 3 (October 1931) pp. 465–495.

In a speech at a meeting of the American Institute of Electrical Engineers arranged in his honor at the old Waldorf-Astoria hotel in New York City on January 13, 1902, G. Marconi said:

"I have built very largely on the work of others, and before concluding I would like to mention a few names. I may miss a few of them, but I would like to mention Clerk Maxwell, Lord Kelvin, Professor Henry and Professor Hertz."

This quotation was taken from page 116 of **Marconi: The Man and His Wireless** by O. E. Dunlap, Arno Press and The New York Times (1971) New York; L.C. call no. TK 5739.M3 D8 1971.

periments on the grounds of what was then called the "back campus" behind Nassau Hall. In his laboratory notebook for the date October 7, he recorded separation of his primary and secondary circuits at a distance of 165 feet, after having made successive increases in distance in the course of these experiments. Furthermore, he recorded in the final three experiments on Oct. 8, that

> "the whole parallelogram formed by the secondary wire, was carried backward, so that the farthest side was in the field beyond the Society halls" [1 and 7].

It is inferred from this remark in his record of experiments, and from the later oral communication made at the October 21 meeting of the American Philosophical Society, that the distance between the primary wire and secondary wire in this final experiment of October 8, 1842, was more than 220 feet. The entry recording this experimental result is:

> "Prof. Henry . . . had succeeded in magnetizing needles by the secondary current in a wire more than two hundred and twenty feet distant from the wire through which the primary current was passing, excited by a single spark from an electrical machine." [2]

Exactly one year later, on October 16th through the 19th, 1843, Prof. Henry renewed his electrical signal transmission experiments. This series of experiments was made on the lawn of the front campus.

(b) Destruction of recorded evidence of signal transmission. On the bitter cold day of January 24, 1865, a fire in the old red brick Smithsonian building on the Mall in the city of Washington (caused by some workmen making repairs on the building), destroyed

> "a large number of papers and scientific notes of the Secretary [Prof. Henry]; [and] a series of diaries. . . . This [was a] truly 'irreparable loss' of original notes of many series of experiments . . . running back for thirty years, . . . and of which but few had been published even by results". [3, 4.i]

H. C. Cameron, who was a student of Prof. Henry, and later, himself a Professor at Princeton University, in his later years verified this loss with his remembrance [4.ii] that

> ". . . the record of those experiments perished in the flames when a portion of the Smithsonian building was burned. . . ."

In addition, Henry himself wrote in 1849 in a letter to S. B. Dod and quoted by Dod in his "Discourse" [4.vii]

> "Since my removal to Princeton [in November of 1832 from Albany, New York] I have made several thousand original investigations on electricity, magnetism, and electro-magnetism, bearing on practical applications of electricity, brief minutes of which fill several hundred folio pages. They cost me years of labor and much expense."

Unfortunately, because these records of his experiments no longer exist, we lack the quantative details as well as a more definite description of the layouts (especially with regard to the insulation of the secondary circuits) of these electrical experiments; and an analysis of this lack of detail will be made.

(c) Analysis of existing publications and records of signal transmission. I propose to examine the remaining evidence left to us by Prof. Henry that he had recorded in his laboratory note book [1 & 7] in October of 1842 and October of 1843, in which he described the establishment of alternating currents in a secondary circuit wire in out-of-doors experiments. The existing record will be examined (along with information gleaned from maps[b] of the grounds of the old College campus in order to estimate the approximate locations of his wire arrangements). This visualization will be helpful in understanding and in determining whether he transmitted and detected (by observation of the magnetic state of a steel needle) the inductive effect of electromagnetic waves on the wire conductor, or whether he detected the effect of a ground current and ground return. The written evidence is clear for all to see that Henry hypothesized that electric effects could be propagated as a wave in an elastic aether, and further, that he called distant electric effects dynamic induction-at-a-distance because they occurred during the passage of time. I add at this point, that Michael Faraday went a step further than Henry in Faraday's brief Note **Thoughts on Ray-vibrations** (in 1846), when he hypothesized that an electric aether was not necessary to explain the transmittal of electromagnetic action.

Faraday's comments which set him apart from Henry on this Philosophical interpretation are recorded in Appendix I.

Henry had experimented with inductive effects at a distance from lightning as early as the year 1840. In the experiments examined in this paper, he used the same wire coil that he had used in his "Induction from a thunder cloud" experiments of June 10, 1842. In the experiments of October 1843, that are also examined in this treatise, he used the same coil as well as five other ones individually as part of his detector. During his experiments of June 2, 1842, at his study at home, he observed inductive effects from lightning at a distance of 20 miles, a

[b] These maps are:

(1) *Fire Map of Mercer county* (1890) page 88B. Scarlett & Scarlett, Newark, N.J., LC: G1258.M4S4 1890 folio.
(2) *Facilities Survey Map of the old Princeton University campus (circa 1904),* Office of the Vice President for Facilities, The MacMillan Building, Princeton University. This map was donated to The Joseph Henry Papers of the Smithsonian Institution. Washington, D.C.
(3) *Atlas of the City of Trenton and borough of Princeton, N.J.* (1905) Plate 22, A. H. Mueller & Co., 530 Locust St., Philadelphia, LC: G1259.T7L3 1905 folio.
(4) *The Campus of Princeton University from the Northwest* (1952) Princeton University Press, LC: G3814.P9 1952.S5.

distance which he could determine by comparing the time of the flash to the time he heard the thunder peale.

Many years afterward, in his publication of 1894, Ernest B. Rutherford [5] published the following discussion concerning the magnetization of steel needles under high frequency discharges; and in this discussion, he acknowledged Prof. Henry's prior studies on the magnetization of steel needles. After citing the researches of J. J. Thomson,[c] J. Trowbridge,[d] and Andrew Gray,[e] appertaining to magnetization of iron from high frequency discharges, he wrote:

> "In order to investigate the effect of 'magnetic penetration' in iron for fields varying very much more rapidly than could be obtained with the use of the 'time apparatus', the readiest means to hand for obtaining a very rapid oscillatory current was the ordinary leyden-jar discharge."
>
> "The subject of the magnetization of iron in these fields has been very little touched upon since the time that Henry experimented on the effect of leyden-jar discharges on the magnetization of steel needles."
>
> "In the experiments that follow it will be shown that iron is strongly magnetic in rapidly-varying fields, even when the frequency is over 100,000,000 per second."

In an 1896/1897 paper [6], Rutherford wrote in his introductory remarks that:

> "THE present paper deals with the subject of the magnetization of iron by high-frequency discharges, and the uses of magnetized steel needles for detecting and measuring currents of very great rapidity of alternation."
>
> "It will be shown that these magnetic detectors offer a very simple means of investigating many of the phenomena connected with high-frequency discharges, and may be used in ordinary Leyden jar circuits, but they also offer a sensitive means of detecting electrical radiation from Hertzian vibrators at long distances from the vibrator."
>
> . . .
>
> "The magnetization of steel needles when placed in a spiral through which a Leyden jar discharge was passed has long been known."
>
> "In 1842 Professor HENRY was led to suspect from the anomalous magnetization of steel needles that the Leyden jar discharge was oscillatory."[f]

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[c] *Recent Researches,* p. 322; and *Philosophical Magazine* (1891) p. 457.

[d] 'Damping of Electric Oscillations', *Philosophical Magazine,* (December 1891).

[e] First edition: **The Theory and Practice of Absolute Measurements in Electricity and Magnetism** (1884) 2 volumes. Second edition: **Absolute Measurements in Electricity and Magnetism** (1889) Macmillan and Co., London & New York.

[f] These particular experiments done by Henry were antecedent to his out-of-doors transmission (i.e., electrical signal) experiments described in this paper. For particular information about the works of Abria and several others on steel needles, refer to A. G. Gluckman, *A brief Overview of the Historical Progress of Joseph Henry's Studies concerning Alternating-Current Phenomena,* Research Notes and Memoranda of Applied Geometry for Prevenient Natural Philosophy, POST-RAAG Reports, No. 199 (March 1986) pp. 1–55. (LC: Q1.R45 no. 199). In Europe, a copy may be read at the Science Museum library at South Kensington, London, or else at the library of the British Museum on Great Russell street, London.

See also: A. G. Gluckman, **The Discovery of Oscillatory Electric Current,** Journal of the Washington Academy of Sciences, **80,** no. 1 (March 1990) pp. 16–25; **Corrigenda,** J.W.A.S., **80,** no. 4 (December 1990) p. 187; and **Corrigendum,** J.W.A.S., **81,** no. 1 (March 1991) p. 43.

The Experiments of October 1842

2. ***What is known about the preparation of the primary circuit arrangement (for the transmission experiments) by Prof. Henry on October 6, 1842.*** **(a) The wiring of the primary and secondary (i.e., 1^0 and 2^0) circuits.** The principle source of the descriptions of these induction-at-a-distance experiments (that is, induction of a current in a wire) by Prof. Henry is chronicled in his own handwriting in his surviving laboratory records. [1] Diagram 1 shows the approximate layout of the wire arrangements for Henry's 1842 experiments in the old back campus. The layouts of the parallelogram-shaped circuits are not shown because of the lack of positional information.

A few days prior to October 6th, Prof. Henry arranged as his primary, a copper wire (400 feet in length and $\frac{1}{20}$th of an inch in diameter) to snake its way through the windows of (1) Philosophical Hall (since demolished), and from there into (2) Nassau Hall, and into (3) his residence where Prof. Henry had his study. At the windows of these buildings, the copper wire was supported by using insulating silk ribbon that was fastened to the sides of these windows.

> "I made an arrangement a few days since of a long wire, extending from the Electrical machine in philos. Hall, to my study on the opposite side of the campus. The wire passed diagonally across the large lecture room to the south west window facing the library and thence to the southern most window of the two upper ones of the East end of the college, to the door of my study."

The locations of these windows are depicted in diagrams 1 and 2 of the old campus.

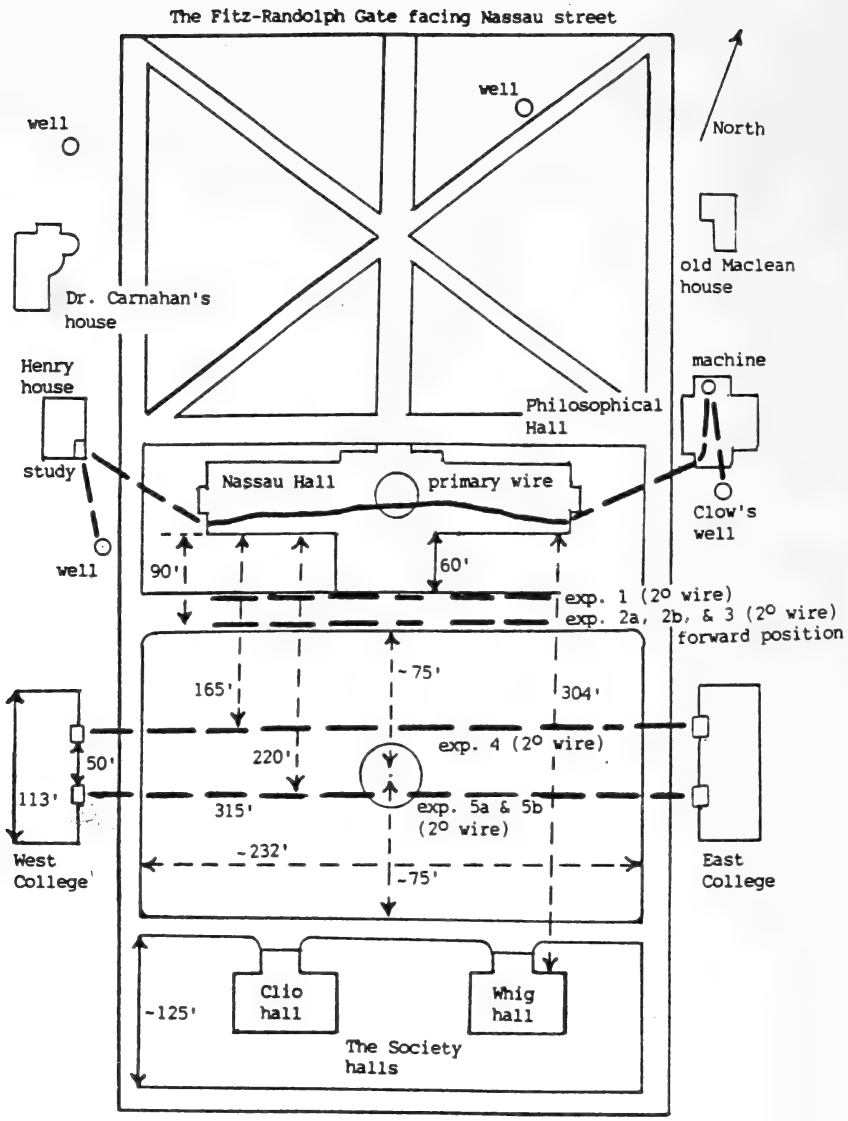
On the morning of October 6, the end of the wire next to Prof. Henry's study, was grounded by means of a connection to another copper wire that was already attached in the well just outside his study. This attachment in the well was already there from experiments with atmospheric electricity made about two years earlier.

> "This morning I completed a current with this wire and the ground by plunging the end next to my study into the well, or rather by connecting it with the wire which is already in the well for the experiments on atmospheric elec., . . ."

Thus, the other end of the wire was attached to a length of copper wire which was securely connected (and grounded) into Mr. Clow's well, and weighted with lead. Earlier, on June 10, 1842, Henry [1 & 7] described the placement of the wire connection in the water well next to his house.

> "I . . . passed the lower extremity [of the bell size[g]] copper wire] into the

[g] Copper wire of the size used to ring church bells, hence the name bell wire, which was 0.05 of an inch in diameter (American manufacturing standard size 16). Modern American gauge size 16 is 0.0508 inches in diameter.



Legend

55' assumed from mid-door to mid-door of the East and West College buildings

the helix in the 2° wire had 45 turns in each of 6 layers (strata)

experiments 1 , 2a, 4, 5a, had 3 charged jars in 1° circuit as the current source

experiments 2b and 5b had 1 jar in the 1° circuit as the current source

experiment 3 used a single spark thrown from the electrostatic machine as the current source

Diagram 1. Sketch of the campus of the College of New Jersey at Princeton, circa 1842, showing approximate locations of wiring

water of the well. This was effected by fastening a cylinder of lead to the end of the wire, and passing this through a hole in the cover[h] near the [hand] pump of the well."

Prof. Henry recorded on October 6th that his primary circuit with ground return via the connections in the two water wells, transmitted a test current that tested the wire connections. He described this experimental demonstration in the following [1 & 7] words:

"Whence small galvanometer of fine wire was placed in the circuit in my study and a small electrometer, consisting of a plate of zinc of about a tenth of an inch in width and the end of the wire ($\frac{1}{20}$[?th?] an inch) for a negative element, the needle [of the galvanometer] was deflected showing that this small galvanic [d.c.] arrangement was sufficient to send a current through [?left blank by Henry?] feet of wire and [?left blank by Henry?] feet of earth."

In his laboratory note book record [1] for that day he wrote:

"Two poles supported by tripods formed of long slats of boards, were placed upright in the back campus, and over the tops of these a part of a wire was stretched parallel to the wire, through the old college, and of a length equal to the whole breadth of the campus. This wire was continued backward on each side, until it extended to the two halls, it then crossed over with the two ends united, so as to form a complete parallelogram."

(b) A magnetometer for measuring the strength of magnetism and polarity due to the transmitted signal, described by Prof. Henry on June 2nd 1842. [1 and 7; pp. 275–276] **Method given by Prof. Henry for using his magnetometer.** On June 2nd of 1842, Henry also described the way in which his magnetometer instrument in the laboratory was used to measure the magnitude (or equivalently, degree or intensity) of magnetization of a steel needle, and its polarity as follows:

"I have mentioned that I had prepared[?] a new magnetometer, and since I have referred to it several times, yesterday, to day it will be best before going farther to describe the instrument. It is on the same principle as the instrument described [on] page [?left blank?]. The index needle is formed of a slender sewing needle 2 inches long, and balanced by a piece of wood at the larger end, (Thus see margin) and suspended by a fine silk filament in a paper stirrup[?]. The suspension string is at right angles to the plane of a graduated circle, and this is covered by a piece of mica cut out at one corner, so that the needle to be experimented on may [be] approached sufficiently near the end of the index needle. The sides of the figure are enclosed by glass. The oscillations of the needle are stopped by the glass plate, which is placed directly across the zero point. The repulsion of the needles drives the index from its point of rest, and the extreme dynamic deflection gives the magnetic force required. The for[c]e in this case is the *vis viva.* The operation of this instrument was very satisfactory."

[h] The usually wooden cover used to prevent leaves and debris from falling into the well.

In the field, Prof. Henry may have simply placed a magnetic compass near the magnetized needle. As per his June atmospheric experiment on dynamic induction from lightning, the needle point may have been stuck into a cork stopper and placed into a glass tube around which the coil wire could be formed. The coil wire could have been "commercial cork wire".

(c) Description of the receiver coil in the secondary circuit. In his first experiment on October 7th, he explicitly wrote [1 & 7] that he used

> ". . . the needle placed in the helix used in the study for atmospheric electricity"

Table 1.—Description of the Induction-at-a-Distance Experiments Performed on October 7, 1842 [1 and 7]

Experiment number: 1
Source: 3 jars of the French battery
Distance between 1^0 and 2^0 circuits: 60 feet between the closest parallel wires of the two circuits[(i)]
Descriptive remarks: "Two poles[(j)] supported by tripods formed of long slats of boards, were placed upright in the back campus". These poles supported a part of the 2^0 wire stretched over their tops and parallel to the 1^0 wire in Nassau Hall. This wire was continued backward on each side, until it extended to the two [Society] halls, it then crossed over with the two ends united, so as to form a "complete parallelogram". "A needle placed in a helix[(k)] connected with the secondary wire was strongly magnetized".

Experiment number: 2a
Source: A battery of 3 Leyden jars that were charged using his machine. A single discharge of the battery was made to the 1^0 wire that was grounded in a well.
Distance between 1^0 and 2^0 circuits: 90 feet between the closest parallel wires of the two circuits
Descriptive remarks: This experiment was repeated several times using a single needle each time. Did Henry use an intermediary calibration jar (i.e., his unit flask) of the Lane type to determine the quantity of charge? The "needles were magnetized to a degree scarcely less than" experiment 1.

Experiment number: 2b
Source: 1 jar, same wire arrangement as experiment 2a.
Distance between 1^0 and 2^0 circuits: 90 feet between the closest parallel wires of the two circuits
Descriptive remarks: The experiment was done once only, and the needle in the helix was "strongly magnetic".

Experiment number: 3
Source: "The jar was removed, and [using the machine] a single spark [was] thrown on to the suspended end of the conducting wire, while the other end was connected with the rubber".
Distance between 1^0 and 2^0 circuits: 90 feet between the closest parallel wires of the two circuits.
Descriptive remarks: Same wire arrangement as experiment 2b. "The needle with this [arrangement] was also magnetic but apparently not quite as strongly as before."

Experiment number: 4
Source: 3 jars gapped to the primary circuit
Distance between the 1^0 and 2^0 circuits: 165 feet between the 1^0 circuit wire in Nassau Hall and the parallel part of the 2^0 circuit wire "stretched between the two upper windows" above the doors closest to Nassau Hall of the entries of the "parallel colleges" (see diagram 1)
Descriptive remarks: The 2^0 "wire was removed from the long poles, and the parallel part stretched [across the breadth of the back campus] between the two upper windows" of the two parallel colleges that are over the entry doors closest to Nassau Hall.

[i] The terms 1^0 and 2^0 mean primary and secondary respectively.
[j] The height of the poles is not mentioned in Henry's surviving notes.
[k] Arago suggested the use of the wire helix for magnetization to Felix Savary in the 1820s. Savary was a Professor at the École Polytechnique in Paris, France. Henry had studied the researchers of Savary, and adopted the use of the magnetizing wire helix for his own subsequent experiments as a detector device for these electrical oscillating pulses.

from his studies on dynamic induction from lightning discharge. The coil that he used was described on June 10th. It was [1 & 7]

> "a compound spiral . . . [which] was formed of 6 strata [layers] of wire, each consisting of 45 spires [turns], and insulated by cement."

I interpret from an earlier reference in his laboratory notes of the point and eye of a needle, that he continued to use an ordinary steel sewing needle (American manufacturing specification size no. 5) in the same coil that he used as a detector in his atmospheric experiments of June 10, 1842, on dynamic induction from lightning. In fact, on Monday, May 16, 1842, Prof. Henry wrote that he used

> "a medium sized sewing needle"

for magnetization in a spiral that day. He also wrote about his use of a

> "slender sewing needle 2 inches long"

in his laboratory note of June 2, 1842, as reported above in section **2(b).** The helix and needle together comprised a detector for the signal. The degree and polarity of magnetization of the steel needle could be determined either roughly with a pocket compass, or with the magnetometer device that is described in section **2(b).**

3. Critique of the transmission experiments made on October 7th and 8th, 1842. There is no extant description of any insulation measures that may have been taken for the 400 foot length of copper wire through Nassau Hall, other than the description of using silk ribbon at the windows of all three buildings to support the wire and prevent it from being grounded by touching the masonry at the window locations.

In his June 10th, 1842 atmospheric experiments on lightning discharge, Henry stuck a needle into a cork stopper which he then placed into his vertically positioned helix. He used the compound spiral coil that is described above in section 2(c), which was interconnected into the grounded circuit of his June 10th lightning experiments. The wire coil (possibly of fine "commercial cork wire") could have been threaded around a glass tube (the wire loops being without question insulated by cement), in the manner he reported in his laboratory notes, and which is described above in section 2(c) of this paper. Henry inter-connected this coil into the secondary circuit in these experiments of October 7 and 8, of 1842.

In charging the flasks that he used in these experiments, Prof. Henry used a hand cranked grounded friction machine. In his method of charging the Leyden jar capacitor (i.e., condenser), he would count the sparks. The friction machine

using a rubber is but one kind of electrostatic machine or generator. The other kind of electrostatic machine is the induction machine, such as the Holtz machine or the Wimshurst machine. A modern kind of electrostatic machine is the Van de Graaff generator. The Wimshurst machine might still be handily used today in certain applications.

The degree and polarity of magnetization of the steel needles used in these experiments would be tested at some time during or after the experiment on a magnetically responsive torsion balance device modelled after the Coulomb torsion balance device (See section **2b** above). He would note the direction of the turning of the recording needle of the torsion device (i.e., its direction (+ or −) and degree of deflection/torsion). The magnitude of the swing of the recording needle indicated the strength/intensity of the current. Another simple measure which was often adopted was to use the magnetic compass as Ørsted had done, with the compass held or laid near to a galvanic wire. The alternating current in these out-of-doors dynamic induction-at-a-distance experiments was transient. A travelling pulse of oscillations was sent along the primary wire.

Prof. Henry remarked that

> "The needles in all the above mentioned experiments [shown here as experiments 1, 2a, 2b, 3, & 4] were magnetized in the same direction . . ."

In experiments 5a and 5b, the forward parallel line of the secondary wire was moved farther away from Nassau Hall to the distance of 220 feet. It was stretched between the two upper windows of

> "the farther entry of the parallel buildings. . . . The needle placed in the helix [in the previous experiments] . . . was again magnetic, and in the same direction as in" [ref. 1; p. 8, & ref. 7; p. 100]

experiments 1, 2a, 2b, 3, and 4. Refer to diagram 1 for a depiction of the layout of the wire. This primary circuit had ground return through the dry soil consisting of compact shale, according to J. H. Lefroy, who in his letter of October 25, 1842 to Edward Sabine [8], stated that the distance of the helix in the secondary circuit (containing the needle to be magnetized) was set at about 600 feet. See also Lefroy's diary, page 173. There is no indication as to whether the helix was located in the forefront part or the rearward part of the parallelogram shaped wire circuit. There is no existing description here of the construction of any wooden support poles or any existing further information about insulation of the secondary wire circuit at this distance. Henry recorded that the weather on October 8, 1842, was damp, and that

> "In the morning a very heavy fog rested on the ground—until about 10 oclock AM." [ref. 1 and 7]

Table 2.—Description of the Induction-at-a-Distance Experiments Performed on October 8, 1842 (a Damp Day)

Experiment number: 5a
Source: 3 jars
Distance between 1^0 and 2^0 circuits: 220 feet = 165′ + 50′, which is the total distance between the 1^0 circuit wire in Nassau Hall and the parallel part of the 2^0 circuit wire.
Descriptive remarks: Secondary wire placed across the breadth of the back campus (i.e., the old campus behind Nassau Hall), at the distance from the farthest doors of the parallel colleges to Nassau Hall. The wire was supported at the windows above these entry doors, and was stretched between the two upper windows of "the farther entry of the parallel buildings". (See diagram 1).

Experiment number: 5b
Source: 1 jar
Distance between 1^0 and 2^0 circuits: 220 feet
Descriptive remarks: Same as in experiment 5a: the needle was magnetized in the same direction as in the previous experiments, "but not to the same degree of intensity".

Experiment numbers: 6a, 6b, & 6c
Source: not known
Distance between 1^0 and 2^0 circuits: not known from Henry's surviving notes, although J. H. Lefroy (see Appendix III) mentioned a distance of about 600′. It could have been 300′[1].
Descriptive remarks: The secondary wire circuit is formed into a parallelogram, whose "farthest side was in the field beyond the society halls."[2] Therefore, the farthest side of the 2^0 circuit (i.e., the parallelogram of wire with the inter-connected small helical coil) is greater than 304′ from the 1^0 wire in Nassau Hall. Nothing was reported about insulation of the 2^0 circuit wire from the ground in experiments 6a, 6b, & 6c.

[1] J. Henry, London, Edinburgh & Dublin Phil. Magazine & Journal of Science, **30,** series 3 (1847) 186: Proc. American Philosophical Society, **iv,** p. 260: American Journal of Science, **3** (1847) 25.

[2] The Society hills evidently lie behind the two Society halls at the fartherest extension of the old back campus. The distance between the back of Nassau Hall facing the back campus, and the front of the two Society halls is 304 feet.

From the testimonies (Appendixes II and III) we infer two different setups for the primary wire. The first arrangement is earlier in time (1835 and 1836) and is powered by a direct current source. This could have been either a cruikshank or a daniell d.c. battery. Because Prof. Henry's laboratory notebook entry for May 12, 1840, noted that he used a daniell battery, and because of its electrical output characteristics, I am inclined to believe that he used this in his 1842 direct current test of the primary circuit. According to Lefroy's letter [8], the coil for the direct current test was introduced into the circuit in Prof. Henry's house, before exiting the line of wire towards the other well by his house. The purpose of this earlier 1835 and 1836 arrangement seems to be for the empowerment of an electromagnet to do work in causing a bell to ring for the purpose of communication. In this primary circuit, ground return closes the current between the two wells.

The 1842 primary circuit arrangement (in contradistinction to his 1835–6 setup) is mentioned in the testimonies (see both Appendixes) and is the one that is described by Henry [1 & 7] in his Laboratory notes of October of 1842, which carried a pulse of alternating current. In this setup, the primary wire is high

above ground having passed to "the two upper ones [windows] of the East end of the old college [Nassau] hall" [1 & 7]. The primary wire arrangement is grounded at both ends in water wells. The primary wire passed through Prof. Henry's study in his house on the West side of the campus, exited through a window and was grounded in his water well. His electrostatic machine consisting of the prime conductor and rubber was located in his cabinet on the second floor of Philosophy Hall at the East side of the old front campus.

I can not determine from the remaining historical record of these experiments, how the rearwards portion of the parallelogram configuration of the secondary circuit was positioned so as to insulate it from the ground, if indeed it was. The primary circuit acted as a transmission line terminating in a characteristic impedance.

Experiments of October 1843

4. Description of the direct current tests on Monday, Oct. 16, in preparation of the circuits for the discharge to the primary and the induction of the secondary circuit wires. From what can be reconstructed from the description of the setup for Oct. 16, Prof. Henry stretched a line of copper bell wire across the breadth of the front campus from the second storey window of Philosophical Hall to the second storey window of his study in his home, the line then being continued out through another window to end at a water connection in the well situated next to his house for grounding. According to this reconstruction of these events, I envision that there was a sag in the 400 foot length of copper bell wire across the 315 foot wide campus, in front of Nassau Hall, which would resemble the appearance of wire hanging between telephone poles today.

Refer to Prof. Henry's remarks in Appendix IV, for his description of the arrangement of the primary and secondary wires, and for his method of testing, by which he was able to determine the poor conductivity of the soil between the two wells on the front campus.

The long length of copper bell wire was insulated at the windows by being overlain across the top of an insulating tube (supposed to have been made of

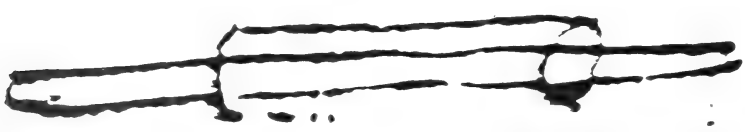


Fig. 1. Henry's own hand-drawn picture of his insulation technique of 1843

glass) which had been slipped over a round wooden stick emplaced horizontally into the frame of the window, as

"the stick was then fastened across the window" [1; vol III, pp. 70–76]

The end of the primary circuit wire in Philosophical Hall went to his "cabinet" where his batteries of one jar and of three jars, and his machine were kept. Another length of wire ran in turn, from there as grounding, out of the window

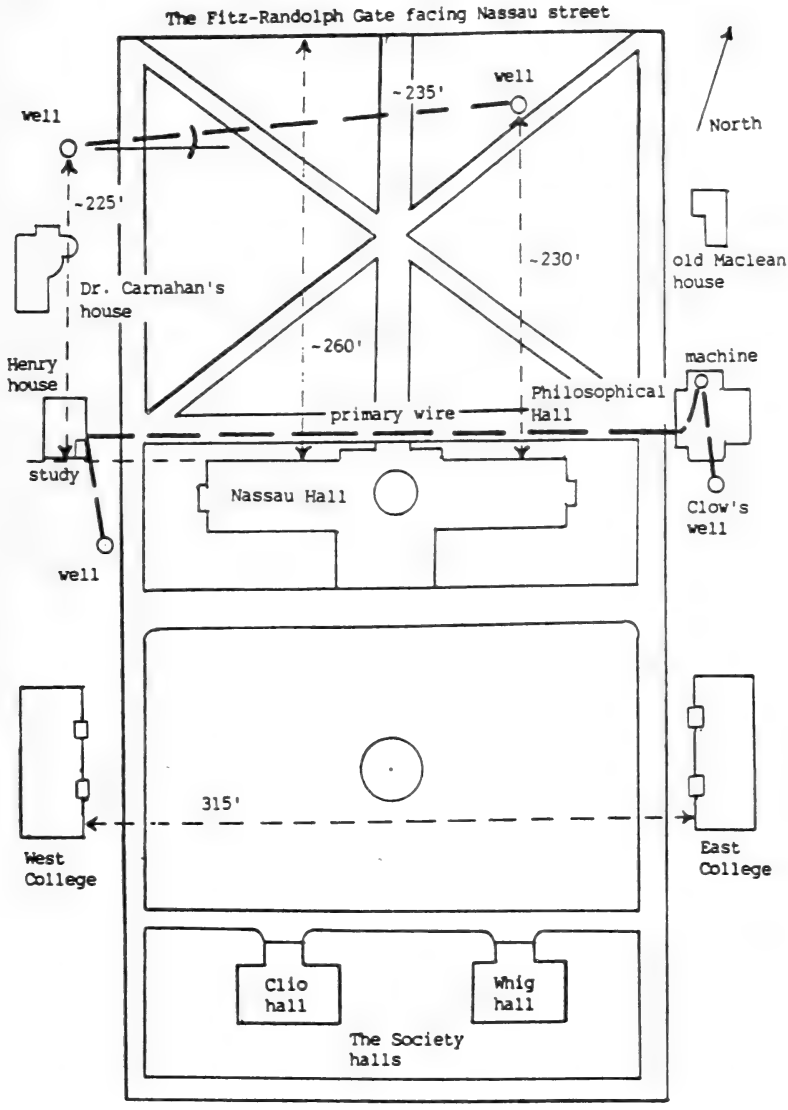


Diagram 2. Sketch of the campus of the College of New Jersey at Princeton, circa 1843, showing approximate locations of wiring

at the side of Philosophical Hall, ending in a water connection in Mr. Clow's well.

5. Discharge signal experiments performed in October 17 and 19, 1843. Two classes of discharge experiments were considered in this series of examinations, for the mutual field effects of a pair of very long wires across the breadth of the front campus at Princeton. The class of experiments done on Oct. 18, where the primary and secondary wires were about 18 inches apart, is of no concern for the purpose of this paper. In the other class of experiments that were conducted on October 17th and 19th, which is of concern to us, the secondary wire was far removed from the primary wire, and the layout of the wires is shown in diagram 2.

The following tests were made by Prof. Henry, whereby he determined the "adverse" nature of the current of the secondary circuit, the principal oscillation amplitude of the transient pulse being found to be of opposite sign (or in other words, of opposite polarity). The adverse nature was determined from the resulting polarity of magnetized needles after discharge.

The secondary line of wire lay between Maclean's well and Dr. Carnahan's well, and it was far apart (i.e., removed) from the parallel line of primary wire in front of Nassau Hall running from Philosophy Hall to Prof. Henry's study.

Chart 1 outlines the methodology of the class of test series of interest for the primary and secondary circuit wires far removed from each other.

6. On the possibility of ground return from the secondary wire. Prof. Henry excluded ground return currents as a possibility for explaining the observed magnetization effect at a distance from the source. The conductivity of the soil at Princeton is of the order of 10^{-3} siemen per meter (or in other words, 10E-03 siemen/m) and the conductivity of annealed standard copper wire at usual temperatures is about 5.8×10^{7} siemens per meter; which is at least of ten powers of magnitude greater than the soil conductivity. If for example, experiment no. 1 is examined for its conductivity, it becomes immediately obvious that the recorded electromagnetic effect in the large loop of copper wire (i.e., the secondary wire circuit) will maintain the path of its current in the loop; and in no way, can an earth current be established from the loop to return to the primary wire.

7. The magnetization of the steel needles in the coil was caused by a transient alternating electric current in the secondary wire, and this current was induced by electromagnetic waves. Henry believed that the transmission in distance would increase with an increase in the length of his wires in the parallel wire arrangement. But it does not seem that he was concerned with the possibility of transmitting intelligent communication over distances, but rather with the inductive effects themselves, as regards polarity and intensity. Because ground

Chart 1.—Outline of the October 1843 Test Series

Date	Test no.	Remarks
Tuesday, Oct. 17	1	Deflections [n] of needles magnetized in coils (interconnected with the secondary wire), by a discharge from one Leyden jar. **SEE Table 2.** The spiral used in each discharge is identified by number or letter[o]).

Table 1.—One jar

Experiment group	a	1	2	3	4	Remarks
1	−22	−12.5	−17	−5	−3	highly charged
2	−20	−3	−15	−3	−1	
3	−20	−10	−18	−3	−1	
4A	−45	−45	−40	−20	−4	
4B		−45	−38	−23	−2	stronger charge—loud snap
5A	−25	−12	−18	−4	−0	
5B		−17	−12	−3	−0	

RESULT. "all the [above] results give an induced current opposite to that of the [discharging] battery" [1].

Date	Test no.	Remarks
Tuesday, Oct. 17	2	Deflections [n] of needles magnetized in coils [o] (interconnected with the secondary wire), by a discharge from two Leyden jars. SEE **Table 1.**

Table 2.—Two jars

Experiment group	a	1	2	3	4
1	−20	−3	−7	−11	−0
2	−20	−20	−3	−12	−2

[n] These deflections were measured in relative units of measure.

[o] J. Henry designated by the letter a, the coil he used in his lightning discharge experiments of June 2, 1842, and his inducation at a distance experiments of October 1842.

Date	Test no.	Remarks
Tuesday, Oct. 17	3	Tests performed using a coil of about 300 turns of wire, attached into the secondary wire, with a needle suspended in the center. A movement of the needle occurred when the alternating current transient was passed from the discharge of a Leyden jar. With three jars, the movement of the needle was greater. Reversal of the discharge still indicated a current adverse to the discharge current of the primary circuit.

RESULT. Movement of the needle occurred when the a.c. was passed from the discharge of a Leyden jar. With 3 jars, the movement of the needle was greater. Reversal of the discharge still indicated a current adverse to the discharge current of the primary circuit.

NOTE. Some other experiments were performed on October 17th; and Prof. Henry also mentioned that he used Dr. Franklin's battery of 24 jars with a charge of 100 units. The weather had become more damp.

Date	Test no.	Remarks
Thursday, Oct. 19	4	The "adverse" effect of the discharge current was also determined by means of a method that was due to C. Matteucci (Annals de chimie 1847).

return currents can be ruled out, the question naturally arises as to whether Prof. Henry was observing an effect that was due to magnetic induction or an effect that was due to the creation and detection of electromagnetic waves. A crude means by which a decision can be attempted as to whether the observed magnetization effect was due to the presence of electromagnetic waves, is to model the primary wire as straight and parallel to and situated at a sufficient height above the ground, and calculate an approximate value for the frequency, and see if this value is in the range of radio spectra. Therefore, given

The parameters describing the primary circuit wire

length ℓ	= 400 feet	= 12,192 cm
diameter d	= 0.05 inch	= 0.127 cm
height h	= 19 feet	= 579.12 cm

and using the appropriate formulas [9] for estimation of capacity C for a single wire parallel to the ground, (for which by algebraic reduction, I derived a simpler version of formula 117 on page 237 of Ref. 9), and for the estimation of inductance L for a fine copper wire,

$$C = 0.2416\ell \div [\log_{10}(4h/d) + \log_{10} A];$$

$$A = (\ell + \sqrt{\ell^2 + d^2}) \div (\ell + \sqrt{\ell^2 + 16h^2})$$

$$L = 0.002\ell[2.303 \log_{10}(4\ell/d) - 0.75]$$

and the formulas for frequency $f = 1/2\pi\sqrt{LC}$ and wavelength $\lambda = cf^{-1}$ (where $c = 2.997 \times 10^8$ meters per second, is the velocity of the propagation of light in air according to A. A. Michelson), the following can be shown:

Comparative estimations of electromagnetic parameters for a primary wire of a 400 foot (121.92 m) length

$C \approx 691.92 \times 10^{-12}$ farads
$L \approx 295.31 \times 10^{-6}$ hernies
$f \approx 3.52 \times 10^5$ cycles/sec or Hz
$\lambda \approx 850.8$ meters/cycle

The magnetization of the steel needle in every experiment was caused by the induction of a transient alternating electric current in the secondary wire. The record of the October 1843 experiments shows that the negative polarity of the magnetization of the steel needle is a remnant that corresponds to the positive polarity of the initial electric current alternation in the primary wire. The initial alternation of current possessed the greatest amplitude in this ringing of the circuits.

The steel needle and the coil surrounding it, together constitute the detector of the transient field of the wave at time approximately r/c (r being the approximate distance between the two circuits) after transmittal of the wave from the primary wire, since the primary and secondary circuit elements are not infinitesimal in size. A train of waves in these experiments, corresponded to a single spark. The activating of a train of waves and current oscillations is called ringing the circuit. From the standpoint of human cognition, the intellectual recognition of the existence of this magnetization event may be considered to be the signal. From a physical standpoint however, the signal may be considered to be the actual physical event itself. The detected wave was wireless.

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Appendix I

Suggestion Made by Faraday in 1846 for Explaining the Action of Radiant Phenomena Including Gravitation, by Transverse Vibrations of Lines of Force Instead of an Aether

In his Note, Faraday hypothesized that

> "a notion which as far as it is admitted, will dispense with the aether. . . . The consideration of matter under this view gradually led me to look at the lines of force as being perhaps the seat of the vibrations of radiant phenomena."
>
> . . .
>
> "The power of electric conduction (being a transmission of force equal in velocity to that of light) appears to be tied up in and dependent upon the properties of the matter, and is, as it were, existent in them."
>
> . . .
>
> "Whatever the view adopted respecting them may be, we can, at all events, affect these lines of force in a manner which may be conceived as partaking of the nature of a shake or lateral vibration."
>
> . . .
>
> "It may be asked, what lines of force are there in nature which are fitted to convey such an action and supply for the vibrating theory the place of the aether? I do not pretend [t]o answer this question with any confidence; all I can say is, that I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted. The lines of weight or gravitating force are, certainly, extensive enough to answer in this respect any demand made upon them by radiant phenomena. . . ."
>
> . . .
>
> "The view which I am so bold as to put forth considers, therefore, radiation as a high species of vibration in the lines of force which are known to connect particles and also masses of matter together. It endeavours to dismiss the aether, but not the vibration."

Appendix II

Testimonies Concerning Placement of Primary Circuit Wire in 1835 and 1836

These testimonies in Appendix II concern that setup arrangement of the primary circuit in the earlier years (1835 and 1836), it being different from the setup arrangement (or placement) of the primary circuit wire in the experiments in 1842.

The end attachments of the primary circuit wire were located in each well. This circuit wire seems to have gone from the window of the upper story of the library building, down to what seem to have been insulating supports located on the grounds of the "front campus" facing (i.e., in front of) Nassau Hall; and then across the breadth of the front campus at about ground level, and up into a window in Philosophical Hall, thence out a window, and then down into the second well for grounding. Refer to diagrams 1 and 2 above, and to the pictures drawn by Henry in his *Laboratory Notebook* [1] on page 6, of the date Oct. 6, 1842, for an illustration of the setup of his primary circuit.

Prof. Henry [4.iii] gave the following description of the setup for his primary wire arrangement in a letter that he wrote to the Reverend S. B. Dod, on December 4, 1876.

> "I think the first actual line of telegraph using the earth as a conductor was made in the beginning of 1836. A wire was extended across the front campus of the college grounds, from the upper story of the library building to the philosophical hall on the opposite side, the ends terminating in two wells. Through this wire, signals were sent, from time to time, from my house to my laboratory."

Another description of the setup of Prof. Henry's primary wire arrangement was given by Prof. Henry Clay Cameron [4.iv]

> "From his lecture-room to the opposite building, and thence to his house, which was the house now occupied by General Kargé, but then standing on the site of Re-Union Hall, stretched a wire, through which currents of electricity were sent that rang bells and thus conveyed messages."

William B. Taylor [4.v]:

> "In 1835, wires had been extended across the front campus of the college grounds at Princeton from the upper story of the library building to the Philosophical Hall on the opposite side, through which signals were occasionally sent, distinguished by the number of taps of the electro-magnetic bell. . . ."

Appendix III

Testimonies Concerning the Placement of the Primary Circuit Wire in 1842

In the experiments of October 1842, the primary circuit wire extended through the second story of Nassau Hall, elevated high above the ground.

A description of the primary wire arrangement used in the series of 1842 experiments is found in a letter from the English military officer John Henry Lefroy to Edward Sabine [9].

> "He [Henry] leads a wire 400 ft from his Physical Cabinet [Laboratory room] to his house, where it joins an helix, and is conducted into a well. At the other end he forms a dimutive battery of about halfinch plates, also communicating

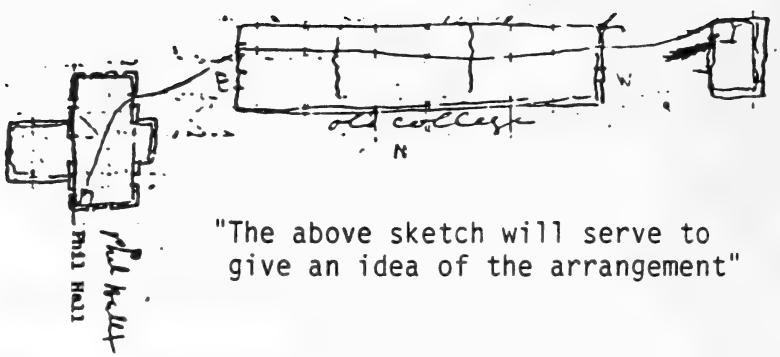

Fig. 2. Henry's own hand-drawn picture of his primary wire layout of October 1842

with another well there. In ⟨*making*⟩ immersing the plates a ⟨*circuit*⟩ current is transmitted which magnetizes the needles in the helix, the circuit being completed only through the *dry soil* (compact shale) between the two wells[p]. He finds that *every machine*[q] spark he makes in his cabinet [laboratory room], magnetizes a needle in an helix formed in a wire entirely unconnected with the machine, or the building, and about 600 ft distant."[r]

The following account appears in to **The Papers of Joseph Henry** [8a].

"For Henry's account of the meeting with Lefroy, see Record of Experiments, October 15, 1842, above. The experiment Lefroy described is in the Record of Experiments entries of October 6, 7, and 8, 1842, above. Henry and

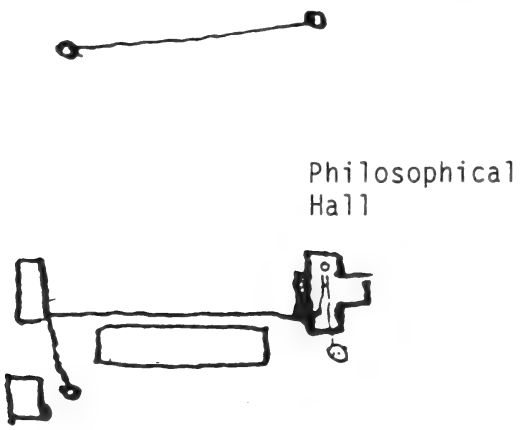

Fig. 3. Henry's own hand-drawn picture of his primary and secondary wire layout of October 1843 on the old front campus

[p] This description is the same as found in Prof. Henry's description of the preliminary test on the primary circuit that was made on the morning of October 6, 1842. The well by Dr. Carnahan's house was about 7.5 feet across, and the well by the Maclean house was about 6.25 feet across.

[q] The electrostatic friction machine with the grounded leather "rubber".

[r] Also refer to Lefroy's **Diary**, page 173.

Lefroy took their magnetic measurements in 'Mr. Clow's Field,' two hundred yards [editor] southwest of the college (Lefroy, diary, p. 173), the same field in which Henry's electrical experiment was set up."

Appendix IV

Monday, October 16, 1843. The Experimental Setup

According to Prof. Henry, on the afternoon of Monday, October 16, 1843, he

"Arranged . . . a wire across the campus[s] for transmitting a discharge of electricity from[t] + the Phil[osophical] Hall to the well opposite our house[u] [after having entered/ingressed and exited/egressed], and back through the ground to Mr[.] Clow's well at the end of Phil. Hall." [1; vol. III, pp. 70–76]

Monday, October 16, 1843. The Testing of the Earth Connections of the Primary and Secondary Circuits with Direct Currents

Prof. Henry

"tested by means of the galvanometer with the[?] small single battery, the negative element of which was formed of a silver thimble, and the positive of a single point of zinc plate. . . . When the circuit[?] was completed, the needle was violently agitated by[?] the mere touching of the point of zinc to the surface of the acid".

[s] This line of wire was "stretched across the campus [breadth] in front of Nassau Hall", according to Henry.

[t] In the old Franklin single fluid theory, + indicates the excess of electricity; that is, the terminal of Henry's primary wire was at the indicated source of charge.

[u] After having entered and egressed via the windows at the upper storeys of the two buildings, in accordance with the remarks made by Prof. Henry [4.iii] in his letter to S. B. Dod of December 4, 1876, and Prof. Clay Cameron's remarks [4.iv], in which there were described the layout of the primary wire for Prof. Henry's earlier experiments in 1836, according to Henry.

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Development of Psychology at The Catholic University of America

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ABSTRACT

Problems and controversies are described that surrounded the introduction of psychology as an academic subject in 1892 at The Catholic University of America in Washington, DC. This undertaking was a true pioneering effort, since it was the first time any university under Catholic auspices had made a psychology laboratory course a part of its curriculum. The unfolding aims and achievements of the Psychology Department, now celebrating its centennial, are described by detailing the views and accomplishments of its two forceful early leaders, Edward A. Pace and Thomas Verner Moore. Note is taken of the Department's leadership role in its early establishment of psychological clinics, a counseling center, and schools for special populations. Perspectives, particularly in regard to theoretical assumptions, are broadened by comparing developments in psychology at Catholic University with temporally parallel developments at the leading Catholic institutions in Europe.

Development of Psychology at the Catholic University of America

In discussing the origin and subsequent course of an academic department, more is often said about shifting personnel and courses than about the rationale for instituting a department and the nature of the department's reigning ideas. I have emphasized the latter, not because of any value judgment, but because the material is available, as it rarely is, to permit an in-depth approach. In my concentration on the ideas of the first two departmental chairpersons who were in charge of psychology at Catholic University—either as chair or dean—for a total of 56 years, I have doubtless slighted the contributions of other departmental members during this period. But in searching the sources available to me, I have been able to find only passing references to other department members. Certainly no marked divergence from the leadership exerted by the two dominant leaders has come to light. Also, it should be kept in mind that at least until 1941, this small department operated much in the European tradition, with the senior professor setting the agenda for the research undertaken by the graduate students. The most recent 30 years of the department await a future

chronicler, but my prejudice as an onlooker for much of this period is that recent years have shown more mainstream progress but less drama than the earlier period.

Early Years: Contributions of E. A. Pace

In considering academic disciplines from the standpoint of their ability to generate strong opposition and bitter controversy, including condemnatory objections from organized religion, *experimental psychology* would seem to be one of the more unlikely candidates. If today one were to consider which academic disciplines would be most likely to stir people's ire, topics might be mentioned that can be found in sociology, economics, intellectual history, and those recently organized cross-disciplinary fields that deal with sexual and minority studies. A century ago, particularly for the Catholic church and particularly in America, the situation was quite different; the new experimental psychology promulgated by Wilhelm Wundt and other learned German professors was in many quarters viewed with great suspicion. Never mind that at that early date in academic psychology, study was largely confined to the description and measurement of sensation and perception. Therefore, when the young priest Edward A. Pace returned from Wundt's Leipzig laboratory in 1891 to begin a professorship of psychology at the brand new Catholic University of America, writings and talks justifying the place of this innovative subject in the curriculum formed a large part of the new Ph.D.'s scholarly output at that time and for many years thereafter. Justification was all the more urgent because the original faculty of this newly instituted university, confined solely to graduate teaching, consisted of no more than a dozen priests and religious brothers. Thus the variety of courses that could be offered was quite limited.

There were a number of reasons why Catholics, particularly those in the Catholic hierarchy, considered experimental psychology to be incompatible with their religious beliefs. One factual reason was that during the second half of the nineteenth century several German adherents of the "new" psychology, as it was then called, left the Catholic church. Obviously, participation in psychology was dangerous to religious faith. Among these Catholic apostates were Carl Stumpf, who was to hold the prestigious Berlin chair in psychology for many years; August Messer and Karle Marbe, prominent investigators of the Wurzburg School; and most influential of all, the ex-priest Franz Brentano who had, however, actually resigned because of disagreement with church doctrine promulgated at the First Vatican Council. This post-Darwinian period was full of clashes between science and religion within many Christian churches, but within the Catholic Church the clash between psychology and religion was especially sharp because an important cornerstone of Catholic philosophical teach-

ing was philosophical (or rational) psychology, based primarily on the teachings of St. Thomas Aquinas. Thus, Misiak and Staudt, (1954) authors of the most comprehensive volume on Catholic psychology in America, claim that the primary reason that Catholicism and the new psychology were deemed incompatible seems to have been the inability of Catholic scholars to recognize a real distinction between experimental and philosophical psychology. They considered the *soul* of philosophical psychology as the ultimate subject matter, and therefore could not see how psychology could ever be an experimental study. The very possibility of a laboratory approach was precluded, they believed, by the nature of the subject matter, the immateriality of the soul. For this reason the new psychology came to be labeled among these Catholic philosophers as "The psychology without a soul." Father Hughes of St. Louis University in an article of 1894 quoted by Misiak and Staudt stated: "A man has to forswear his belief in a truth of Christian faith and must be willing to admit that his soul is no more spiritual than his eye, if he wishes to have anything to do with the new psychology" (p. 5). The situation is summed up by Misiak and Staudt:

> From the beginning, the new psychology was scrutinized by Catholic philosophers, especially those in the field of philosophical psychology, and very soon they began to take definite stands toward it. The majority were suspicious of the new psychology, a great number were opposed to it, and only a few accepted it (p. 4).

Other reasons for opposition were not lacking. In Italy, for example, early empirical psychology developed as an outgrowth of the materialistic positivistic philosophy. A more long-lasting objection was that the allegedly scientific psychology was completely grounded in an unacceptable biological materialism and evolutionism. Other conservative Catholic philosophers conceded that a physiological psychology might have a limited place in describing purely physiological aspects of mental states and processes, but generalizations of greater scope would not be valid. The old scholastic argument against the possibility of quantification and psychometric measurement in psychology was once more trotted out (Braun, 1969).

> Only that which is extended can be measured that which is immaterial is not extended, and man's highest powers of intellection and decision making are immaterial. Therefore, the use of psychometrics in the study of man is premised on a materialistic concept of man (p. 65).

Other philosophical difficulties, as might be expected, were found in treatment of the mind-body problem and in the failure to adequately support the doctrine of free will.

The year 1879 was considered important in Catholic circles as being the birth

year of the Neo-Thomistic movement. Pope Leo XIII issued the encyclical *Aeterni Patris*, asking for the revival of the philosophy of St. Thomas Aquinas, as "the remedy for the confusion and sterility of the philosophy of that period." This was the same year as that of the founding of the first psychological laboratory at Leipzig by Wilhelm Wundt. Misiak and Staudt (1954) comment:

> While there seems to be no apparent relationship between the two events, the founding of the psychological laboratory and the promulgation of this papal encyclical, it is in *Aeterni Patris* that one should see the reason which prompted some Catholic philosophers to recognize the new experimental psychology. In this message, Leo XIII directed attention to the progress that had been made in the natural sciences, and exhorted Catholic philosophers and theologians to take note of this progress, to advance with it and not against it (p. 34).

The leader in integrating Neo-Thomistic philosophy with psychology was Father and later Cardinal Desire Mercier. (Note: Neo-Thomism and Neo-Scholasticism can be considered here as synonymous, although at times Neo-Scholasticism can have somewhat broader connotations.) Mercier founded a psychology department at the University of Louvain in 1891 and of greater immediate fame, the Institute of Neo-Thomistic Philosophy at the same university in 1895. An interesting sidelight is that at this time Catholic University wanted to recruit him as a professor. In spite of entreaties to Pope Leo XIII to prevail upon Mercier to accept the position, Mercier never consented, as he felt he was needed in Belgium. Mercier remained a philosopher and did not undertake formal study of psychology, but he argued strongly for its necessary inclusion in philosophical studies. In 1891, he said (Misiak & Staudt, 1954):

> Psychology is undergoing today a transformation from which we would be blameworthy to remain aloof. . . . Here is a young, contemporary science, which is in itself neither spiritualistic nor materialistic. If we do not take part in it, the psychology of the future will develop without us, and there is every reason to believe, against us (pp. 34–35).

A man whose many talents were in demand, Mercier was soon a university administrator, then an archbishop, and by 1908 a cardinal. Ten years older than Pace, Mercier seemed in many ways a role model.

Most of Pace's articles supporting psychology and meeting the above objections were, appropriately enough, addressed to Catholic audiences in Catholic publications. An exception was a quite long article, *The Definition of Psychology*, in a 1902 *Scientific American Supplement*. I will limit myself to citing the general tenor of a few of Pace's themes in his support of empirical psychology (Braun, 1969). Perhaps the most obvious argument in behalf of psychology was that it supplied necessary knowledge for philosophizing. (All remaining cita-

tions in this section are from Braun unless otherwise noted. Material quoted by Braun that is directly from Pace is in single quotes.)

> There are sizable philosophical problems concerning man; what precisely is his nature, what are the reasons for his acting in such a manner, how culpable is he for a particular action, and so forth. The discoveries of experimental psychology offer not only an aid to the solution of these problems but also provide indispensable knowledge for a better philosophical understanding of man. The more we know about the operations of man, 'the better we are prepared to speculate about his nature' (p. 71).

In amplification of this argument, an appeal was made to the developmental formation of knowledge; this genetic causality argument was innovative in that it added a dimension that was largely lacking in scholastic philosophy.

> With information obtained in the laboratory it is possible to refine the philosophical axiom 'all knowledge begins in the senses' by showing how later cognitional experiences are heavily conditioned by childhood sensations. Psychology has discovered the laws which govern the functions of man's higher mental activities and the part these laws play in those functions. . . . The child is constantly receiving impressions and storing up images which will persist throughout the remainder of his life. The limitations which then arise are such that 'the whole future of the mind is largely determined by what is seen and heard in this period' (p. 74).

Psychology may provide desirable and even necessary facts, but Pace never passed up a chance to stress that psychology is incomplete without philosophical interpretation.

> Psychology finds itself obliged to canvass its results in the light of philosophy. Even though at times psychology may attempt to cautiously avoid any contact with philosophy, it is hardly able to do so. 'Its anxiety to keep clear of philosophical problems sometimes lets out its real, though clandestine, relations with this or that system.' The imperative of the philosopher is to concern himself with this study so that any interpretive bias may be exposed as an interpretation and not a demonstration. Also, the philosopher must become knowledgeable with the conclusions of psychology so that the new material may be interpreted in the light of first principles (p. 75).

Not much of scientific value could be considered apart from philosophy, according to Pace, since philosophers were the ultimate arbiters of cause-and-effect inferential reasoning.

> 'Science also takes over from philosophy certain indispensable ideas and concepts, and of these the principal one is causality.' When science explains that in a series of events, B is the effect of A, it is presuming the principle of causality. But *cause* is a philosophical term and 'it pertains to philosophy to determine exactly what is meant by *cause* or *effect*, and to determine whether cause is anything more than mere succession' (p. 87).

Pace conceded 'that a man may become a first-class chemist, or an excellent biologist, without so much as opening the primer of philosophy.' But each time the scientist is using the terms *cause* or *effect*, "he is paying an unconscious tribute to philosophical speculation" (p. 88).

Interestingly, in relation to cause and effect, there was claimed to be circular transfer benefits for the student in studying the sciences, including psychology, together with Thomistic philosophy.

> 'Scientific training lends them [students] exactness when they begin to philosophize. Even though this may be a secondary consideration in the relation of philosophy to science, it does not detract from the conclusions that modern science is the best ally, and not, as some would think, the lurking foe of philosophy' (Braun, 1969, p. 87).

On the other hand, "the person whose mental powers have been developed by a familiarity with the scholastics, particularly Thomas Aquinas, will be prepared 'to deal successfully with the problems of modern science' " (Braun, 1969, p. 136). Finally, Pace presented a pragmatic argument for the study of psychology in which he

> speaks quite harshly to those Catholics who indiscriminately criticize experimental psychology. In speaking to them he says: 'Either get hold of this instrument and use it for proper purposes, or leave it to the materialists and after they have heaped up facts, established laws and forced their conclusions upon psychology, go about tardily to unravel, with clumsy fingers, this tangle of error' (Braun, 1969 p. 76).

In other words, whoever has the best grasp on the stick of experimental psychology can use it to thrash threatening adversaries. And the stick is already lying there, so be first to grab it.

Pace in his early years at the University had to sustain direct *ad hominem* attacks on the basis of what were considered his unorthodox views. When he was invited to lecture at the Columbia Catholic Summer School in Madison, Wisconsin, in 1896, and had accepted, the Rev. Dr. John Zahm, who had invited Pace to speak, wrote to him that Bishop Sebastian Messmer of Green Bay had attempted to cancel the invitation—"the only reason assigned . . . was that you are not sound in philosophy, that you are a dangerous liberal, etc." (p. 9). Subsequently, Messmer wrote directly to Pace,

> We finally compromised by instructing him [Zahm] to inform you . . . and to state clearly that we shall consent to your lecturing at our session only on the clear understanding that you will not treat or bring up any matter or question in connection with your subject, which might give rise to dispute and unpleasant objections (p. 10).

What has been summarized thus far is an elucidation of the earlier statement that the seemingly "routine science" field of experimental psychology when introduced into the Catholic University was not only a pioneering but also a controversial venture. Certainly the amount of polemics that attended this curriculum innovation and to a surprising extent continued for many years, seems amazingly disproportionate to the cause. Irony is added when it is considered that experimental psychology was—as will be explained—not planned to be part of the curriculum but a sudden interest on the part of the young philosophy teacher.

Father Edward A. Pace, who had studied in Rome, was still a mere 26-year-old when he returned to Europe in 1888 to further prepare himself for his appointment as professor of philosophy at the soon-to-open Catholic University of America. That year he studied biology at Louvain along with his philosophy courses which stressed the then newly emphasized Neo-Thomistic outlook. The next year was to be a turning point in his career. While studying chemistry and physiology at the Sorbonne, according to Pace's own account, he chanced on a secondhand copy of Wilhelm Wundt's *Grundzuge der Physiologischen Psychologie* (the first of six editions was published in 1874) at a bookstall in Paris. Reading Wundt's book so impressed him that he resolved to study psychology with the author. Suiting deed to desire, he went to Leipzig the next year, enrolled as a student of psychology, and further broadened his education by taking courses from the eminent physiologist Carl F. W. Ludwig. He was the first Catholic priest to study with Wundt and the sixth American. In 1891 he received his doctoral degree magna cum laude.

What would the early course of experimental psychology have been without the adventitious element introduced by the used psychology books sold in Paris? A more famous case of bibliographic inspiration had occurred slightly earlier. In the late 1870's, the German, Hermann Ebbinghaus, recipient of a 1873 doctorate in philosophy, came across Gustav Fechner's *Elemente der Psychophysik* (1860) at a secondhand book shop in Paris. Noting that the vigorous approach Fechner took in studying sensations could be adapted to the study of human memory, Ebbinghaus undertook a laborious pioneering study of human memory without any acquaintance with psychologists or university affiliation, "with his sole reliance on Fechner's book and his own interest" (Boring, 1950, p. 387). The two biblio-converted psychologists convened together in 1904 when Pace, now dealing with psychology more as an administrator than a participant, was honored by being invited to serve as the Chairman of the Experimental Psychology Section at the St. Louis Universal Exposition. The leading speaker was Professor Ebbinghaus.

Father Pace with his new doctorate in psychology returned to Catholic Uni-

versity in the autumn of 1891 as a member of the teaching faculty. The curriculum included a *course illustrated by the most modern scientific apparatus, imported expressly for the purpose by the professor*—the famous "brass instruments" associated with the early psychological laboratories. With the centennial dates of a number of psychology departments including Catholic University celebrated or in prospect in recent years, various claims have been put forward as to the dates of origin of the celebrating departments and this includes the founding date of the experimental psychology laboratory at Catholic University. (In that era and for many years afterwards, the mark of a *real* psychology department was that it possessed a laboratory, so that the department was referred to by the laboratory name—a particularly long-standing tradition in the Ivy League.) In some places the Catholic University founding date is said to be 1891, but elsewhere it has been listed as 1892. With classes then opening in November, rather than late August as at present, not much "founding" could have been done in the waning months of 1891. (Time required for Pace's psychological studies meant that he had missed the first two academic years at Catholic University, as classroom teaching had begun in 1889.) The 1892 date can probably best be taken as definitive, since the American Psychological Association has consistently listed that year as the founding date for the Catholic University laboratory (Fernberger, 1932; Garvey, 1929). Trustworthy ordinal rankings are unclear, since some early laboratories soon closed or operated discontinuously. For example, there has been a claim for Harvard preceding Wundt's establishment of his Leipzig laboratory in 1879, but others deride this claim as pertaining only to some lecture demonstration equipment. The generally acknowledged American *first* is G. Stanley Hall's Johns Hopkins laboratory of 1883. Other early laboratories were established at Pennsylvania, Yale, Cornell, Wellesley, Clark, and Columbia. Often overlooked but equally early were several midwestern state universities, including Indiana, Iowa, Nebraska, and Wisconsin. Doubtless there are others, as founding is often in the eye of the retrospective beholder. Certainly the psychological laboratory at Catholic University was a member of this early group. The inclusion of psychology in the Catholic University curriculum was more notable, however, because, despite strong opposition, this psychological laboratory was "the first of its kind in any Catholic institution of higher learning, antedating that of the Belgian University of Louvain" (Misiak & Staudt, 1954, p. 82).

Pace is reminiscent of William James in that, though he advocated empirical laboratory research, his own participation was brief. Among his numerous writings, not more than seven or eight can be counted as empirical studies, including two articles on pain, and two on fluctuations of attention with weak stimuli. His doctoral dissertation under Wundt was on a theoretical topic, *The Relativity*

Principle in Herbert Spencer's Psychological Theory of Development. When the American Psychological Association was formed under the auspices of G. Stanley Hall in 1892, the new Ph.D., Pace was not among the 26 charter members at the initial organizational meeting, but he was among the first five psychologists elected by the charter members. At the first annual meeting of the Association's 31 members in December 1892, Pace presented a paper titled *Tactile Estimates of Thickness*, and the following year a paper on *Pain Contents*, but he did not give papers at later meetings. Pace was a founding member of the American Philosophical Association in 1893.

After 1902, Pace's primary contribution to psychology, apart from teaching some courses, was as an administrator and psychological apologist. In 1891 when Pace returned to Catholic University, all courses, including psychology and philosophy, were offered under the faculty of theology. In 1895, a School of Philosophy was organized with Pace serving as dean for the first four years. A separate department of psychology was established in the school of philosophy in 1905. Pace's title was professor of psychology (1891–94) and professor of philosophy thereafter (1894–1935). Although Pace continued to teach psychology courses, he also went on to hold other offices in the University, including additional terms as dean of philosophy (1906–14, 1934), general secretary (1917–25) and vice rector (1925–36). He took an active role on the editorial board of the *Catholic Encyclopedia*, at that time a pioneering venture, which published fifteen volumes between 1907 and 1912. His interests also turned to education, and in 1911 he became co-founder and first editor of the *Catholic Educational Review.* In the latter part of his career he turned once more to writing on strictly philosophical topics, and co-founded the American Catholic Philosophical Association, becoming co-editor of its journal, *The New Scholasticism.* By this time Pace's role in psychology was limited to being an encourager and propagandist for psychology and nominal editor of the psychology department's proprietary publication (common in many universities at that time), *Studies in Psychology and Psychiatry from The Catholic University of America.* Thus in 1969, thirty-one years after his death, when Pace's life was traced in a Catholic University dissertation by William P. Braun, C. S. C., it was probably appropriate that the title omitted specific mention of psychology—*Monsignor Edward A. Pace, Educator and Philosopher.*

Theoretical and Applied Psychology: Diverse Contributions of T. V. Moore

The Paulist priest, Thomas Verner Moore, heard Pace speak in the summer of 1896 about the new psychology in Germany and the laboratory he had established in Washington. The next year Moore asked Pace to accept him as a student. After taking courses to make up for his lack of background in mathe-

matics and science, Moore received the Ph.D. degree under Pace's direction in 1903. But this was just the beginning of his training. That summer he got to know the famous psychologist of learning theory, Edward L. Thorndike, while taking his course at Columbia Teacher's College. The following fall he went to Leipzig to study with Wundt resolved, he said, to study *the problem of how to open the field of thought processes to experimentation*, a problem that Wundt thought could not be brought into the laboratory. Thorndike had told Moore to look up a young Englishman then resident at Leipzig, Charles Spearman. At that time Spearman was just developing the mathematical technique of factor analysis in which he postulated a general or *g* factor interacting with a variety of task-specific factors in all intelligent behavior. From that time on factor analysis became Moore's preferred research tool, although he later favored Louis Thurstone's group factor approach to factor analysis over Spearman's. Moore later extended his application of factor analysis beyond the study of intelligence to investigations of personality and emotion.

Moore spent the year 1905–06 recuperating at a tuberculosis sanitarium in Germany. On his return to America in 1906, Moore went to Berkeley, California, where, while serving as chaplain of the Newman Club of the University of California, he studied physiology and chemistry. Shortly after returning to teach psychology at Catholic University in 1909, he visited Lightner Witmer's clinic, the pioneer American psychological clinic, at the University of Pennsylvania. This gave Moore the idea to found such a clinic at Catholic University. But, no doubt correctly at that time, he realized that this goal could not be accomplished unless he acquired a medical degree. He began his medical studies at Georgetown Medical School in 1911, continued them at Munich in 1913, and returned to America still a semester short of a degree because of the outbreak of World War I. He finally obtained an M. D. degree from Johns Hopkins in 1915. In 1916, now established as an associate professor, he opened a clinic at Providence Hospital in southeast Washington, DC. The clinic was transferred to the campus of Catholic University in 1937, where it was known simply as the Child Center. A name change occurred with the psychology department becoming the Department of Psychology and Psychiatry in 1939. At this time the Department and the Child Center were enabled to expand by means of a grant from the Rockefeller Foundation. The work Moore started with Wundt in 1904 was completed at Berkeley and published in 1910 under the title, *The Process of Abstraction.* Moore (1948) concluded in that study:

> the technique of the study allowed one to distinguish various stages in the process of perception and to arrive at an important generalization: Perception proceeds from that which is most general, that is to say, from knowledge without reproducible imagery, to an adequate analysis of the object perceived

> and a final full sensory representation of the individual object. . . . Perception first develops a knowing rather than a sensing, even though it does so by means of sensation (p. 43).

While he was in Munich in 1913, Moore worked with Oswald Kulpe, the famous Wurzburg psychologist who had recently taken the chair at the University of Munich. This study was published as a monograph in 1919, and "The result of this study showed that an unanalyzed consciousness of the meaning arises prior to any conscious image in the perception of printed words, and prior to the word in the perception of pictures." These results, together with the Berkeley findings, substantially supported the concept of *imageless thought* that was a major conclusion of the Wurzburg investigations. Moore's study received a renewed number of citations about 20 years ago when the psychological study of imagery was resuscitated in academic psychology. Moore's interest in clinical problems was further stimulated by his service in the Medical Corps in France in 1918–19:

> The war neuroses offered a valuable opportunity for studying emotional conditions and enabled me to delineate a group of emotional disorders, the parataxes, which lie between the normal emotional reactions or *psychotaxes* [italics added] and the major psychoses. . . . the factorial analysis demonstrated the existence of a group of empirical syndromes and these syndromes were in general easily identified with certain Kraepelinian diagnostic entities (Moore, 1948, p. 45).

> It was during this (war) service that he became a friend of Dr. Winfred Overholser, who later became superintendent of St. Elizabeths' Hospital in Washington, D. C. For many years thereafter a strong relationship of mutual benefit existed between the Department of Psychology at Catholic University and St. Elizabeths Hospital (Peixotto, 1969, p. 846).

Moore taught the first clinical psychology course in 1916, entitled, *A clinic for the examination of defective children.* In 1921 he taught a course in psychiatry for the first time. In 1921 it also became possible for undergraduates to major in psychology, with the proviso that the student perform an empirical thesis. For decades, however, the number of undergraduate majors remained small and undergraduate teaching of secondary importance. As late as 1954, Misiak and Staudt wrote that "at Catholic University, undergraduate psychology has not been developed to any great degree," while at the same time praising the graduate program at Catholic University as a leader among Catholic institutions. Only in the late 1960's and especially in the 1970's was the undergraduate program highly developed. In line with undergraduate enthusiasms of this period, undergraduate majors usually exceeded 150 students.

In 1922 Moore was promoted to professor and became chair of the psychology department, a post he was to hold for the next quarter century. Moore's

variety of activities only intensified. In 1923 he left the Paulists and after a brief novitiate in Scotland became a Benedictine monk, who with six others established St. Anselm's Priory near the Catholic University campus.

> He founded a school for retarded girls in 1924 (St. Gertrude's), opened a preparatory school for boys in 1942 (The Priory School), taught at Trinity College in Washington, DC, for a number of years, served as editor of the Catholic University *Studies in Psychology and Psychiatry*, wrote numerous books, articles and book reviews, gave countless lectures, and offered invaluable psychological and psychiatric services personally to troubled individuals (Misiak & Staudt, 1954, p. 192).

In 1944, not long before his retirement, Moore proposed that the university should establish and staff a Catholic psychiatric hospital. This additional project was not acted on by the University (Nuesse, 1990, p. 225).

Moore wrote five books dealing with psychological subject matter between 1924 and 1948. In putting forward his views, I will mostly consider his best known book, *Cognitive Psychology* (1939), and to a lesser extent his later book, *The Driving Forces of Human Nature and Their Adjustment: An Introduction to the Psychology and Psychopathology of Emotional Behavior and Volitional Control* (1948).

Moore often described himself as a functionalist, and he described the work at Catholic University as functional in nature. This assertion did not mean that he followed the well known functional schools of psychology that were prominent at the University of Chicago and Columbia University. Moore's concept of functionalism was to determine the functions of mind, which task necessarily involved an ultimate theoretical interpretation. But like the better known functionalists, his functional approach included the concept that applied psychological findings could furnish useful perspectives. The linchpin of his theoretical undertakings was the revision and modernizing of faculty psychology as expressed in the Neo-Scholastic philosophic synthesis. Moore (1948) implied that considerable revision of philosophic doctrines might be brought about by psychological results:

> it is important in the study of the human to turn to the mind itself rather than to the commentaries on Aristotle and St. Thomas. The commentaries have their value, but if there is to be progress in psychology we must not only interpret the past but make investigations in the present and bring ancient truths into contact with the developments of the present (p. 45).

Immediately following this statement is a suggestion of a role model with whom Moore could identify:

> This task was the life work of St. Albert the Great, to whom St. Thomas owed so much. The study of empirical data is of even more importance in our day

> than in the time of St. Albert, because of the vast amount of material waiting to be synthesized. The data available at the present transcend in importance anything that St. Albert could ever have dreamed about. Let us look up from the texts and the commentaries at least long enough to have a glance at what is available in the present (p. 45).

But a few sentences further on he assures the reader that the basic philosophic dogmas will not be threatened: "To call attention to the various processes in concrete situations is not to deny the importance of any mental function defined by St. Thomas." Probably the most distinctive way in which Moore went beyond Neo-Scholastic philosophy empirically was his insistence, deriving from his experiments on imageless thought, that the phantasm (image) could not figure as prominently in the process of rational thought as the Thomist descriptions claimed.

> As previously mentioned, (Moore, 1945) factor analysis was to be the key: Thurstone stepped into the field with his 'vectors of mind' which, however, turn out to be faculties of the mind or mechanisms involved in the operation of a faculty. Thurstone himself recognized this when he wrote, 'Factor analysis is reminiscent of faculty psychology. It is true that the object of factor analysis is to discover the mental faculties' (p. 37).

But Thurstone's work needed interpretations grounded in philosophy, thus without a philosophic base Thurstone's contribution was no more than technical. "In general his work has been a contribution of technique, but the results he [Thurstone] has obtained support theoretical implications that transcend any philosophy to which he has as yet given expression." This limited praise ranked pretty well up the ladder on Moore's scale of compliments. The lack of a philosophical base was also held against Thorndike's theory of learning:

> A little sound philosophy would have eliminated many of the fallacies in the publications of Teacher's College and there might have been produced a psychology more true to the nature of man and more helpful to the philosophy of education (pp. 34–35).

Moore thought that factor analysis reinvigorated faculty psychology because it derived faculties and powers that ordinarily operated mainly through interactions with other mental faculties. According to Moore, the use of this technique allowed one to supersede the over-simplified and debased notion of faculty psychology prevalent in the twentieth century view that there are faculties that act independently, the view that had earlier been popularized in the pseudoscience of phrenology. Not everyone, however, found the factorial studies emanating from Catholic University compelling. A. A. Roback (1952), who included a chapter on Neo-Scholastic psychology in his *History of American*

Psychology, described a dissertation by Sr. M. R. McDonough on *The empirical study of character* that he claimed:

> exemplifies both the adequacies and inadequacies of the Catholic University standards—a broad survey of the literature and elaborate statistical treatment of the somewhat meager results, on the one hand, and a profuse citation of the literature, on the other hand, in lieu of delving into the core of the problem at issue (p. 367).

More troubling are the cause-and-effect inferences Moore derives from mathematical correlations. In one study that exemplifies this consideration, Moore (1948) gives a short description of a study he characterized as typical of work carried out at the Catholic University psychology department:

> St. Thomas Aquinas had a concept of sensory memory which has not been given specific attention by modern experimental psychology. . . . Imagination according to St. Thomas is the conservation of past sensory experience but without any accompanying label of its having been in consciousness in the past. To label it as past belongs to sensory memory. . . . Subjects were presented with words or pictures that they could remember as having been presented, and also any others that came to mind for a period of ten minutes. Answer sheets were scored by counting (1) words or pictures recalled and recognized (Thomistic memory), and (2) words or pictures recalled but not recognized (Thomistic imagination). It was found that Thomistic imagination for words correlated with pictures .46 and had negative but low correlations with Thomistic memory. It was therefore concluded that the two functions were capable of independent variation and were therefore distinct (pp. 51–52).

Moore's interpretation is especially unusual in drawing specific conclusions from the *absence* of positive correlations.

Roback, who was critical of Moore's factor analytic studies, was, however, amazed by Moore's early acceptance of many psychoanalytic doctrines. In his 1952 book, Roback wrote:

> Of greater import is Moore's acceptance, in large part, of Freudian psychology. Many years ago he astonished the writer, [Roback], when he told him that these psychoanalytic concepts were found very useful in his clinical therapy. Who would have believed even fifty years ago that such a deviation on the part of a Catholic would be permitted by his supervisors, but Moore is not the only ecclesiastic who has adopted Freud's methods and interpretations (p. 367).

Moore did in fact write several articles in which psychoanalytic interpretations were dominant and contributed at least one article to a psychoanalytic journal. Moore showed less enthusiasm for Jung and psychiatrists who, following Jung, intermixed their patients' religious convictions with psychotherapy. Further, Moore cautioned that religion as a therapeutic aid was only applicable to those

who have sincere and honest religious convictions and is of no use to patients who lack religious convictions.

During the time of Nazi domination in Europe, Moore (1948) undertook an unusual task that he performed at the behest of Pope Pius XI. This task came about because the Nazis:

> maintained that the difference between the higher and lower races of man is similar to that between the higher and lower animals. The German race was supposed to be at the summit of human development and lower human races scarcely differed from animals. . . . It was thought desirable to have a chapter on animal intelligence, so that one might see from the empirical evidence whether or not it was possible to consider the highest animals as approaching the level of the lowest existing human races, and so our department was asked to abstract and summarize the experimental and empirical evidence on this problem (p. 50).

Moore alone wrote up this literature search as sole author. Among other findings, he drew the conclusion that "no animal below man is capable of intellectual operations involving the handling of even simple general principles. Intellectual functions, therefore, must be differentiated from those of the special senses and the synthetic sense (p. 50). What is the *synthetic sense*? This is Moore's own theoretical term (1939) which he defines as:

> a mental ability that has to do with the holding together in one complex unit the various elements of a sensory presentation; and, for the purpose of interpretation and adequate behavior, accentuating now this and now that element of the complex without losing the structural unity of the whole. We may term this ability the *synthetic sense*, . . . [which] is not itself a knowing or an interpretation of a meaning but is the necessary condition so that objects can be known, pictures be interpreted, and speech be understood (p. 241).

Moore wanted his readers to know that novel as his terminology was, it was just a convenient concatenation of traditional Thomistic distinctions. "Scholastic philosophy distinguished between the functions of the *sensus communis* and the *vis aestimativis* (in animals) or the *vis cogitativa* in man. It may be pardonable in the present state of a psychological analysis to discuss both functions under the heading: synthetic sense" (p. 238). Adding to the oddness of the request and the esoteric strangeness of the answer is that Moore seemingly never did any animal research, nor ever showed any interest in animal psychology and, on occasion, he deplored the behaviorist emphasis on animal research to the neglect of the study of higher intellectual functions.

In Catholic circles in both Europe and America, the preferred therapeutic theorist in the 1930's and 1940's was neither Freud nor Jung but Alfred Adler. A foremost Adlerian proponent was the Viennese Rudolf Allers who moved to the United States in the later 1920's after writing several books, including *The*

Psychology of Character (1932), a Thomistic formulation of Adlerian psychology. In 1938 he was appointed professor of psychology and scholastic philosophy at Catholic University, but shortly thereafter joined the Department of Philosophy at Georgetown University. In 1940 Allers brought out a book denouncing psychoanalytic theory, titled *The Successful Error.* He insisted that Freudian doctrine and method are one and inseparable, therefore implying that it was impossible to reconcile Freudian psychoanalysis with Catholic doctrine, even if one made use of only a limited number of Freudian techniques. Allers followed up this controversial book with several journal articles in the same vein that were not only in opposition to Moore but also to several other prominent Catholic psychologists. Psychoanalytic permissibility came from the highest Catholic authority in 1953 when Pope Pius XII addressed the International Congress of Catholic Psychotherapists in Rome and "in this address the Pope approved the use of the psychoanalytical method and pointed out some abuses which Catholics must be careful to avoid" (Misiak & Staudt, 1954, p. 264).

Recently it has been pointed out (Knapp, 1985) that in titling his 1939 book *Cognitive Psychology* and in his emphasis on the functions of thought, Moore partially anticipated the *cognitive revolution* that was to come 25 years later. But the case is rather weak in that the commonalities found are chiefly the rejection of behaviorism and the use of reaction time procedures to differentiate among mental processes. In his "conclusion," the author, rather suddenly reverses his argument:

> *Cognitive Psychology* may be better viewed as a remnant of the past rather than as an anticipation of the future. To that end it can serve as supportive evidence for those who have argued that contemporary cognitive psychology is merely a return to an earlier period in the history of the discipline (p. 1315).

Too much should not be made of Moore's rejection of behaviorism. Apart from factor analysis and some psychoanalytic procedures there was little that he didn't find worthy of rejection and some disdain. One can consider the forthrightness of the following quotations from Moore's *Cognitive Psychology* (1939):

> Materialism, whether as behaviorism or configurationalism, [Moore's term for Gestalt psychology], or in any other guise can have an apparent ground to stand on only by restricting discussion to the dimension of movement or the physics and chemistry of sensory stimuli. . . . By evading issues materialism can contribute volumes to psychological literature, but by doing so it develops a psychology that has no value as a practical instrument in dealing with human problems, and which contributes nothing to the real mental life of man (p. 549).

Two of the psychologists whose work was to be particularly important in the cognitive psychology of the 1950's and 1960's were dismissed in footnotes by

Moore (1939) without further mention elsewhere. Regarding Frederic Bartlett's landmark book *Remembering* (1932): "This whole book of Bartlett's is so chatty and vague in its treatment that it is difficult to derive from it any definite conclusions" (p. 507). As for developmental psychologist Jean Piaget:

> Piaget's conclusion that the first stage of reasoning is characterized by wish fulfillment and magical premises was due to the fact that the problems transcended the sphere of the child's experience and he was forced by questioning to give an answer of some kind. . . . When there is less chance to read one's own ideas into the child's mind, Piaget's stages are not found (p. 370).

Moore (1939) himself apparently found it extremely difficult to give wholehearted approval to any psychologist without adding some important correction. (His former host and collaborator Oswald Kulpe was the exception.) One can take Moore's treatment of Helmholtz as an exemplar. On the plus side, Helmholtz "developed a theory of knowledge which in many points coincides with scholastic philosophy." But "he [Helmholtz] gave way to social pressure in adopting materialism. All his associates believed in the power of matter in motion to nature and man. And so Helmholtz postulated their belief, but it is logically incompatible with the intellectualism of his theory of knowledge" (pp. 189–190).

In 1947 at age 70 Moore accepted an invitation to lecture on psychiatry in Spain. While in Spain, he transferred from the Benedictine order to the stricter Carthusian order. In 1950, he returned briefly to the United States to establish a Carthusian foundation in Vermont. Nine years later his last book (1959) was published, *Heroic Sanctity and Insanity: An Introduction to the Spiritual Life and Mental Hygiene.* Moore died in Spain in 1969. Father John W. Stafford (personal communication, 1959) who succeeded Moore as departmental chair from 1947 to 1959 reported in a departmental colloquium shortly before Moore's death that, when visited in Spain, Moore had suggested that someone should undertake a factor analysis of the experience of religious ecstasy. Apparently the monastery did not quell his psychological cognitions.

Without consideration of the period in which Pace and Moore worked, their achievements in Catholic higher education can scarcely be appreciated in the climate of opinion that prevails today. Prior to 1939–45 Catholic graduate education, even aside from the special difficulties psychology presented, was all too often an unappreciated and misunderstood enterprise among American Catholics. Nor was the reputation of Catholic graduate education high in the wider American scholarly community. In the 1934 Raymond Hughes "Report of the Committee on Graduate Instruction," a pioneer reputational survey of a type which has since become recurrent, five departments of the twenty-three fields of the arts and sciences at Catholic University were rated adequate in equipment

and staff. Psychology was one of the five rated as adequate. Still this was in some measure an accomplishment, since the only other graduate department in a Catholic institution with a reputation rating of adequacy was the Chemistry Department at Notre Dame.

Some Catholic spokesmen accepted the concept of graduate education but thought concern with research was either unnecessary or overdone. Here objectors could cite that most eminent of English-speaking Catholics, Cardinal Newman. In 1937, Martin McGuire, Dean of the Graduate School of Arts and Sciences of Catholic University wrote:

> It is not necessary to discuss here Newman's view that research should have no place in a university, as President Angell of Yale has dealt adequately with this point in his essay on the aims and province of a modern university. Nor do I think it necessary either to examine here the strange idea of the late George Bull, S. J. that "a Catholic university which accepts research as the dominant objective of its graduate school, is by that much attempting the impossible task of being Catholic in creed and anti-Catholic in culture" (p. 112).

But opposition to too much emphasis on research could be found within the Catholic University faculty. Fulton J. Sheen, in the 1930's a young member of the Catholic University philosophy department,

> who was already becoming the foremost American Catholic apologist through his use of radio, saw as a desideratum "a de-emphasis on research as an end and purpose of university education." Allowing for the need of research in the natural sciences, a Catholic university as he envisioned it existed primarily for the "organization and dissemination of truth in the natural and revealed order" (Nuesse, 1990, p. 234).

Monsignor Sheen's quest for dissemination in later years was undoubtedly better fulfilled by television than anything the university could offer.

Views on Psychology (1945–1962) at Catholic University vs. Other Institutions

It might be taken as a foregone conclusion that in the changed atmosphere after 1945 psychology would soon be widely accepted and taught at Catholic colleges. It is some measure of the achievement of Pace and Moore, that a well-known psychology program had been in existence at Catholic University for more than 50 years. However, elsewhere in Catholic educational circles, psychology was commonly resisted and ignored. In 1953, William Bier, S. J. of Fordham University at the annual meeting of the Catholic Educational Association put forth the estimate that in the United States only 26 per cent of the Catholic men's colleges and 18 per cent of the women's colleges had psychology departments. What were the reasons why psychology could find such small acceptance in Catholic colleges? Certainly it must have seemed easier to teach

the rational psychology of Neo-Scholasticism in dogmatic form than allow for extensions and amplifications through empirical research, as Pace and Moore had wanted to do. As Moore's writings showed, given the ultimate goal of improvement in the scope and application of philosophical principles, it was a complex task to pick out and integrate findings that were both empirically valid and doctrinally irreproachable.

The doctrine that was the greatest single sticking point in Catholic higher education was the Darwinian theory of evolution. The acceptance of this theory that had pretty much come about in the science departments of many old-line Protestant colleges by the twentieth century did not hold for Catholic colleges. And, as has often been reiterated, the theory of evolution is a fundamental assumption of modern psychology. In 1923, just two years before the famous Scopes' trial concerning the teaching of evolution in Tennessee, the Fordham University digest of lectures in "fundamental psychology" stated a not atypical anti-evolutionary position: "Darwinism for many reasons must be rejected as false" (p. 14). Among the reasons was that "it openly **conflicts with well ascertained data** of experience, as well as with some of the conclusions of geology and palaeontology." A more specific proposition also called for refutation: "It is **false to say that even man's body is descended from the brute**" (p. 14). An argument was put forward that was common in the nineteenth century. "If the human body has been evolved **gradually** we should find in the earth's strata intermediate grades or forms of life. No such intermediate forms, or missing links as they are called, can anywhere be discovered. Nor could man's body have sprung from the brute by means of saltatory evolution for the effect cannot be more perfect than the cause" (p. 15). (Cited as sources were the *Catholic Encyclopedia* and five Jesuit authorities.)

In the same set of lectures obeisance to Aristotle reaches a ludicrous extreme in that Aristotle's notorious error of attributing many of the main functions of the brain to the heart is given considerable credence. Discussed is the determination of movement of man's limbs or organs by what in Neo-Scholastic terminology is termed the faculty of sensitive appetite: "All agree that the heart is at least the organ which manifests the working of this appetite. Experience clearly shows that sensitive impulses especially when vehement, constrict, dilute, accelerate or retard the heart and its movements. It is controverted whether or not the heart is also the **eliciting** organ. Many maintain that the **brain** immediately concurs in sensitive impulses. Others, with higher probability, teach that the heart is also the chief **eliciting** organ for they include likewise the nervous system, especially that portion of the large sympathetic nerve which resides in the heart. *This opinion seems more in accord with consciousness, with the common universal way of speaking which ascribes all the movements of the sensitive appetite to the*

heart, and finally is confirmed by the fact that the impulses are received in the heart and greatly modify it" (p. 46) [italics added to last sentence].

In fairness to Fordham University it should be noted that a psychology department and laboratory was established there in 1931. By 1952 Roback would write: "Although the Catholic University of America ranks with the better educational institutions in the United States, Fordham University is a close rival; and, in psychology, it is in a fair way to outstrip it in the not distant future" (p. 367).

The above scientific absurdities were of course far from any argument the medically-trained Moore would ever make. I have been unable to find any pronouncements Moore made directly about the theory of evolution, but he did find in the neighboring area of embryology support for some Aristotelian philosophical concepts. This subject matter is dealt with in Moore's book, *The Driving Forces of Human Nature* (1948) in the final chapter titled Formal Causality and the Philosophy of Nature. Moore argued that the embryological evidence, citing particularly the embryologists, Hans Driesch and Hans Spemann, often shows crucial preformational tendencies that enable embryos to achieve their entelechy in spite of experimental conditions that would seem to present an inseparable detriment. "Nature is not a mere swarm of moving particles, but a matrix of *materia prima* in which by laws, known as yet but dimly, formative forces arise, *rationes seminales* [seminal reasons], which coordinate development and disappear as ideas flash into consciousness and then cease to be" (p. 444). Thus Moore argues that Aristotelian and Thomistic concepts are still appropriate, especially the doctrine of formal causality—preordained intent as to design—that most biologists find far too teleological to accept.

The question arises as to why Pace and Moore found it necessary to advocate a psychology tied to the Neo-Scholastic movement. It is not too difficult to determine for Pace, who having been employed as a philosopher had the Wundt-inspired insight in Paris that psychology was capable of expanding the philosophical horizon. Moore, on the other hand, after performing orthodox and well-accepted psychological experiments seemed to want to achieve a more comprehensive synthesis that would fulfill his polymathic tendencies. His pioneering work in establishing clinical training and service was well served by this outlook. But when it came to psychological theorizing his denunciation of all contemporary points of view put him in an isolated position. Viewpoints other than his own were dismissed not for methodological faults but because they lacked a correct, largely historical interpretation. In striking contrast, the two most prominent European Catholic psychologists, both contemporaries of Moore, adopted the mainstream position, which is almost universal today, that psychology should be severed from metaphysics and philosophical constraints.

Both by example and publications, they argued that Catholic psychologists do not need a special Neo-Scholastic psychology.

One of these European psychologists was the Italian priest Agostino Gemelli who studied psychology first with Friedric Kiesow in Turin and then, like Moore, with Kulpe in Bonn and Munich. As was the case with Moore, Gemelli took on heavy administrative duties when in 1921 he was appointed rector of the newly opened Catholic University of the Sacred Heart in Milan, the first Catholic university in Italy. Gemelli was a strong promoter of Neo-Scholasticism in Italy. "The main aspect of Gemelli's whole philosophical work is his stand against positivism, and his endeavors to revive and establish Neo-Scholastic philosophy in the tradition of the Louvain movement" (Misiak & Staudt, 1954, p. 139). Notwithstanding these impeccable credentials, however, no stronger opponent to the attempt to establish a *Neo-Scholastic psychology* can be imagined. This view is elaborated in his paper *On the Relationship Between Psychology and Philosophy* delivered, appropriately enough, at the International Thomistic Conference in 1936. Excerpts include the following admonitions: (Misiak & Staudt, 1954).

> Psychology was born in the womb of philosophy, but now it should finally free itself, once for all, from the constraining bonds of philosophy, and remain an autonomous science. Many psychological schools had their theoretical foundations impregnated with explicit or implicit philosophy, and this was detrimental to their scientific pursuits. As soon as the unity between bodily and psychic activity in man is admitted, psychology can have its object and method without the necessity of referring to philosophical doctrines. . . . [Further, in an exact literal translation:] "I consider psychology a science, no more nor less than every other experimental science; and I add that we must step up all the more the process of liberation of this science from philosophy, of which just a short time ago psychology was merely a modest chapter. We must sever completely the final bonds which are still attached, so that completely freed psychology may develop without obstacles in its path" (p. 146).

The second outstanding European Catholic psychologist of Moore's era was Albert Michotte who studied at Louvain and then like Pace studied with Wundt at Leipzig and with Kulpe, who was then still at Wurzburg. In 1905 he returned to the University of Louvain as a teacher, and in 1912 became a full professor. As a student at Louvain, Michotte had Cardinal Mercier as a teacher and afterwards said that his influence "was determinative in his intellectual and human development." Michotte favored a phenomenological framework for his numerous experimental investigations which brought him friendship with the Gestalt group, although he was never a member. His work was not widely known in America until the publication of his book, *The Perception of Causality*, in 1946, but for years he had been friendly with the leading psychologists in America. His

work was appreciated by psychologists of widely varying persuasions in both Europe and America. In a commemorative volume for a celebration of the fortieth anniversary of Michotte's professorship, tributes came from T. V. Moore, and also from L. M. Terman, E. L. Thorndike, and the arch-behaviorist W. S. Hunter. Michotte, as a student of Cardinal Mercier, and as the professor of psychology at Louvain where the philosophical institute devoted to the study of St. Thomas Aquinas was located, might have been expected to advocate Neo-Scholasticism in his teaching and the interpretation of his research. His experimental work, however, was like that of the majority of experimental psychologists free of any metaphysical interpretations. Nevertheless, Michotte was an active member of the Pontifical Academy of Sciences. He attracted many Catholic students to his laboratory, which the Danish gestaltist David Katz called "a mecca for pilgrims of experimental psychology." Among these students were Fr. John Stafford, successor to Moore as Catholic University psychology department Chairman, and another professor in the department during this period, Fr. James Van der Veldt.

Gemelli was not only a member but also the President of the Pontifical Academy of Sciences in addition to being the rector of a leading Catholic University. But, as noted, these papal honors came to Gemelli and Michotte even though both men shunned the Neo-Scholastic psychology movement. Between the turn of the century and 1939 there were, however, a number of priest-psychologists who furthered the cause of Neo-Scholastic psychology in Continental Europe. They primarily wrote textbooks, summaries, and reviews and published often acute criticism of extant theories. A difficulty in making themselves known to the wider psychological community was that they performed few experiments and gathered little data so that their writings, usually untranslated, were scarcely known in America. Neo-Scholastic psychology in Europe, as distinct from Neo-Scholastic philosophy, did not survive after 1945.

The Neo-Scholastic *philosophy* (Anable, 1941) lived on in America and for a time increased in popularity after 1945, but somehow American savants seemed to make little contribution. The French scholars, philosopher Jacques Maritain and historian Etienne Gilson were cited repeatedly, but other authorities possessing similar scholarly distinction never emerged. Eventually even in America the Neo-Scholastic philosophy seemed no longer pertinent to ongoing concerns. For some scholars there was a reaction that went further: it was suggested that this philosophy in spite of initial great hopes had been stultifying and reactionary as long as it held sway. In the fall of 1985 an article from *Commonweal* that expressed these sentiments with an undertone of bitterness (Galvin) appeared in *Envoy*, an in-house magazine of Catholic University:

> Yet Neo-Scholasticism did not suffice to address the pressing religious issues of the day. Unlike the medieval predecessors from which it drew much of its content, it was born of a spirit of reaction and closed to the world in which it lived, marked by defensiveness and hostility, it was more disposed to reject alternative views than to engage in constructive dialogue with them. Armed with a fixed catalogue of questions and answers oriented narrowly on the Church's dogmatic tradition, it tended to classify both biblical thought and patristic theology as mere precursors of its own final synthesis, to be mined for support where this was feasible, ignored where it was not. As a result of these characteristics, it was theologically unproductive and spiritually sterile.

With the withering away of the Neo-Scholastic philosophy, the attempt to forge a Neo-Scholastic psychology lost its rationale, even in America. No longer was empirical psychology united in a marriage of inconvenience to the metaphysical axioms of rational psychology. There were additional factors, but this change in outlook was certainly a strong enabling factor in establishing psychology as an autonomous discipline at Catholic colleges and universities. Numerous Catholic colleges during the sixties and seventies added psychology departments that were little different in composition and outlook from similar departments at non-Catholic colleges.

What can be said in summarizing Moore's achievements? His role as a founder of psychological service institutions under Catholic auspices was no small accomplishment. Misiak and Staudt (1954) emphasize, as just one example, that the Catholic University clinic "served as the model after which other Catholic clinics were subsequently patterned." Moore never relented in his vigorous campaign among the clergy and educators for the development and extension of the clinic movement in Catholic circles. He urged his fellow Catholics, especially his fellow clerics, to be more accepting of psychology and psychiatry. Moreover, he struggled to convince them of the acceptability of psychoanalytic methodology. He further urged them to establish Catholic schools for the retarded, one of which he himself founded. Misiak and Staudt concluded: "His versatility as lecturer, teacher, writer, psychologist, and psychiatrist made him the logical person to establish psychology firmly among American Catholics" (p. 203).

Moore's dual role of psychologist and psychiatrist was undoubtedly beneficial in allowing him to address a wider audience. But it was not so beneficial to other faculty and students with clinical interests to be in a Department of Psychology and Psychiatry that issued the publication *Studies in Psychology and Psychiatry* when the only permanent resident psychiatrist was Moore himself. In addition to his many other duties, Moore carried on a clinical practice and incorporated many vignettes from his psychotherapeutic endeavors into his writings. Thus it

might be thought that he was well placed to be a pioneer in the liberation of clinical psychology from the shackles of psychiatry. Such was not to be. In fact, it comes as a jolt when after reading some 300 pages of his first book (1924), with the title *Dynamic Psychology: An Introduction to Modern Psychological Theory and Practice*—a book which dealt with the theories of Freud, Jung, and Adler, all then living—to find a footnote warning psychologists away from clinical endeavors: "It should be noted that psychotherapy should not be practiced except by one who is a properly qualified physician if serious blunders are to be avoided" (p. 304). Freud himself was broader-gauged in not restricting the practice of psychotherapy solely to physicians when he wrote his short book *The Question of Lay Analysis* in 1926, two years after Moore's admonition.

The establishment of a counseling center whose members collaborated with the Department of Psychology and Psychiatry came in 1948. Soon after, the department was approved for the training of clinical psychologists by the Veterans Administration and in 1950 became the recipient of training grants from the U. S. Public Health Service. These events occurred under the tenure of Moore's successor, Fr. John W. Stafford. The concept of clinical psychology supported by government funds was exclusively a postwar phenomenon that allowed clinical psychology to establish itself throughout the U. S. But undoubtedly, Catholic University with its well-established university clinic had a head start, one that expressed itself in establishing the first clinical psychology graduate training program in the entire Washington, DC, area. It would be interesting to know what Moore in the Spanish monastery thought of this rush to train nonmedical psychotherapists. Perhaps he changed his mind.

The vision of Pace and spirited efforts by Moore in attempting a synthesis of psychological results that would both support and amplify the perennial Catholic philosophy can be seen as a laudatory and idealistic effort, although ultimately a failure. The plausible rationale had been that the Christian philosophy of the medieval mind was once strengthened and elaborated by the adoption of the thought of Aristotle, so why after the passage of centuries could not a similar remodeling take place again. Rather than reinvigorating the perennial philosophy through ancient manuscripts, what could be more in touch with our own times than gaining data from laboratory experiments and therapeutic efforts that would enlarge and, to a limited extent, revise our conception of the human situation. Great hostility was, as I have shown, felt toward this scientific intruder, experimental psychology. In hindsight this is surprising, since experimental psychology as a new endeavor, limited itself to sensory and perceptual experiments. On the other hand, it is just this part of psychology that possesses the best claim to being a rigorous science in the traditional sense, and therefore its practitioners are the most likely to find attempts to attach either metaphysical

assumptions or interpretative guidelines gratuitous and irksome. The philosophical interpretation of fairly straightforward laboratory findings continued with Moore in later years when psychology branched out into social and personality areas. As previously detailed, the leading Catholic psychologists in Europe, Gemelli and Michotte, who held more important positions at European Catholic universities and received greater recognition from the Vatican than any psychologist at The Catholic University of America could aspire to, had no difficulty separating psychology from philosophy, even while arguing in behalf of the Neo-Thomistic philosophy, *qua philosophy*.

Summing Up

The years immediately following Moore's retirement in Spain were to result in a rapid secularization and/or normalization—depending on one's point of view—of the psychology department. After Stafford's retirement in 1959, Dr. James P. O'Connor, the first lay psychologist, became Chairperson, and soon, as with many universities, the Chairperson position was on a rotating basis. By the late 1960s, the department consisted solely of laypersons. The clinical institutions that Moore had founded were mostly separated from the university or discontinued at the behest of the permanently tenured faculty member, Financial Exigency. For a time, the Child Center was a well merited exception. The Child Center, in addition to psychology faculty members, had a consulting psychiatrist and for several years continued to offer a psychiatric residency. But even before the Child Center was closed as a budget-tightening measure, new stationery was ordered and a name change was made to "Department of Psychology."

The accomplishments of the department during the last 30 years will not be described here. Suffice it to say, although psychological theorizing has been recognized by the psychological community as an important contribution on the part of some recent department members, no one has attempted the enormous task of giving the whole of psychology a metaphysical grounding, as was advocated by Pace and Moore. Psychological history books describing psychological systems and theories published after the 1950s ceased mentioning the possibility of a Neo-Thomist or Neo-Scholastic psychology, and at the Department of Psychology of the Catholic University no memory of such a comprehensive enterprise exists. Neo-Scholastic psychology had ceased to be an effective force some years previously in spite of the best efforts of Moore and others. Perhaps the death knell was sounded in 1941 when the leading non-Catholic, "return-to-Aristotle" philosopher, Mortimer Adler, wrote in the preface to Robert Brennan's *Thomistic Psychology* (1941), "the edifice of psychology will not be moved from these foundations." The historical course of psychology has

never been predictable, even by the compiler of the encyclopedia of great ideas, the *Synopticon* (1952).

The ending of the Pace-Moore era changed the mission of the Psychology Department in line with the decline in parochial attitudes of the American Catholic Church. The Psychology Department, like the Catholic University in general, was no longer considered the graduate capstone of Catholic education. The pioneering days of introducing psychology as an academic subject and the mission of training psychologists to provide faculty for American Catholic colleges and universities was over. At an earlier time, the Psychology Department had been

> the model after which several of the early departments of psychology at Catholic colleges and universities were patterned. Furthermore, it was the first American training center of many teachers who staffed the new psychology departments at these Catholic colleges and universities. From The Catholic University in Washington, DC, the experimental psychology of Wundt originally radiated to Catholic circles throughout the United States (Misiak & Staudt, 1954, p. 82).

Present-day psychology in America with more than 100,000 practitioners of wide variety is an enterprise perhaps inconceivable when Pace became one of the first 31 members of the American Psychological Association. The department today is part of the academic mainstream in its three graduate programs of clinical, developmental, and applied experimental psychology. Graduates of these programs are located around the country and in foreign countries in every type of college and university, public and private research institution, and professional service facility. Their placement prospects are in a national market, not a Catholic market in which they are favored by virtue of a special type of preparation. Faculty members strive for and achieve, in varying degrees, success in reaching interested colleagues in particular specialist areas. It is safe to say, however, that no psychologist will in the future have to contend with the ideological opposition that Pace met with in advocating the teaching of psychology. Nor will anyone ever match the energetic restlessness of Moore in founding psychologically-based service centers and taking the whole of theoretical psychology as his province—with, to be sure, his learned corrections liberally dealt out to theorists of all persuasions. Although shrewder theoretical propositions can often be gleaned from Moore's negative criticisms than from his experimental work, his goal was a positive one that remains a laudable ideal. Moore (1939, p. 604) wrote as the last sentence of his *Cognitive Psychology*: "And so psychology, by fairly facing problems and facts, will rise above the atomism of sensationalism and materialism, and will attain to the concept of the human person as a living, intelligent, substantial being."

Acknowledgment

This article is dedicated to one of the author's informants, the Department of Psychology's senior member, Professor Emeritus Maurice Lorr.

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A History of the Department of Psychology at Howard University

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ABSTRACT

A brief history of the Psychology Department at Howard University is presented. Some background information is reported about the University, important early professors, and some prominent graduates. How being in a black university affected the Department's activities is described.

The First Congregational Church of Washington, D.C. was established in 1865 and it still stands at 10th and G. Streets, N.W. In a series of meetings at that church in 1866, Howard University was conceived. The Congregational parishioners who founded Howard planned it as a university for the freedmen and their children. Yet when classroom instruction started on May 1, 1867, the student body was 100 percent white: four daughters of these first Howard professors. Within a few years of its opening, Howard started to receive the students for whom it was intended, but their number in the College of Liberal Arts was quite small. Dyson (1941), in his history of Howard remarks:

> When the College of Liberal Arts of the University opened formally on September 21, 1868, it opened with one student in a class of Greek and Latin. And for more than thirty years, the classics were emphasized primarily. It is surprising to learn this. That a university for ex-slaves should emphasize Greek and Latin seems today, very, very, foolish. (p. 156)

That emphasis on the classics might explain why the first psychology course at Howard was not offered until 1899. Listed under Philosophy and called, Psychology: the Briefer Course, it was the only course provided until 1906. It is probable that William James's *Psychology: Briefer Course*, published in 1892, two years after the two volume *Principles of Psychology*, was the text for this class. Starting in 1906, other courses in psychology were added, but there was no department of psychology until 1928.

These early courses in psychology were mostly in the service of teacher education and had a very applied cast. Psychology's relation to education at Howard was similar to those relationships at many U.S. colleges during psychology's early years. A major purpose of these colleges was to train teachers. That was surely a major purpose of Howard, whose Normal Department's announcement in 1867 stated: "The primary object of the Normal Department is to prepare teachers for the colored population." (Logan, 1967, p. 33)

To place psychology in the context of courses offered by the University, it must be remembered that Howard was established as a private, not a public, institution by white and black former abolitionists. These founders had as a model for Howard, private universities in the northeast. Most of the early faculty at Howard were graduates of these private, northern universities. If these schools taught Greek, Latin, Philosophy, and now Psychology, Howard would also have these courses in its curriculum.

Professor Lewis B. Moore, one of the earliest black Ph.D. graduates in the U.S. (from the University of Pennsylvania in 1896), taught the two courses of which the psychology curriculum comprised between 1911 and 1917: General Psychology and Advanced Psychology. Dr. Moore was Professor of Latin and Pedagogy and later became Dean of the Department of Pedagogy. During the next five years, two psychology courses were added: Social and Abnormal Psychology. All four courses in the psychology curriculum were taught in this period by McLeod Harvey who was also Professor of Old Testament History at Howard until he left in 1923.

Albert S. Beckham, with a master's degree in psychology from Ohio State, came in 1924, the first person to teach psychology at Howard whose education had been primarily in that discipline. Beckham founded a psychological laboratory at Howard and taught all the courses in psychology. He left Howard in 1928 to continue his studies in psychology at New York University where he received the Ph.D. in 1930. His dissertation was one of the earliest of a number of studies of intelligence of black children done by black psychologists in response to the prevailing psychological reports of below average black intelligence. His data showed that black children in the public schools of Washington, D.C., Baltimore, and New York City were at the same IQ level as national norms (Guthrie, 1976).

With the arrival of Francis C. Sumner at Howard in the fall of 1928, a Department of Psychology was formally established. In *Even the rat was white*, Guthrie (1976) called Sumner the "Father of Black American Psychologists." Sumner deserves this designation because the great majority of American black psychologists who earned Ph.D. degrees before 1950 were either undergraduate or master's level students of the Howard Department of Psychology. In 1930, Sumner,

who received his Ph.D. at Clark University in 1921, was joined by another Clark Ph.D. (1926), Max Meenes. Sumner, Meenes, and a young M.S. graduate of Howard, Frederick Watts, formed the nucleus of Howard's Psychology Department for the next fifteen years.

Bayton (1975), in a brief biography of Sumner, describes a broadly educated person with an encyclopedic knowledge of psychology and a non-charismatic teaching style that, nevertheless, inspired many students to careers in psychology. Sumner entered Lincoln University (Pennsylvania) in 1911 at 16 by passing a written examination, because he had no high school diploma, having received, according to his employment application at Howard, "private instruction in secondary subjects by father." Sumner graduated from Lincoln in 1915 *Magna cum laude* with special honors in English, Modern Languages, Greek, Latin, and Philosophy.

Sumner began graduate study in psychology at Clark University in the fall of 1917 after receiving a fellowship in psychology from G. Stanley Hall. Sumner had written Hall that he wished "to study race psychology" (Guthrie, 1976, p. 179). Although he did not study race psychology with G. Stanley Hall, the issue of race was an important one for a time in Sumner's graduate career at Clark. According to Guthrie (1976), Sumner wrote several letters to local Worcester newspapers strongly criticizing the U.S.—during the peak of its involvement in World War I—in a psychoanalytic interpretation of racism in America. These letters aroused the local citizens to demand an investigation of Sumner's loyalty by the postal authorities. To restore him to good standing, it took a letter from Sumner apologizing for his "disloyalty to my native country" (Guthrie, 1976, p. 180) and a memorandum from G. Stanley Hall explaining that Sumner's actions were, in fact, reactions to harsh incidents of prejudice based on race, some of which he (Sumner) had experienced personally.

Instead, then, of studying race, Sumner's dissertation was, "The Psychoanalysis of Freud and Adler." His major professor was G. Stanley Hall. The other members of his Ph.D. committee were E.G. Boring, J.W. Baird, S.W. Fernberger, and K. Karlson. The topics of Sumner's dissertation and the majority of his research with his master's students at Howard are not about race. That, however, reflected the catholic interest of Sumner rather than any lack of concern about psychological issues of race. Bayton (1975) points out that Sumner was continuously concerned with psychology and black life. Several M.S. theses, done under his direction, were on topics that would, in the current terminology, fall under psychology and the black experience.

Psychology at Howard in the 1930s and 1940s was influenced by Clark University and G. Stanley Hall through Sumner's teaching. Yet G. Stanley Hall had an earlier and more direct contact with Howard. In 1872, Hall applied for a

position as a chemistry teacher at Howard. A facsimile of Hall's application letter in Dyson (1941) has him writing that he had a "strong preference" for the university. It is well-known that Hall's views on race went through several shifts in his lifetime.

Hall, an abolitionist, "both by conviction and descent," recanted those abolitionist views, "I wish to confess my error of opinion in those days" (Hall, 1905, p. 104). An example of his changed opinion is seen in this observation of Hall's (1905):

> Another social trait of the negro is found in the sphere of sexual development. . . . The negro child up to about twelve is quite as bright as the white child; but when this instinct develops it is earlier, more sudden, and far more likely permanently to retard mental and moral growth than in the white, who shoots ahead. (p. 102)

The final irony here is that G. Stanley Hall, probably the leading luminary of early American psychology and whose terminal views about black people seemed quite negative, became, through his key financial and moral support of Francis Sumner, an important, if indirect, influence in the development of black psychologists.

Further evidence of the concern of Howard's psychology department with black issues during the 1930s was the research of M.S. students under the guidance of Max Meenes. Meenes, like Sumner, was a Clark Ph.D. and, like Sumner, was a person of comprehensive interests. He had studied with Alfred Adler, E.G. Boring, Walter Hunter, Kurt Koffka, Wolfgang Kohler, and E.B. Titchener. Meenes had broad experience in almost all of the psychology of that time. He was a general psychologist when it was still possible to be a "specialist" in general psychology. The range of topics which his master's students studied was a good indication of his breadth. Eidetic imagery, retinal rivalry, whole and part methods of learning, racial stereotypes, intelligence tests of Negro school children, and self-concepts of children were a few of the topics of the M.S. theses that were later published.

Mamie K. Phipps (later Mamie K. Clark) wrote a 1939 master's thesis directed by Meenes, entitled, An investigation of the development of consciousness of distinctive self in pre-school children. This study of 211 pre-school children (a number several times larger than that typical of today's master's studies) in Washington, D.C. and White Plains, Mount Vernon, and New Rochelle, New York was the genesis of the Clark (Mamie) and Clark (Kenneth B.) "doll studies" whose results are believed to have played an important part in the 1954 Supreme Court decision that segregated public schools are illegal and must be desegregated.

Both Kenneth and Mamie Clark had distinguished careers in psychology that

began with bachelors' and masters' degrees at Howard. Both earned their PhDs at Columbia University and worked in New York City: Mamie Clark at the Northside Clinic (which she and Kenneth Clark founded) and Kenneth Clark as a Professor at the City College of New York. In addition to his prominence as a social psychologist, Kenneth Clark, in 1970, was the first black president of the American Psychological Association. There were none before and, so far, none after.

Although Kenneth B. Clark is the best known of the psychologists who were undergraduates in the Howard University Department, two other graduates of that 1935 class, James A. Bayton and Carlton B. Goodlett, achieved national prominence after becoming Ph.D.s in psychology. Goodlett became the first black graduate student in psychology at Berkeley, and the first to receive the Ph.D. at the University of California in 1938. After four years of teaching psychology at several black colleges, Goodlett became an M.D. and left for San Francisco. There, he practiced medicine, published a newspaper, and ran in the primaries for governor; becoming one of the most visible black figures in San Francisco. After receiving an M.S. from Howard in 1936, Bayton went on to earn the Ph.D. in 1943 from the University of Pennsylvania. He taught in the Howard Department from 1947 until his death in 1990. His long career was marked by many outstanding accomplishments and recognitions of his achievements. One of the most appropriate and deserved acknowledgements that Bayton received was the Distinguished Teaching Award of the American Psychological Association in 1981.

After World War II, Howard, like most other colleges had a large influx of veterans. Psychology became a more popular major. For example, in 1937, there were 40 psychology majors among all of the undergraduate students, while in 1949, there were 88 seniors who graduated with the B.S. in psychology. The number of bachelor's level graduates has not been that large since. It has averaged 60–65 a year over the last 15 years. The number of undergraduate majors has held steady at about 340–390 during the same years. Faculty numbers have gone from six in 1947 to 22 in 1986.

While psychology, as a major, had become much more popular with undergraduates, it was in graduate programs that huge increases in students from pre-World War II levels took place. That was especially true at Howard. Until 1968, Howard's graduate program in psychology consisted of courses offered only in the evening and leading to the master's degree. The Ph.D. program in psychology began in fall 1968; the program switched from evening to full-time in the day. Enrollment went from an average of 20–25 graduate students in the 1960s to 85–95 in the 1980s. There were 96 graduate students in the fall of 1992.

A Ph.D. graduate program in clinical psychology began at Howard in 1972.

As is true for many departments in the U.S., clinical psychology is the choice of most psychology graduate students at Howard. As is becoming true for psychology departments in America, most of our students are women; of the 96 graduate students, 68 are women. Since 1973, 102 students have graduated with the Ph.D. in psychology.

Twice in the last 25 years, large increases in faculty occurred: five new faculty were hired in 1968 and five more were added to this number in 1980. These professors made Ph.D. concentrations possible in the following areas: biological, clinical, developmental, experimental, personality, and social psychology. Among the professors hired in 1968 were Nissim Levy, Martha T. Mednick, and Sherman Ross. These were experienced professors with a history of training graduate students before arriving at Howard. Levy and Ross have since retired, but both were major advisors to many doctoral students during their time at Howard.

Martha Mednick has become a major figure in the psychology of women since her 1968 arrival at Howard. She has been president of APA's Division of the Psychology of Women and has edited three books on the psychology of women. She has sponsored many studies of career choice and professional activity in black women, supervising several doctoral students who after graduation are continuing research on the psychology of black women.

In 1980, five black, male psychologists were hired. That only men were hired was fortuitous, not intentional. Not so fortuitous, however, was the hiring of black professors. While the human research studies at Howard always had black participants, there were times in the Department's history when black researchers were a distinct minority. For example, in 1956 the six person Department had one black and five white professors. These lop-sided numbers, in a still largely segregated society, were not because a black university was discriminating racially by not hiring black psychology professors. Rather, there were no black professors to be hired. By 1980, young black professors were available. By that year also, black consciousness was well established at the University, and though not the most prominent feature or perspective of human research in psychology, it had not gone unnoticed. The Department brought in Curtis Banks, Wade Boykin, Alfonso Campbell, Jules Harrell, and Stanley Ridley (since resigned). Banks and Boykin left tenured positions at Princeton and Cornell, respectively; the other three new faculty were just beginning their academic careers.

Banks and Boykin are each maintaining programmatic research on black issues in psychology. Banks and his students are studying the psychology of skin color in black populations. Boykin has several students investigating cognitive and motivational styles of young black children and he is also working on

contrasts in Afrocentric and Eurocentric perspectives and personality correlates in black populations.

Alfonso Campbell, a neuropsychologist, has been studying, with the help of several graduate students, neuropsychological norms in black patients with lateralized brain lesions. Jules Harrell, a psychophysiologist with a special interest in personality variables, is studying the reactions of black subjects to psychological stress brought on by racially-noxious stressors.

These brief descriptions of faculty and their research give some flavor of the Howard Department. There are many other faculty who also could have been listed. Yet research was not the major mission at Howard. Howard, throughout its existence, has been primarily a teaching university. That is why much of this document is about the teaching of Sumner, Meenes and Bayton. These professors taught five courses per semester for most of their long careers. It was not until 1961 that the teaching load at Howard was reduced from five to four courses per semester in the College. The normal load continues to be four courses per semester!

Francis Sumner, who shaped the foundation of the Department, as a teacher, was a continuous mentor to Kenneth Clark, James Bayton, and many others. He was a significant person in the development of their careers, continuing in this role beyond their student days at Howard. This version of the professor—discovering, developing, and motivating talented students—has been the major accomplishment of the Howard Psychology Department.

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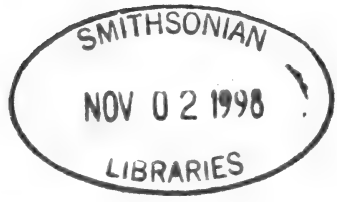

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Nanoscience and Nanotechnology[1]

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ABSTRACT

An overview is given to major developments in nanoelectronics, new device concepts, physical modeling and computational techniques in nanoscience and nanotechnology.

Introduction

Modern electronic materials fabrication techniques utilizing controlled particles and energy beams coupled with etching and masking technology, such as molecular beam epitaxy (MBE) and advanced lithography, have provided means to fabricate small structures with resolution already approaching the atomic scale. The advent of scanning tunneling microscope (STM) and atomic-scale fabrication using the principle of STM have added a new dimension in the fabrication towards atomic sizes (García, 1992; Dobisz, et al., 1991; Marrian, et al., 1987; McCord & Pease, 1986). The term nanofabrication is appropriately applied to modern epitaxial growth and lithographic techniques that are capable of making artificial structure with 10 nm feature size or less.

In the late seventies, the ideas of mesoscopic physics and nanoelectronics were borne, encouraged and stimulated by intriguing theories (e.g. localization in lower dimensional structures) and speculations regarding the novel physical phenomena that can be observed in finite "atomic-scale" structure which can be fabricated (Buot, 1986, 1987a; Anderson, et al., 1979; Dolan & Osheroff, 1979; Krumhansl & Pao, 1979; Physics Today, 1979). The distinction between mesoscopic physics and nanoelectronics is used here to reflect an idea, regarding the treatment of transport problems, similar to the distinction between solid-state

[1] Paper to be presented at conference on science and technology for developing countries. Manila, Philippines, July 12–13, 1993, Entitled "Mesoscopic physics and nanoelectronics: nanoscience and nanotechnology."

physics and solid-state electronics. Moreover the electronic community sees a promising potential application of nanostructures and nanostructure physics for the continued downscaling of device size, system architecture and intercon nects. A rapid growth of mesoscopic physics and nanoelectronics really started around 1985, with mesoscopic physics largely dominated by the IBM-related community (Likharev, 1988), and nanoelectronics integrated circuits (IC) research by the Texas Instruments (1989), geared towards planar IC layout, and by AT&T Laboratories (Capasso, et al., 1989) and Fujitsu (Yokayama, et al., 1985), who are mainly involved in discrete "mesa" nanodevice design and circuit demonstrations.

The electronics device community has been witnessing a strong and definite trend in the miniaturization of integrated circuits and components towards the atomic-scale dimensions. Today conventional devices with feature size below 100 nm are made and function well. There are, however, beginning to be reliability-related issues and problems, for conventional devices in smaller dimensions, which point to other directions for continued down scaling of electronics devices and integrated circuits. When transport dimension reaches a characteristic dimension, namely, the charge-carrier inelastic coherence length, and the charge-carrier confinement dimension approaches the Fermi wavelength, then classical device physics based on the motion of particles and ensemble averaging are expected to be invalid; the wave nature of the electrons, discreteness of energy levels and sample specific properties must now be taken into account The main goal of nanoelectronics is to sustain a continued downscaling of IC, resulting in more complex functions per chip. These goals can be accomplished by scaling down each transistor. However, the circuit interconnects do not scale down with device dimensions, and may actually dominate the IC delays Thus, circuit interconnects would present a formidable problem at some point in the continued downscaling of transistors, whose solution may lie in the development of information-based physics of coupled quantum devices. This may lead to novel device concepts and IC architectures. The nanoelectronics community expressed optimism that if all these goals succeed, supercomputer could be realized in a single chip.

Conventional IC technologies will definitely reach a limit and cease to function properly for a number of reasons: (1) the device physics in conventional ICs obey the law of large numbers and/or thermodynamic limits; (2) device dimensions are large compared to coherence lengths so that energy quantization, quantum interference and discreteness of charge carriers do not play a significant role in device transport physics. Thus, in order for the downscaling of electron devices to realize nanoelectronics one must address quantum transport, tunneling and interference effects, and discreteness of electron charge in

small semiconductor structures. The investigation of nonequilibrium phenomena in small structures is the common denominator between the research community from mesoscopic physics and nanoelectronics. Indeed, the nanostructure science field offers unprecedented opportunities for both fundamental scientific research and technological breakthroughs.

Several reviews on small structures have focused on nanofabrication and/or mesoscopic phenomena. In this paper, we present an overview of nanoelectronics, focusing on new device concepts, physical modeling and computational techniques.

Nanoelectronics

Nanoelectronics research aims in applying the novel mesoscopic phenomena towards the goals of continued down-scaling of IC components down to the atomic scale and the consequent upscaling of computational complexity per chip (Bate, 1989). Nanoelectronics is highly interdisciplinary and may eventually need the cross disciplinary approaches based on the mathematical, physical, chemical and/or biological sciences to fully implement its goals (Hopfield, 1990). This involves the study of cognitive symbolization, their interactive relations, and computational aspects of the natural laws of the physical and life sciences.

The most useful device in solid-state electronics, which has brought the information revolution, is the transistor. The transistor is basically a device which accomplishes two very important functions, namely, "control" and "drive." The "drive" function is accomplished by applying a large voltage difference across two terminals of the device, known as the source (emitter) and drain (collector). The "control" function is implemented by simply controlling the self-consistent potential barriers to current flow between the two terminals. Both functions are important in obtaining the "gain" of the device. Purely electronic control of the self-consistent potential barrier to current flow between the source (emitter) and drain (collector) is accomplished in two general ways:

1. by directly introducing a biasing (control) charge in the way of the current flow, i.e., inside the transport channel to modulate the self-consistent potential barrier. The biasing charge or current comes from an electrode in contact with the "base" region in the transport channel,
2. by depleting the charge carriers in the channel which leads to the creation of potential barrier induced by a voltage applied at the "gate" electrode.

Conventional transistors employing the first method above are called bipolar transistors and falls into two categories, i.e., the p-n-p transistor and the n-p-n transistor. In both cases, the biasing charge arise from the base-emitter current

and has an opposite charge from the current-carrying charge from the emitter to collector. Conventional transistors employing the second method are called unipolar transistors or field-effect transistors. Thus an electronic device which delivers voltage gain and current drive is basically a three-terminal device. Note that the role of the power supply is essential to obtain gain (Buot, et al., 1988). Because of the major role of lateral depletion regions in a conventional bipolar devices, these devices are not scalable compared to field-effect devices. On the other hand, bipolar transistors, are inherently high-speed devices compared to field-effect transistors by virtue of their low base-emitter capacitance. The prominence that three-terminal devices have in the electronics area is due to their ability to transmit and amplify (gain > 1) information signals over indefinite distances without attenuation, and to provide good isolation between input and output of a logic gate allowing a computational process to proceed in a predetermined fashion.

The major attraction of resonant tunneling devices (RTDs), and other proposed quantum-based devices, is their ultrasensitive response to voltage bias in going from the high-transmission state to the low-transmission state. If these devices are able to operate under high bias and far-from-equilibrium condition, this essentially means that a very high transistor transconductance and ultra-fast switching are obtainable. Indeed, numerical simulations of RTD, to be discussed later, and microwave experimental results, indicate the intrinsic speed limit of RTD to be in the tera-Hz range. This high-sensitivity to bias in far-from-equilibrium operating condition can inevitably lead to a very high gain. Moreover, there is a strong indication that the gain can further be improved by appropriate design of the source and drain resistance.[2]

However in the continued down scaling of three-terminal device sizes and consequent upscaling of complexity per chip, a "wiring crisis" will result and is essentially characterized as follows: (a) the number and length of interconnections will scale up and (b) the cross-section of the wire will have to scale down to allow more communication paths per area in the chip. The first will offset the benefits of faster device switching, since long interconnections create delays. The second will aggravate the problem of connection delays since the narrower the wire the larger is its resistance. In fact, for nonideal small metal wires, the resistance can go up exponentially as a function of its length. It is not yet clear whether research is novel computer architecture based on three-terminal devices and multi-valued logic will eventually solve this problem.

A direct approach to the interconnect problem is to scale down the intercon-

[2] The absolute value of the effective NDR was found to increase as a result of the voltage drop across the series resistors, creating a distortion of the I-V Characterictic as calculated in reference (Buot & Jensen, 1990).

nects as well, from the conventional architecture, by eliminating long interconnects per chip, through computerized circuit-layout optimization, e.g., through the use of simulated annealing techniques. Another realistic approach may lie on the advances made in monolithic IC optical communication technologies, such as the one recently proposed by Yamanaka, et al. (1991) using optoelectronic integrated circuit (OEIC) chipset for chip-to-chip optical communication. Elimination of wiring interconnects, and/or interconnects whose number does not scale up with computational complexity per chip, is contained in various proposals which drastically employs very different architectures. Texas Instruments has proposed the use of cellular automaton architecture (Bate, 1989; Texas Instrument Technical Journal, 1989), in which the devices are connected only to their nearest neighbor. A more detailed proposal is explained in (Lent, et al., 1992). What is intriguing about this architecture is that the coupling is not implemented by physical "wires" but accomplished through capacitive coupling between, say, neighboring quantum dots. However, the "forces" and "rule" of the new computational dynamics appropriate to a cellular automation architecture that correspond to the Boolean logic, "drive" and "gain" (coupling) in conventional transistor-based general purpose computers are not entirely clear. General purpose computing requires arbitrary stored programs and data as part of the initial configuration of the computer computational dynamics, and an unlimited depth of computation. Cellular automata (CA) architecture seems to lack the ability to undertake an unlimited depth of processing by virtue of the presence of the inherent feedback mechanisms. Notably, the ability to tailor the deterministic computational dynamics in a general-purpose computer according to an arbitrary program stored in the memory is not even addressed in all the proposals for the CA architecture. Nevertheless, the ability to represent a bit of information by one electron in quantum cellular automaton circuit is very intriguing, and clearly represent the ultimate efficiency in information representation. General-purpose computer design based on coupled CA building blocks, with OEIC chipset optical communication between CA blocks, also need to be explored. In any case, quantum cellular automaton architecture idea should open up strong interests in information-based mathematical, physical and biological sciences to establish the necessary analytical knowledge base needed for the search for a new general-purpose computational dynamics (Hopfield, 1990).

Nanodevices

Current research effort in nanoelectronics are centered on nanodevices, more specifically on nanotransistors. These are essentially three-terminal nanode-

vices. Research effort directed to a new general purpose computational dynamics and drastically novel computer architecture to avoid the wiring crisis is still at the embryonic stage of development (Bate, 1989; Texas Instrument Technical Journal, 1989).

Resonant Tunneling Devices

Nanodevice research is centered on the utilization of resonant tunneling switching phenomena and quantum interference (Bragg interference, as in periodic crystals) phenomena in superlattices, for enhanced inelastic coherence lengths and high-effective charge-carrier mobility (Sakaki, 1992). Resonant tunneling phenomena have attracted the attention of the device community basically in the form of two-terminal GaAs/AlGaAs/GaAs diodes with significant voltages applied at the source and drain (Bonnefoi, et al., 1985; Sollner, et al., 1984; Tsu & Esaki, 1973; Chang et al., 1974). The resulting quantum transport process is far from equilibrium and highly nonlinear. The really attractive feature is that resonant tunneling at these voltages is observed even at room temperature. In order to make nanotransistors, one simply needs to control the current flow by any one of the two general purely electronic methods, used in conventional transistors, to control the alignment of the resonant energy level with the Fermi level of the source (emitter). It is clear that obtaining gain would pose no problem, even at room temperature. As a corollary, a novel quantum transport process has to operate in far-from-equilibrium two-terminal diode format in order to have a clear potential for use in a three-terminal transistor with a reasonable gain (Buot, et al. 1988).

A common wisdom in the IC community can be expressed as follows. In order for a transistor to have a potential for insertion into a general purpose programmable computer system, the input-output transfer characteristic of a logic gate must have an unstable midpoint or "fixed point" where "low" state and "hi" state can no longer be discriminated and a stable limit cycle where complete discrimination of "hi" and "low" states occur. This condition is necessary to have a restoring logic gate.

The variability of input signal will become a critical issue, as pointed out by Landauer (1989), for RTDs proposed for a general purpose computer. Landauer noted that, unlike conventional transistors, the most fundamental problem arises from the fact that in quantum-based devices high-transmission conducting state occurs only for particular values of the input signal and for particular values of the structural and physical parameters describing the device structure. Tunneling current depends exponentially on the thickness of the barriers [moreover, symmetric and asymmetric barriers may have completely different I-V characteristics at very low temperatures (Eaves, et al., 1990), and high-transmis-

sion voltage depends on the thickness of the quantum well. On a related vein, any proposal which advocates the use of multi-valued logic over the binary logic appears to aggravate the signal tolerance and discrimination problem. Clearly, the high-demands placed on manufacturing control are expected to be a challenging manufacturing problem which will continue to confront researchers in quantum tunneling devices and ICs. We estimate that cascaded logic circuits with well-fabricated RTDs, employing binary logic, will have about 20% of the total-voltage-swing signal tolerance, compared to about 50% of the total voltage swing in conventional CMOS. Variability of the RTDs would call for a much larger signal tolerance, if this is at all possible. All these seem to add up to a challenging set of device specifications and manufacturing-yield uniformity.

To our knowledge, RTD integrated circuits have, so far, only been experimentally demonstrated initially in the form of "uncoupled" circuit configurations, i.e., as single-stage input/output circuit configurations (Capasso, et al., 1989). Indeed, single-stage analog and digital-logic circuits have satisfactorily been demonstrated throughout the world, e.g., as frequency multiplier/divider, as exclusive NOR gate, as parity generator, as multi-state memory, and analog-to-digital converter (Capasso, et al., 1989). Recently up to two stage logic coupling has been demonstrated, i.e., two-stage XNOR logic gate combination in the form of a full adder (Takatsu, et al., 1992; Imamura, et al., 1992) circuit. It is most critical for the future of RTD applications to general purpose computer, that as the technology for RTD-based IC further develops, a satisfactory experimental demonstration of coupled and closed-loop array of input-output logic stages must be realized to physically simulate a very long computation. Indeed, a ring oscillator IC of inverter-logic-gate chain with fan-out and fan-in logic blocks should simulate an infinitely long computational process.

Schrodinger-Wave Guide and Aharonov-Bohm Effect Devices

Schrodinger wave guide and Aharonov-Bohm effect device concepts have been introduced basically as three-terminal devices from the very start, with no demonstrated "drive" and gain capability, i.e., source and drain terminals are simply current biased near equilibrium. The present emphasis placed in this research is on the novel controls, which is similar to electromagnetic wave guide devices (including "Fabry-Perot resonance" type of localization resonance), obtained upon varying the geometrical parameters of a "stub," "double constriction" or a "bend," by means of the control of the confining depletion-layer wall in portions of the stub, double constriction or bend through the voltage applied at the gate terminal. These devices also very much depend on the device size to be less or equal to the inelastic coherence length to operate. Thus they are expected to operate only at very low temperatures, for devices that can be fabri-

cated today. To provide gain, these devices must still be able to shut off at a high drain voltage (with the source at zero voltage). However, a high drain voltage will accelerate the carriers changing their wavelengths and therefore affects quantum interference in a complicated nonlinear manner, and an off-state at high drain voltage is not easily met by the electrostatic (depletion-layer controlled) Aharonov-Bohm effect devices, thereby seriously limiting the gain (Sakaki, 1992). Perhaps with ingenuous design of the waveguiding channels, reasonable gain can be obtained for these devices, particularly for Schrodinger waveguide devices with controlled localization at a stub, double-constriction or bends.

Single Electron Effect Devices

Coulomb blockade phenomena, as applied to the concept of a "single-electron dominated" transistor is expected to have an attractive drive and gain capability. They are expected to be the natural consequence of "lateral" scaling down of the present resonant tunneling transistors for ultra high density ICs. Forerunners of "single electronics" is embodied in the current proposal for quantum-coupled logic gates at TI (Bate, et al. 1989) (strictly speaking, a single-electron transport can not support "fan out"/"fan-in" between input/output logic stages).

The basic physical scale in these phenomena is the charging-energy scale. This energy can be large if C is small. For a small area junction or a small Coulomb island or small quantum well, C is proportional to the cross-section of the junction or the size of the Coulomb island and can be of the order of attofarad in GaAs-based embedding medium. Thus, the energy of charging by one electron can become significant and hence a single-electron behavior begins to dominate the carrier flow across the tunnel junction or Coulomb island at low enough temperatures, $T \sim 10$ K.

In the purely classical case, a tunnel junction only serves as a tunnel barrier between conducting wires delivering a quasi continuous supply of charge (polarization charge due to arbitrary shift of electrons and ions in the wire). The parabolic dependence of the charging energy with the charge Q and the discrete transfer of electronic charge across the junction leads to an instability for electron transfer across junction when $| Q | > e/2$, Fig. 1a. When $| Q | > e/2$, an electron transfer becomes favorable reducing the charge by one electronic charge (i.e., the states $-e/2$ and $e/2$ are degenerate and differ by one electron charge). In the presence of a current source the process of charging and recharging periodically repeats yielding a periodic voltage and charge oscillation across the junction, with average frequency equal to I/e. The range of polarization charge $-e/2 < Q < e/2$ is the Coulomb blockade range. i.e.. the transfer of electrons across the junction is energetically impossible.

The other type of single-electron dominated phenomena is much more promising for potential direct applications to nanoelectronics (Averin & Likharev 1991; Likharev & Claeson, 1992; Likharev, 1988; Kastner, 1992; Houten, 1992; Harmans, 1992). What is even more interesting is that this phenomena has been observed in small structures fabricated by simple or minor modification of existing novel heterojunction devices (also true for single-electron tunnel junction discussed above). By means of patterning the metal gates of a GaAs HEMT device structure or a Si-SiO2 MOSFET device structure (Harmans, 1992), one can implement a single-electron field-effect transistor as schematically shown in Fig. 1b, where the region between the two potential barriers defines a Coulomb island (classical) or "quantum dot" (quantum mechanical). The point is that the number of charges in the Coulomb island can be controlled by the gate voltage in a rather continuous manner by means of the polarization by the gate voltage. If we assume that the Coulomb island can have integral number of transferred electrons, i.e., quantized number of transferred charges, the polarization charge induced by the gate allows us to shift the charge in the Coulomb island in continuous manner. By adjusting the gate voltage so that $Q_o = (N + 1/2)\,e$, then U becomes degenerate for $Q = Ne$ and $Q = (N + 1)e$, i.e., it does not cost energy to add one more electron, Fig. 1c, this indicates that transport across the Coulomb island by one electron is most favorable. On the other hand when $Q_o = Ne$, it will cost energy to transfer an additional electron to or out of the Coulomb island (increase/decrease of charge by one electron). Under this condition, there will be an activation energy for a current to flow, this is the Coulomb blockade. Thus, one expects the conductance to be zero for Q_o equal to integral number of electron charge and to become large for odd-half integral number of electron charge. Indeed, experiments on Coulomb island in a 600 nm diameter size in a 2-DEG of GaAs/AlGaAs HEMT-like heterostructures have observed the conductance to go to zero periodically as a function of the gate voltage (Harmans, 1992).

The promise of single-electron transistor and the goals of nanoelectronics for ultra-dense IC's point to a serious consideration on how the inherent classically-based single-electron effects change with the reduction of the Coulomb island to quantum dot, where there are quantization of energy eigenstates in addition to the quantization of charge. The quantization of energy will surely effect the tunneling. Groups from Massachusetts Institute of Technology and IBM Thomas J Watson Research Center have observed Coulomb blockade in quantum dots (McEuen, et al., 1991). The AT&T Bell Laboratories and the University of New York at Stony Brook have also obtained Coulomb blockade behavior using quantum wells (Su, et al., 1992; Guéret, et al., 1992; Ashoori, et al., 1992). The effect of the quantization of energy on the Coulomb blockade behavior is schematically explained in Fig. 1d.

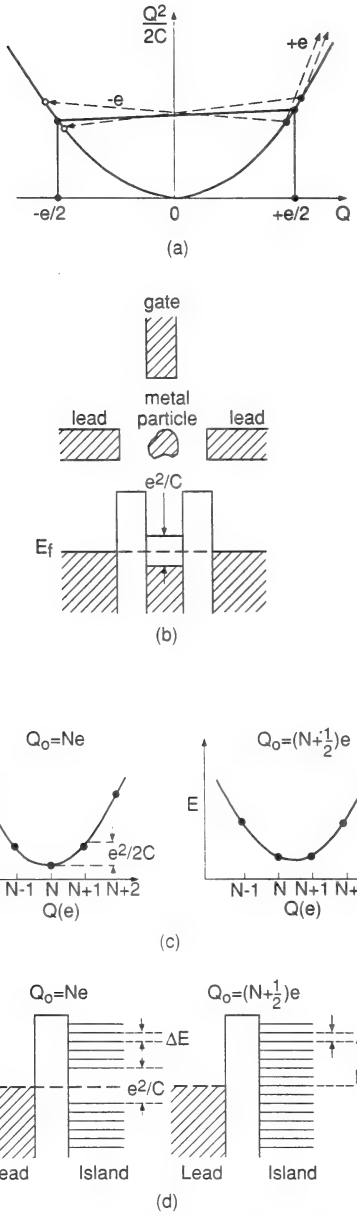

Fig. 1. (a) [Averin & Likharev, 1991 reprinted with permission.] Charging energy of small junction as a function of the polarization charge Q. The discreteness of the electronic charge e leads to the instability for electron transfer at $| Q | > e/2$.(b) [Kastner, 1992 reprinted with permission] Coulomb island and corresponding energy levels. The Coulomb island has an energy gap in its tunneling density of states. (c) Charging energy versus polarization charge Q in a Coulomb island. Q_o is the charge that minimizes the charging energy by varying the gate voltage. Because charge is discrete, only quantized values of the energy will be possible for a given Q_o When $Q_o = Ne$. there will be an activation energy for current to flow, this is the Coulomb blockade. However. when $Q_o = (N + 1/2)e$, the state with $Q = Ne$ and $Q = (N + 1)e$ are degenerate, and the energy gap of the tunneling density of states vanishes. This results in the conductance with periodic sharp peaks as function of the gate voltage, with period e/C. (d) Schematic energy-level diagram showing the effect of the quantization of energy in the Coulomb island. The conductance peak at $Q_o = (N + 1/2)e$ also requires that the Fermi level of

Nanotransistor Designs

Several proposals and experimental nanotransistor devices are based on the different ways of controlling resonant tunneling current behavior in multibarrier structures. Because of the high resolution in the process control of vertical dimensions down to 2 A monolayer uniformity, well-defined characterization and experimental results are usually obtained for nanotransistor designs based on vertical transport. However, the use of high-electron mobility 2 DEG for lateral transport-based nanos tructures have also yielded some respectable results, with added dimension of control by virtue of the ability to change confinement potentials through the manipulation of gate voltages.

The different degree of fabrication control in lateral and vertical directions has resulted in a dichotomy of advanced-electron-device research efforts. The planar-IC community efforts tend to be lateral-quantum transport based and utilize the transport of carriers in the lateral direction, whereas the second group of researchers utilizes the quantum transport of carriers in the vertical direction, across heterojunction interfaces. Indeed, band-gap engineering of materials brought about by molecular beam epitaxy (MBE) and metal-organic chemical vapors deposition (MOCVD) was immediately employed by the second group to improve the performance of conventional bipolar and field-effect transistors, yielding heterojunction transistors. The lack of abrupt confinement which resists lateral scaling, in lateral transport using confining depletion layers, urgently calls for some form of lateral heterojunction technology for nanostructures. Indeed this is an active field of nanofabrication research and this technology is vigorously being pursued (Ide, et al., 1988; Beaumont, 1989; Stormer, et al., 1992). If this technology becomes viable, quantum dots with atomic-scale dimensions and stronger coupling can be realized.

Vertical Transport Nanotransistor Designs

The obvious approach to controlling the resonant-tunneling current behavior in heterojunction double-barrier structure is to introduce a biasing charge in the quantum well, thereby altering the self-consistent potential of the quantum well. However, if the introduced electron charge occupies the same quantized energy level as the current-carrying electrons then the base-collector leakage current can become significant so as to short out the base and make transistor control impossible, as can be seen from Fig. 2.

As in conventional bipolar transistor, one needs to distinguish the introduced

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the emitter be equal to one of the discrete energy levels. This means that the gate voltage difference between the (N + 1)th and Nth peaks is equal to (e/C) + energy-level spacing/e

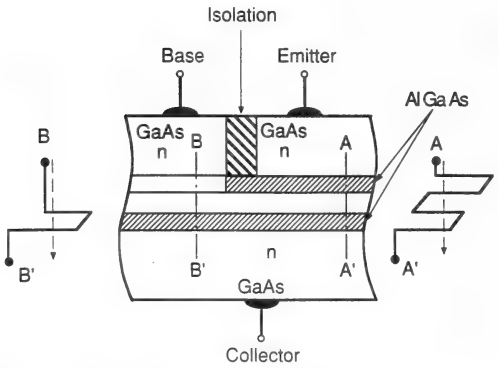

Fig. 2. [After Bate, 1989 with permission.] Unworkable unipolar RTT IC layout design. Section AA′ shows tunneling behavior, however BB′ shows parasitic base current making transistor action impossible.

charge from the main current carrying charge between emitter and collector. A way to do this is to somehow assign one energy level (e.g. ground level) in the quantum well for the charge introduced (to change the self-consistent potential) and the next excited energy level as the current-carrying channel between the emitter and collector. It is also desirable to "hide" the ground level from both the emitter and collector Fermi level, to completely eliminate leakage current. These requirements are accomplished by using a quantum-well layer made up of semiconductor material with a narrower band-gap than both the emitter and collector material, as shown in Fig. 3. It should be emphasized that this design results in a unipolar transistor, i.e., current is carried only by one carrier, the electrons.

A new set of problems is introduced when designing nanotransistors for

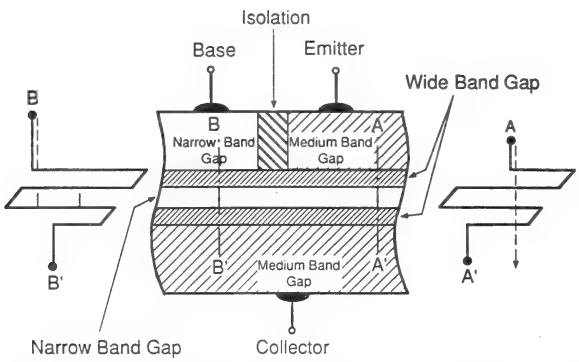

Fig. 3. [After Bate, 1989 with permission.] Workable unipolar RTT IC layout design which eliminates the parasitic base current. Emitter-collector current channel is through the excited quantum-well state. Control charge-carriers are supplied to the ground energy level by the base contact. and prevented from flowing to the collector by the medium-band gap collector region.

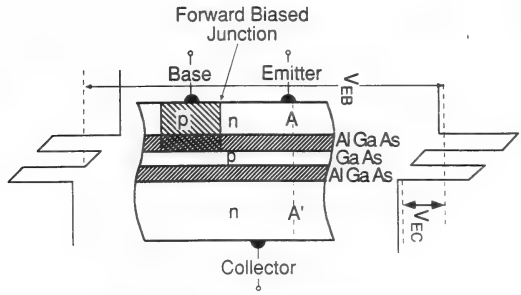

Fig. 4. [After Bate, 1989 with permission.] Unworkable bipolar RTT IC layout design. The emitter-base bias must be greater than the bandgap to achieve resonant tunneling. But contacting the holes in the quantum well requires a region in the base that is p-doped. Unfortunately a p region in contact with n-type emitter region subjected to forward biases greater than the bandgap would result in catastrophic parasitic current, making transistor action impossible.

planar IC layout configuration compared to the discrete mesa-device design configuration. These problems have recently been discussed by Bate (1989). A heterojunction implementation of a quantum-based bipolar, two current-carrier transistor, is to simply have the quantum-well layer p-doped, as shown in Fig. 4. However, by inspection of the symmetry in the band-edge diagram of Fig. 4, the possibility of large emitter-base hole currents becomes immediately obvious, whenever there is a resonant emitter-collector electron current. The catastrophic parasitic current will degrade the current gain completely. This problem can be overcome by using a semiconductor material in the quantum well with narrower band-gap than both the emitter and collector layer. In the transport channel, it effectively "hides" the hole-energy level in the quantum well from seeing the available states in both the emitter and collector, eliminating the leakage currents. The above vertical transport design have been implemented at TI (Bate, 1989; Texas Instrument Technical Journal, 1989), they have fabricated a bipolar quantum resonant tunneling transistor (BiQuart), Fig. 5.

A different approach to nanotransistor designs is to interchange the base and collector regions of the approach of Fig. 1, discussed earlier. In this new scheme, the biasing charge is introduced on the other side of the wide barrier, mainly creating electric field and affecting the self-consistent potential in the quantum well, with the main current flow extracted by contacting the quantum well, i.e., the quantum well becomes the collector region. In this design, the barriers are in general highly asymmetrical, with the third barrier many times larger than the two barriers, Fig. 6, to eliminate tunneling leakage current from the "base." Classified as a bipolar transistor this design has a negligible base current and large current-transfer ratio. However, according to our classification since the biasing charge is not directly introduced into the current channel but through the capacitive control of the "base" electrode, this design can also be classified as

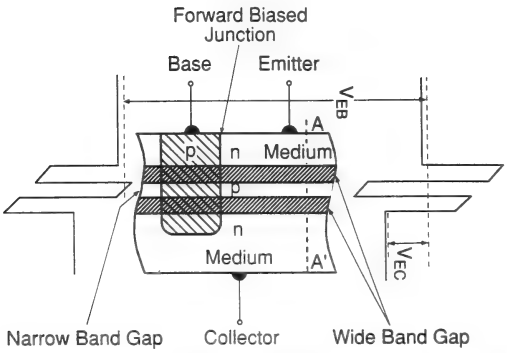

Fig. 5. [After Bate, 1989 with permission.] Workable bipolar RTT IC layout design, which eliminate p-n junction catastrophic parasitic current. A narrow-bandgap quantum well sandwiched between wide-bandgap barriers and medium bandgap emitter and collector regions allows for a reduced forward bias which does not create catastrophic parasitic current across the p-n contact junction.

a field-effect nanotransistor. Because the quasistationary states in the well are modulated by an electric field to produce transistor I-V characteristics, Bonnefoi, et al. (1985) propose to call this design as a Stark-effect transistor (SET) which was later experimentally demonstrated at AT&T Laboratories (Beltram,

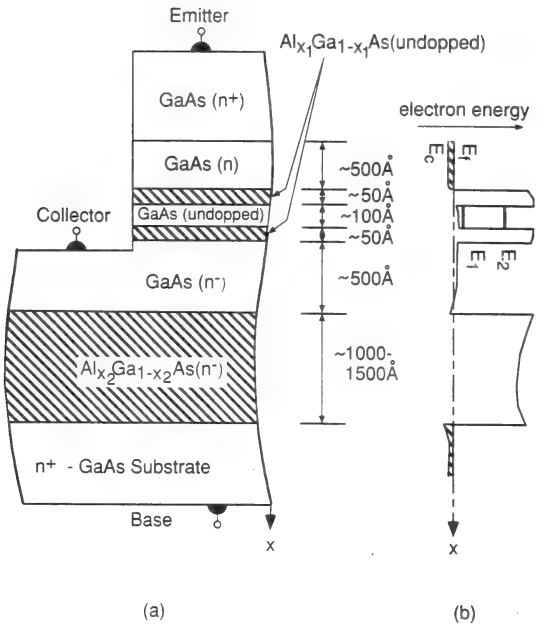

Fig. 6. [Bonnefoi et al., 1985 reprinted with permission.] Workable unipolar field-effect RTT mesa design, with the relative position of the base and collector being interchanged. Control is by means of the polarization charge at the base electrode, therefore this design can be classified as a field-effect RTT. Thus the emitter maybe called the source, the collector can be referred as the drain, and the base metalization as the gate.

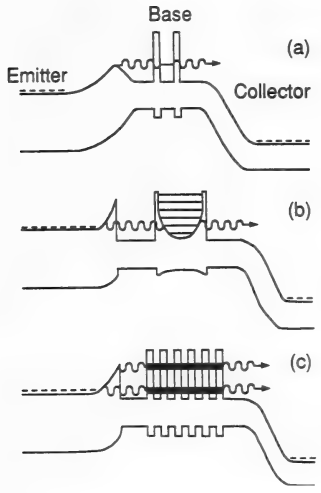

Fig. 7. [Capasso, 1990 reprinted with permission.] Band diagram of multistate resonant tunneling bipolar transistor (RTBT). (a) Design with graded emitter and abrupt double-barrier at the base (at resonance). (b) RTBT with parabolic quantum well in the base. for equally spaced resonances. (c) RTBT with superlattice base.

et al., 1988). One can also add a potential step in the quantum well using a narrow band-gap thin layer similar to the TI approach to further decrease the base-collector leakage current, to eliminate intervalley scattering and to enhance current drive.

A vigorous effort aimed at developing multistate resonant tunneling transistors (Potter, et al., 1988), in discrete mesa-device design configurations, has been undertaken at AT&T Bell Laboratories (Capasso, 1990). The earlier design is based on the modification of abrupt emitter-base heterojunction (wide band-gap emitter region) n-p-n heterojunction bipolar transistor by incorporating a double barrier or a superlattice structure in the base region (Capasso & Kiehl, 1985). The device has a multivalued transfer characteristic, having as many peaks as the number of resonances in the resonant tunneling structure in the base, Fig. 7. These devices have been demonstrated for potential multivalued logic applications (Capasso, et al., 1989).

Another multi-state resonant tunneling bipolar transistor (RTBT) pursued at Bell Laboratories is based on the "cascaded" vertical integration of double-barrier (DB) resonant tunneling diodes embedded in the emitter region (Capasso, et al., 1988). An RTBT with a single DB in the emitter was first reported by Futatsugi, et al. (1986). These DB's are separated by heavily-doped cladding layers to quantum mechanically decouple the adjacent DB's from each other. The band diagram is shown in Fig. 8. The idea is to obtain current peaks at the same current level with similar peak-to-valley ratios, which naturally lead to

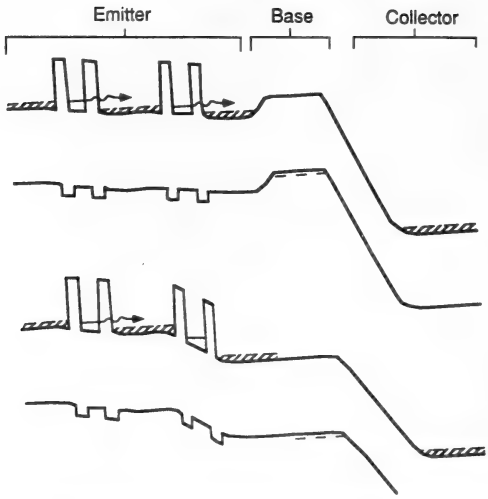

Fig. 8. [Capasso, et al., 1989 reprinted with permission.] Energy-band diagram of multiple state RTBT. operating common-emitter transistor with a series of double-barrier (DB) structures in the emitter region. for different base-emitter bias conditions. (a) Uniform voltage drop across each DB: in this condition the transistor operates as in conventional bipolar transistor. (b) nonuniform voltage drop. and quenching of RT through DB adjacent to p-n junction give rise to negative differential resistance in collector current vs. base-emitter voltage VEB. The successive quenching of RT for the succeeding DB in the series away from the p-n function produces multipeaks in collector current - emitter/base voltage characteristics.

designs using analogous resonance of a series of quantum wells. The operation of multistate RTBT can be described as follows. Assume that collector-emitter bias (V_{CE}) is kept fixed so as all DB's are resonantly conducting, and let the base-emitter bias (V_{BE}) be increased. For V_{BE} smaller than the p-n built-in voltage, most of the V_{BE} is dropped across the emitter-base p-n junction. At this point the device behaves as a conventional bipolar transistor, with the emitter-collector current increasing with V_{BE} until the base-emitter junction reaches a flat-band condition. Beyond the flat-band condition, the impedance across p-n becomes negligible, and the additional increase in V_{BE} is dropped across the series of DB's. Because of the screening effect of the cladding layers, quenching of the resonant tunneling (RT) is initiated across the DB adjacent to the base and anode regions. Once RT is suppressed across a DB its voltage drop across it quickly increases with bias because of increased resistance, and a non-RT current build-up provides the continuity for the RT current through other DB's operating in RT mode. As V_{BE} increases further, the high-field region widens (i.e., needs more cladding layers to screen the field). Therefore, quenching of RT sequentially propagates toward the cathode end and negative differential regions are obtained in I-V characteristics corresponding to the quenching of RT through each DB. Thus with n diodes embedded in the emitter region, n peaks

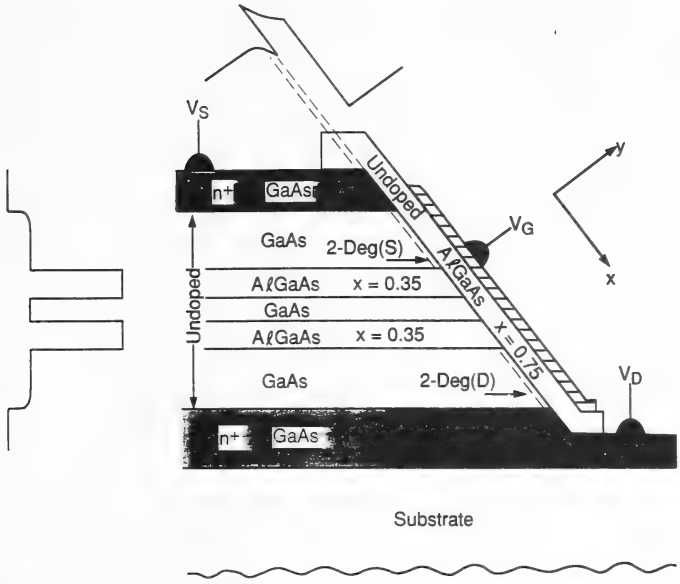

Fig. 9. [After Luryi & Capasso, 1985 with permission.] Unipolar surface resonant tunneling transistor and energy-band diagram along the gated "surface" conduction channel. The thickness of the two undoped GaAs layers outside of the double barrier region is sufficiently large to prevent parasitic resonant tunneling current in the bulk.

are present in the I-V giving saw-tooth features. This was experimentally demonstrated by Capasso, et al. (1989).

We note that except for the stark-effect transistors developed at Caltech, most of the nanotransistor designs based on resonant tunneling phenomena are essentially modification of conventional heterojunction bipolar transistors. A different approach based the modification of field-effect conventional heterojunction transistor (e.g., HEMT structure) has also been proposed at AT&T Laboratories (Luryi & Capasso, 1985). The device consist of an epitaxially grown undoped planar GaAs quantum well and a double AlGaAs barrier sandwiched between two undoped GaAs layers, the whole stack being further sandwiched between two heavily-doped GaAs layers for the source and drain contact. The device area is produced by a V-groove etching which is subsequently overgrown epitaxially with a thin AlGaAs layer, Fig. 9. Gate metalization resembles that of the conventional HEMT structure. The fabrication technique and structure was also proposed earlier by Sakaki (1980). The novel feature of the proposed design is that tunneling transport occurs across a quantum "wire," not across a planar layer as in previous nanotransistors. The operation is as follows. The application of positive gate voltage V_G induces 2-D electron gases (2DEG) at the two interfaces on both sides of the double-barrier structures. These are

polarized surface charges coming from the heavily-doped contact layers. Because of large zero-point energy and transverse energy-level spacing in the "quantum wire," there is a range of V_G in which electrons are not yet induced in the wire. Applications of drain voltage V_D bring about resonant tunneling condition where the transverse energy of electrons in the source matches the unoccupied quantized transverse energy in the wire. The transport process is a tunneling between 2-D electrons to the 1-D density of states in the quantum wire. Assuming conservation of longitudinal momentum k_z and denoting the kinetic energy due to the transport momentum along the direction kx at resonance as E_r, then the available k_z-components contained within the Fermi level in the 2-D source lie in the band of energies $E_o + E_r < E < E_F$. Here E_o is assumed to be the zero point energy of the 2-D source. Therefore the number of tunneling electrons grows with V_D until $E_r = 0$, where there is no more matching of the electrons in the source with the quantized energy level in the wire. This situation gives rise to negative differential resistance, similar to that of "planar" quantum-well nanotransistors discussed before.

The present design, however, offers the possibility of transferring control, of aligning the quantized energy level of the "wire" with respect to the band of energies in the source, from V_D to V_G. It has been demonstrated that the gate potential is nearly as effective in lowering the quantized energy level of the quantum wire with respect to the zero-point level in the source as is the drain potential. This implies the very interesting possibility of achieving negative transconductance for a unipolar transistor. This kind of transistor can perform the function of, and hence replace, a p-channel transistor in silicon CMOS logic, resulting in a low-power inverter, in which current flows mainly during switching characteristic of CMOS logic. This feature should find important applications in GaAs ICs.

Lateral Transport Nanotransistor Designs

The last transistor design discussed above may be considered to be based on the combination of lateral transport and vertical transport. Vertical transport since current flows across heterojunction layers, and lateral transport since current flows along the surface of the V-grove etching. Whereas most vertical-transport-based nanotransistor designs are essentially modifications of conventional heterojunction bipolar transistors, all lateral-transport-based nanotransistors designs, so far, are essentially modifications of unipolar or field-effect conventional heterojunction transistors. Specifically, these devices are modifications of high-electron mobility transistors (HEMT) (Ismail, et al., 1991; Chou, et al. 1991) and are field-effect-controlled heterojunction semiconductor structures. Lateral-transport-based nanotransistor designs make use of patterned

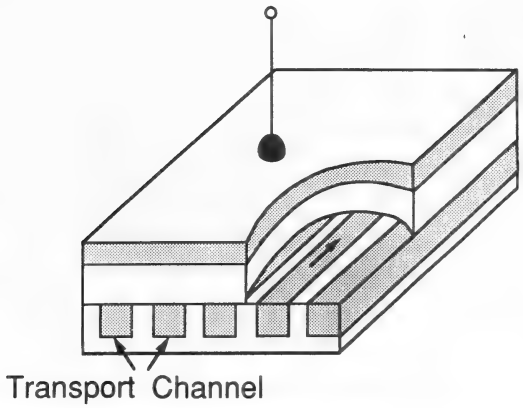

Fig. 10. [After Sakaki, 1980 with permission.] Schematic cross section of a parallel single-channel quantum wire array FET showing the gate metalization on the top layer.

multigates rather than a single gate found on a conventional HEMT structure (Buot, 1987b). This is achieved in a manner similar to the one proposed earlier by the author for MOSFET structures (Buot, 1987a). The longer coherence length and mean free paths in 2DEG transport channel in HEMT structure enables current lithographic techniques to fabricate lateral-transport-based nanotransistors. Lateral-transport-based designs offer a latitude of design parameters defined by the shape and geometrical pattern of the gates. This feature allows one to design nanotransistors whose function depend on the localization of charge carriers in more than one dimension, such as the use of "quantum wires" and "quantum dots," where carrier scattering is further reduced due to multi-dimensional size quantization, thus enhancing the coherence length and low-field mobility.

It was estimated that quantum wires offer about two orders of magnitude improvement in low field mobility, a significant higher saturation velocity and longer coherence lengths compared to 2DEG (Sakaki, 1980). Thus an obvious approach to further improvement of conventional field-effect high-speed devices based on the HEMT structure is to replace 2-DEG transport channel, controlled by the gate, by an array of quantum wires, Fig. 10, say few hundreds of parallel wires to maintain reasonable current drive capability (Sakaki, 1992). Indeed, size quantization by itself has immediate applications to conventional (i.e., nonquantum-transport-based) high-speed electronics and optoelectronic devices. One can expect an even further improvement if the 2DEG transport channel in HEMT is replaced by an array of coupled quantum dots, Fig. 11.

Because size quantization effects are expected to have immediate real impact in optoelectronics, the optoelectronics community, indeed, has strong stake on nanostructure research. Lateral device array designs are the preferred choice

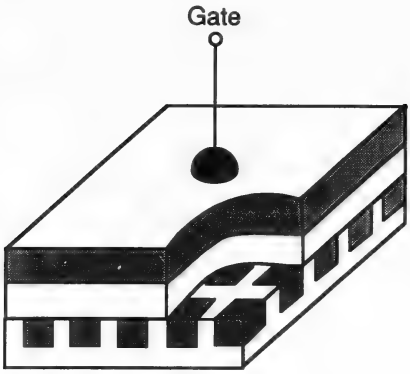

Fig. 11. Schematic cross section of quantum box array FET with top-layer gate.

since light can readily be coupled into lateral structures. The advantage of abrupt confinement across heterojunction has lead to vertical device designs in the form of a mesa device. The use of arrays of quantum wires and quantum dots, providing confinement of injected carriers in the active region of laser diodes has been shown to dramatically improve the lasing characteristics. In the case of quantum-dot array active region, the temperature dependence of the threshold current is virtually eliminated (Arakawa & Sakaki, 1982; Capasso, et al., 1985). It was also shown theoretically that the electroabsorption and the associated electrorefraction in array of quantum wires and quantum dots are greatly enchanced over that of "planar" array of quantum wells by virtue of multi dimensional quantum confinement Stark effect (i.e., bunching of quantum states). Such enhanced effects would lead to electroabsorptive and electrorefractive modulators and optical switches with even lower energy requirements than existing quantum-well devices (Sakaki, et al., 1990). Another interesting phenomena in optoelectronics is the so-called carrier-induced bleaching of optical absorption or the blue shift of absorption edge, primarily caused by band filling that quenches both exciton and band-to-band absorption, in quantum-wells arrays. The subsequent changes in refractive index (recall that refraction index in photon transport is akin to electrostatic potential in electron transport) have interesting applications, for example, it can be externally controlled by the injection of carriers or by a voltage applied at the gate in field-effect or HEMT configurations, where the 2DEG channel is replaced by the multi dimensionally confined arrays, so as to construct optical modulators and optical bistable switches (Sakaki, et al., 1990). Another example in the optoelectronics area is the self-electrooptic effect (SEED) which may have important applications in photonic switching (Miller, et al., 1984).

GaAs/AlGaAs MODFET-based Nanotransistors

Based on the modifications of the Schottky-barrier-gate structure of conventional heterojunction HEMT (or MODFET) transistor, a number of lateral-transport-based nanotransistors have been demonstrated (Ismail, et al., 1991; Chou, et al. 1991). The various structural modifications are: (a) dual gate or split-gate geometry, resulting in the so-called planar resonant-tunneling field-effect transistors (PRESTFET), (b) triple-gate geometry with independently controlled middle gate, this has added features over (a) in that the quantum well depth and barrier heights are controlled independently, (c) "grating-gate" geometry, which lead to induced array of quantum wires under the gate area, where lateral transport is across the wires and Bragg interference in this direction determines the overall transistor characteristics, and (d) "grid-gate" geometry, which lead to induced array of quantum dots under the gate area; two-dimensional Bragg interference (e.g., more bunching of states) determines the transistor characteristics.

At fixed drain bias, V_{DS}, as the gate bias, V_{GS}, is increased in quantum arrays, there are essentially two correlated changes that happen, namely, (a) the quantum-well depth increases resulting in "stronger" periodicity and consequent larger negative-mass portion of the miniband, and (b) the electron concentration increases in each "unit cell" resulting in "filling" of the minibands. Thus as V_{GS} is increased in a continuous manner, the Fermi energy is expected to pass through minibands and minigaps. Whenever the Fermi energy falls on the energy range of the minigap the current should drop to zero at $T = 0$. For $T \sim 0$, due to spread of energies around E_F of the mobile carriers, the current should drop to a non zero minimum at low-enough temperatures. Note that the drain bias, V_{DS}, also creates a spread of energies along the transport channel proportional to the electron "quasi-temperature" by virtue of electron heating by the applied drain voltage. Also since the minigap widths decreases at higher energies, the current minimum should increase with V_{GS}. This behavior is indeed what is observed by the MIT group, with much enhanced effects for grid-gate geometry (Ismail, et al., 1989). Thus the Bragg interference nanotransistors exhibit gate-controlled negative resistance and negative transconductance.

Physical Modeling and Simulation

The characteristic of logic gates that must be served by nanodevices, and the new regimes of operation compared to conventional devices, calls for a whole new approach to analyzing charge carrier transport in small structures. Nanode-

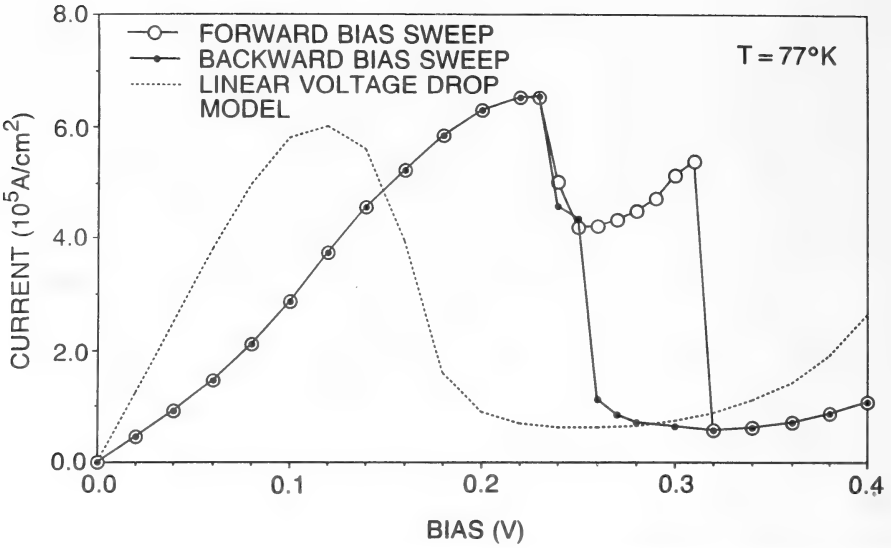

Fig. 12. Simulated current-voltage characteristics of an RTD for increasing and decreasing voltage sweep. Scattering and self-consistency of the potential is taken into account. The dotted curve shown for comparison, do not account for self-consistency, i.., the voltage is linearly dropped across the undoped double-barrier region. The self-consistent result, which agrees with experiments, is obtained by numerically solving a quantum distribution function transport equation with a subsidiary boundary condition for RTD.

vice transport physics must incorporate all quantum effects by virtue of the wave nature of the charge-carrier motion, all nonlinearities, all many-body effects, and all space and time-dependent nonuniformities. Indeed, device simulation researchers are seriously turning to particle Monte Carlo (Salvino & Buot, 1992) and various quantum approaches (Hess et al., 1991, 1992) to develop new computer-aided research and device characterization tools that will lead to novel computer-aided design tools which the IC industry will need in the not so distant future.

Presently, we can identify two major frontiers of research in nonlinear, far-from equilibrium quantum transport physical modeling and simulation that need to be addressed to enable the rapid development of nanoelectronics and "nano" information-processing systems. Grouped in the order of their complexity, these are as follows. The first is concerned with time-dependent, highly-transient and highly nonlinear, far-from-equilibrium quantum transport problems in nanodevice physics. We believed that for one-dimensional systems, these problems can be handled very well by the quantum distribution function (QDF) approach with a subsidiary boundary condition (Buot & Jensen, 1990), including band structure effects. Indeed, so far, this is the only approach that has produced realistic results for the I-V, Fig. 12, time-dependent results for analyzing high-speed operation of RTDs (Jensen & Buot 1991; Buot & Jensen 1991;

Buot & Rajagopal 1993) and has provided the complete transition from basic quantum transport physics to an engineering computer aided design (CAD) tool for ICs. However, for multidimensional systems of arbitrary geometry, and by virtue of the limited capability of present-day supercomputers, we believed that an area of research dealing with the coupling of the self-consistent ensemble particle Monte Carlo (SEPMC) method, which is a very powerful and a proven simulation tool for analyzing high-speed submicron devices, with a model of causal particle trajectory representation (CPTR) of quantum transport (e.g., Wigner trajectory, Bohm Trajectory or a refined model quantum tunneling particle dynamics (MQTPD) (Salvino & Buot 1992) can be a fruitful research direction. This is also an area where the classical picture of quantum transport will be fully developed, thus creating a much deeper understanding of some of the subtle aspects of quantum transport phenomena. The second is concerned with the less CPU intensive steady-state, high-nonlinear, far-from-equilibrium quantum transport problems in nano-device physics. Time independence will result in several orders reduction in the computer simulation time. For one-dimensional problems, this can be handled conveniently by the QDF pure quantum approach with a subsidiary boundary condition. For multidimensional problems of arbitrary geometry, large-scale matrix solution for the nonequilibrium quantum correlation function $G^{<}(q,q',E)$, where q and q′ represent lattice coordinates, should prove to be a very powerful nonlinear quantum transport technique for real solid-state nanodevices; this is an area where one can start from the knowledge of atomic orbitals to account for surfaces, interfaces, multivalley scattering processes, interband tunneling dynamics (Ting et al., 1992) and disorder effects in multidimensional finite systems where the energetics of the charge carriers are not known a priori (Buot, 1992).

Advances in computational hardware and software are expected to alter the current research and simulation capability issues, perhaps by bringing the situation towards a purely quantum transport simulation of a fully time-dependent, highly nonlinear, far-from-equilibrium multi-dimensional systems of "atomic-orbital" real solid-state materials with arbitrary geometries.

Conclusions

Quantum physics of small structures has evolved into a major field of condensed-matter physics in the last twenty years (Chang & Esaki 1992). Indeed, mesoscopic physics and nanoelectronics have undergone a rapid development in the 1980's, and is expected to acquire more momentum as opportunities for fundamental research and practical applications become more and more accessi-

ble with the continued advancement in the fabrication of small structures towards atomic sizes. Band-gap engineering of novel material systems, such as the InAs/GaSb/AlSb-based electronic materials, is currently a very active field of research (Collins et al., 1991). "Vertical" heterojunction technology has clearly reached the atomic size range and has allowed abrupt "atomic size" confinement of electrons in solids. However, there is a strong need to confine electrons in solids in more than one dimension. Quantum-based devices generally perform exceedingly well when the density of states per unit energy range is sharply defined (Sakaki, 1992). Well-separated energy levels of electrons confined in all directions virtually eliminate all scattering processes. Moreover, in resonant tunneling devices for digital applications, the unwanted self-oscillations in the NDR can be eliminated if the density of states is sharply defined (Buot & Jensen, 1991; Buot & Rajagopal 1993). To improve signal tolerance in binary logic applications, the ground state should be well-separated from the first excited state and should be near the emitter Fermi level.

Confinement in more than one dimension is currently being implemented by means of depletion barriers for digital applications, and by a "mesa" device design for optoelectronic and other applications. The inherent semiconductor volume required by the depletion layers inherently prevents scaling the device area for ultra-dense IC applications. Therefore, the development of abrupt heterojunction for device isolation and for lateral confinement would indeed set another milestone comparable to the development of vertical heterojunction MBE technology. Perhaps, developments in overgrowth techniques (Ide, et al., 1988; Beaumont, 1989; Stormer, et al., 1992), advanced lithography and other techniques using the principle of scanning tunneling microscope (García, 1992; Dobisz, et al., 1991; Marrian, et al., 1987; McCord & Pease, 1986) may offer the needed giant step toward lateral heterojunction technology. Lateral and vertical heterojunction capabilities would foster the rapid development of coupled quantum-dot arrays and novel IC architectures. Moreover, the lateral and vertical heterojunction capabilities are expected to encourage the fundamental study and practical applications of condensed systems with small number of electrons, defects and impurities, and thereby usher the rapid development of single electron quantum-coupled logic gates and ICs.

In closing, it's worth pointing out that although the potential technological impact of the study of nonlinear far-from-equilibrium quantum transport phenomena in dynamical (high-speed) small systems is unprecedented, the present-day worldwide efforts in this area are negligible compared to other mainstream areas of transport research, such as the classical and near-equilibrium quantum transport. This situation is expected to drastically change in the near future. The road to nanoelectronic IC is a long-winding road; nanofabrication is certainly

one of its immediate greatest challenges. However, far more greater conceptual challenges, novel computer architecture, complete characterization and understanding of quantum-based IC components, ingenious routing of interconnects, partitioning of logic and processing blocks, and novel communication channels to make use of the capabilities of nanofabrication. still lie ahead. There is no doubt that computer-aided research and development, i.e., the development of software research tools, CAD and R&D tools, will serve the "intelligence" behind the realization of a super computer on a chip.

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How Scientific is Survey Research?

A Comparison of Results of Surveys on the Same Topic

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Presented May 13, 1992

ABSTRACT

In this paper, the issue of reliability of interview and questionnaire or polling survey methods is discussed. Examples are noted demonstrating the variance among a number of survey results and subsequent action. A direct comparison of findings from surveys of the same universe of physicians about receiving and reading medical journals are cited which indicate that results are directly related to factors such as survey sponsorship. It is suggested that these questions be investigated before survey findings are accepted: Who sponsored the survey? How was the sample selected? How complete was the response? How were the queries worded? How was statistical testing done? What data preparation methods were used? and How were the data interpreted?

The speaker was introduced by Academy Fellow,
Julius H. Prince

I have been given the very great pleasure and honor of introducing Dr. Walter Boek to this audience tonight. And I've also had the distinct pleasure of being a student under Professor Boek's aegis when I was doing my doctoral work at Harvard back in 1952. We have had a close and wonderful association ever since.

Professor Boek was trained at Cornell University and at Michigan State University, where he got his doctorate. He then began to show his main characteristic, as I see it, as a professional innovator. I can't imagine anybody I know in the whole field of health sciences who has been more innovative than he. I think he must have read Frances Bacon, who 400 years ago said every medicine is an innovation and he who will not prescribe new remedies must expect new evils. So I think Walter has taken this lesson to heart. He started out in his research career that way, not long after I got to the New York State Department of Health in 1948, he was a cultural anthropologist full-time employee and advisor to the

Commissioner, no less, of the State Department of Health in Social Science Research—the first position of that type that I think was ever created in the United States, much less probably anywhere else.

Dr. Boek did many wonderful things which, as far as I was concerned, was epitomized by his help during the difficult time I had doing my doctoral dissertation. He taught me so many things about applied anthropology that I can't begin to say what they were or give you any details, but also in the book which he wrote in 1956, with his capable and charming wife, Jean, he was one of the first persons I ever recall talking about the relationship between the social conditions in which patients with various health problems lived and the prognosis and control of their illnesses. So with these brief introductory remarks, I give you my good friend and colleague and tremendous scientist, Professor Walter E. Boek.

Thank you Dr. Prince for your kind remarks. Good evening members of the Academy and friends. Some of us believe that one function of our Academy is to foster science. In accord with this, my talk will focus on an activity that influences all of us, either directly or indirectly. Not only are we bombarded by newspaper, radio and television reports of surveys, but our election process and our legislators also are influenced by them. Having designed, carried out and analyzed findings from at least 50 questionnaire or interview surveys that were as scientifically correct as we could make them, it is utterly distasteful to me to hear haphazard surveys touted as scientific, even to the extent of providing percentage "degrees of error." Statisticians have even gone to the extreme of evolving formulas to compensate for their unreliable survey methods. With these they seem to suggest that a complicated statistical formula could revive a dead horse.

Some survey sponsors do not bother much to have reliable methods, as the instructions for a United States Department of Agriculture food stamp program attitude survey on which policy was to be based, stated: ". . . no amount of time should be devoted to attempting a representable sample." Another example that involves both method and ethics is an American Management Association (AMA) survey. Would you believe that an employee of that august organization told us that when results of an AMA survey on which he worked were given to its corporate sponsor, he requested the sample be increased to reduce the stated margin of error. The AMA then waited a couple of weeks while it simply duplicated the cards on which responses were coded. The new "results," from their standpoint, emphasized the accuracy of the initial survey, because they were identical. For this duplication, the AMA received a substantial additional sum from the sponsor who assumed that it was for the increased number of questionnaires requested.

Findings of pseudo-scientific, or non-scientific polling that goes on week after

week reported in our media as facts might not be fed to the public by members of the media if they understood science and/or followed a system of ethics that emphasized honest reporting. The question of how they might be taught science or ethics is not the subject of this talk, however.

Turning to political polling, have you ever wondered how a candidate can be reported by pollsters as being favored by less than 10 percent of the people a day or so before he or she receives more than 50 percent of the votes, as occurred a couple of weeks ago in Pennsylvania? Or how President Bush is reported as being in the 40 percentage range in the polling, but actually receives about 60 percent of the votes in a primary election? Some time ago I became especially interested in this when President Harry Truman won a national election even though almost all the media had reported that the people favored Tom Dewey. It happened that we had conducted a carefully constructed sample of people, Michigan residents, who had been interviewed just before the election. One question concerned political preference. An analysis of responses, which we did after the election produced results very close to the tally of votes in the actual election, with Truman coming out on top. Thus, a survey conducted with scientifically reliable methods produced valid data.

Of numerous other examples of survey fiascoes, I will mention two. Remember the Ford Edsel automobile financial disaster in which Columbia University surveyors concluded that a luxurious Ford car would be welcomed and purchased by the public? As you know, that never happened.

In the second example, a survey company did a study of physicians for RCA in which they concluded that a high proportion of doctors would subscribe to a medical education program. After investing more than one million dollars in setting this up, RCA found fewer than 20 physicians enrolled instead of the thousands projected.

The elaborate reports to the sponsors by survey organizations containing results and recommendations that I have seen were beautiful to behold, but their findings were dangerous to follow. While some of the survey designs were creditable, their execution suffered from insufficient knowledge of methods and poor ethics at each level of the survey process. From our experience with interviewers, we learned early on not to hire those who had worked on surveys for other organizations, including the United States Census, if we wanted to collect valid data.

Science, as you know, is not a body of knowledge but rather a systematic method of seeking answers to questions involving relationships among variables. With surveys of populations being so popular that political candidates and parties hire their own pollsters, consideration could be given to comparing results of different pollers to estimate validity of their findings. This is not very

feasible, however, because they generally neither conduct them at the same time nor cover the same universe.

Polling is also popular with publishing companies who need to sell advertisements in their magazines. If they can convince advertisers that a specified segment of the population obtains and reads their magazines, it becomes easy for them to sell their advertising space. To supply the potential advertisers with information about the readership of their publications, they pay substantial sums to survey companies. These companies know that survey sponsors who have medical magazines would be pleased to learn that more medical practitioners receive and/or read the sponsors' publication than receive or read others.

Surveys conducted for medical journals provide an unique opportunity to make fairly direct comparisons of findings. Accordingly, a group of medical journals for the same universe of United States physicians in the same time frame for which surveys were completed can be compared. Although they were made available for these analyses, they are proprietary reports.

In this article, just a few of the comparisons that were done are presented. In Figure I, results of four surveys that reported how many physicians received each of three journals are shown. The journals are those published for physicians on a national level.

In Study I, shown by way of bar grafts, a large proportion said they received Journal A. Fewer acknowledged receipt of Journal B, and many fewer Journal C. There is a difference of 31 percent from the highest to the lowest journal.

As indicated in Figure 1, Study II reporting on the same three journals stated that Journal C was received by the most physicians while Journal A was in second place, with the number receiving Journal B between the other two journals.

Study III gives still another picture. In it Journal B was reported to have been received by the most physicians and Journal A by very few. The last survey illustrated in Figure 1, Study IV differs from the rest in still another way with all three journals being about equal.

A comparison of Studies I and II shows that Journal C was received by 42 percent more physicians than reported getting it in Study I. Studies I and III, when compared, shows that 28 percent more said they received Journal B in Study III than in Study I. For Journal A, the percentage reported as having received it, dropped from a large majority in Study I to a small minority in Study III.

Why is there this great difference in results among the four surveys that sought an answer to the same question from a sample of the same universe? When possible variables that might cause these wide differences were explored, it was found that the reason for the variation from a high number to a low one between

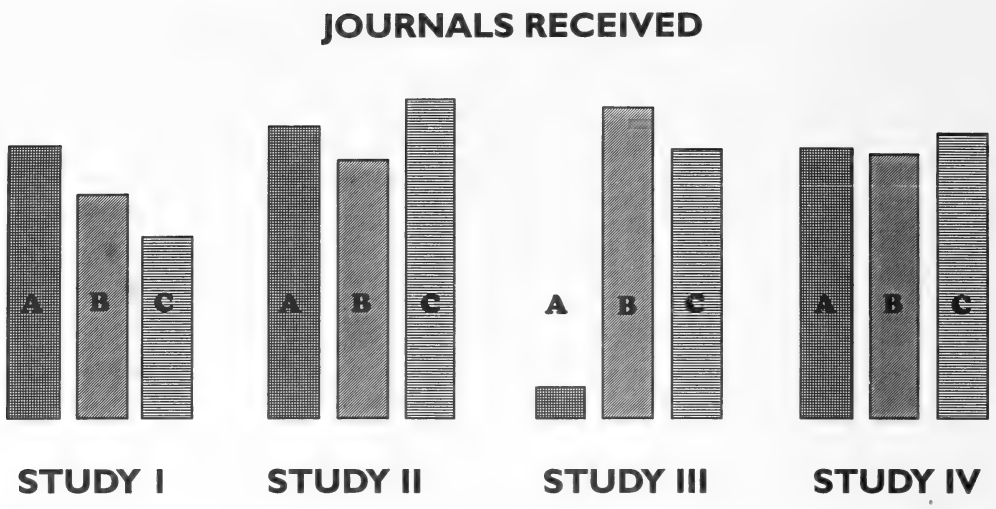

Fig. 1. Results of four independent studies of the same three journals, labeled A, B, and C, for the same universe of United States physicians.

Studies I and III for Journal A seemed to be related to the type of questioning in Study III. In that survey, Journals B and C were included in a list handed the physicians by the interviewer as he asked: "Will you look at this list of medical magazines and tell me which ones you get regularly or have regular access to?" Journal A was not included on the list. Instead, before the interview was completed, physicians were asked: "Now, are there any medical magazines not on this list that you get regularly?" Without the reminder—the journal's name on the list—most physicians did not remember Journal A. Potential advertisers were not told this in the report, of course.

The importance of questions wording and how the words are perceived by the interviewee, was demonstrated in an amusing experience that occurred as I was interviewing near a small village in the upper peninsula of Michigan. To learn what conveniences he had in his modest house, I asked a man a series of questions including this one: "Do you have running water?" "Oh yes, come with me and I'll show you." He then led me around back of his house to see a nice little brook in which the water tinkled merrily as it ran along. After that we changed the question to: "Do you have water piped into your house."

Returning to the medical journals, another hypothesis about the cause of the wide variations among surveys of the same medical journals was explored. It concerned possible effect on results of the source of financing for the surveys. To determine whether or not there was any unconscious tendency for the special interests of the sponsor to be favored the surveys were compared on that variable. It was found that Study I, in Figure 1, was funded by an organization

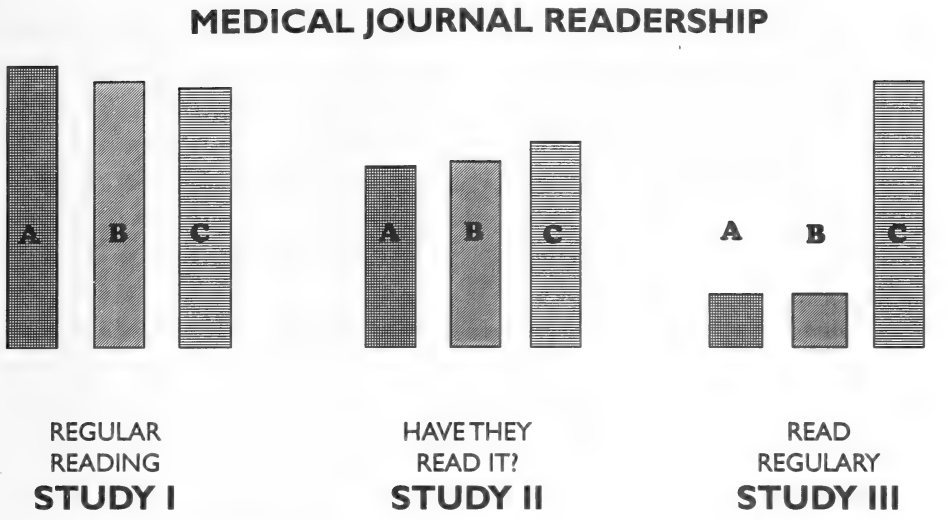

Fig. 2. Results of three independent studies of the same medical journals, labeled A, B, and C, for the same universe of United States physicians.

interested in Journal A that was reported to have been received by more physicians that the other two journals. For Study II the sponsor published Journal C, the one said to have been received by the largest number of physicians. In Study III, the publisher of Journal B, again the highest one, paid for the study. For Study IV, in which the tally of physicians receiving all three journals was about the same, the survey was done for a pharmaceutical house interested in evaluating journals in general. These four surveys, therefore, tended to support the hypothesis that the source of funding did tend to influence the results.

Exposure to a new idea in an article, or to a product in an advertisement, requires more than just for a medical practitioner to receive a journal. The physician they want to influence must open the journal and see the advertisement. Thus, pollsters frequently ask the physician whether one or another journal is read.

Data about the readership of three journals is presented in Figure 2. In Study I, the surveyors found that nearly 100 percent of the physicians their interviewers talked with regularly read Journal A, somewhat fewer Journal B and still fewer Journal C. For Study II, it was reported that only about one-half read Journal A, while more read Journal B and still more Journal C. The pollsters who did Study III, reported that only one out of six of the physicians they contacted regularly read Journals A and B while about 75 percent read Journal C.

In an attempt to learn why these three surveys came up with quite different findings, question wording and the funding sources were examined. First, as the

question wording is indicated in Figure 2 under each study, the questions were not quite identical among the studies. Regarding the funding source, it was learned that Study I was sponsored by Journal A, that was reported to have more physicians reading it regularly. Study II, with Journal C being read more than the other two, had the publisher of Journal C as the sponsor. Study III, in which Journal C was reported to have been read regularly by many more than the other two journals, was paid for by someone who did not have any special interest in any of the three journals.

Although, there is not time tonight to report other analyses about the distorted or blurry pictures arrived at through surveys, you and I as consumers of polling results should examine research designs before accepting or even bothering to listen to findings. I suggest that it is essential to ask these questions:

- Who sponsored the survey?
- How was the sample selected?
- What proportion of the sample selected actually was interviewed, or returned questionnaires?
- How were the queries worded?
- How was statistical testing done?
- What data preparation methods were used?
- How were the data interpreted?

Thank you very much for your kind attention. In a few minutes my term as President of the Washington Academy of Sciences will be concluded. Thank you all for tendering me this honor.

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The Washington Academy of Sciences Awards Program for Scientific Achievement in 1992

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One of the many ways by which The Washington Academy of Sciences contributes to the growth and recognition of scientists in the Washington Metropolitan area is through the awards program of the Academy. Each year the Academy recognizes such persons for scientific endeavors of merit and distinction. Awards are made for outstanding contributions in Mathematics and Computer Sciences, Behavioral and Social Sciences, Engineering Sciences, Biological and Physical Sciences. In addition the Academy makes an award designated as the "Distinguished Career in Sciences Award" to recognize a person who has made distinguished and life-long contributions to science.

The Academy in recognition of the primary responsibilities for the well being of society is in the teaching of science to young persons. In keeping with this goal the Academy presents the Leo Schubert Award for excellence for the teaching of science in college and the Bernice Lamberton Award for excellence in the teaching of science in high school.

Those receiving awards are selected from those persons nominated by either Academy members or the public, by panels of experts in each of the respective fields. The selections of the Awards Committee are then presented for approval by the Board of the Academy.

The Awards were presented on 13 May 1992 at a ceremony, held at the Officers Club at Fort Lesley J. McNair, Washington DC.

Awards were presented to the following persons:

Distinguished Career in Science	Dr. WOLFGANG L. WIESE
Behavioral and Social Science	Drs. CONRAD TAEUBER and CALVIN L. BEALE
Biological Science	Dr. JOANNE M. JONES
Physical Science	Dr. HASSAN EL KHADEM
Mathematics and Computer Science	Dr. DANIEL B. CARR
Engineering Science	Prof. KYU YONG CHOI
Leo Schubert Award for the Teaching of Science in College	Dr. BARRY G. SILVERMAN
Bernice Lamberton Award for the Teaching of Science in High School	Dr. W. ALLEN BARWICK

Distinguished Career in Science

DR. WOLFGANG L. WIESE was selected to receive the DISTINGUISHED CAREER IN SCIENCE award for his national and international leadership in the field of atomic physics and for his able direction of the Atomic Physics Division of the National Institute for Standards and Technology. Dr. Wiese is recognized by his sustained scholarly contributions to the study of high energy physics. Dr. Wiese was nominated by Dr. WILLIAM OTT and Dr. WILLIAM C. MARTIN of the National Institute for Standards and Technology.

Behavioral and Social Science

DR. CONRAD TAEUBER and DR. CALVIN L. BEALE were both selected for the award in the BEHAVIORAL AND SOCIAL SCIENCES for their long and distinguished careers in the development of demography as an applied and academic discipline and for their contribution towards an understanding of major demographic trends in the United States. Dr. Taeuber is a Senior Research Scholar at the Center for Population Research. Dr. Beale is a senior demographer with the United States Department of Agriculture. Both men are nationally and internationally recognized for their contribution to the development of demography as an applied and academic discipline. Not only has their research served to move the field of demography forward, but a broad range of researchers and policy makers recognize their achievements and methods to understand the changing character of the demographic structure of both the United States and the world. Drs. Taeuber and Beale were nominated by Dr. CORALIE FARLEE.

Biological Science

DR. JOANNE M. JONES was selected for the award in the BIOLOGICAL SCIENCES for her outstanding and original studies in the molecular biology and microbiology of transposon transference including her studies on the organization and structure of the transposon. Her contributions to our understanding of bacterial adherence to mucosal surfaces as an important step in the understanding of bacterial colonization and infection. This interaction is complex and is influenced by the ability of bacteria to adhere, the nature of epithelial cell receptivity, and the concentration and time of exposure to specific carbohydrates. Her studies are particularly important with regard to the clinical therapy of mucosal infections. Dr. Jones was nominated by DR. JAMES F. GOFF.

Physical Science

DR. HASSAN EL KHADEM, Isbell Professor of Chemistry at the American University was selected for the award in the PHYSICAL SCIENCES for his dedication to scholarship and his major contributions to carbohydrate chemistry. Dr. El Khadem has served as a member of the editorial board of Carbohydrate Chemistry and Chairman of the Carbohydrate Division of the American Chemical Society. He is recognized in particular for his contributions to the understanding of the formation, composition and reactions of phenylhydrazones, phenyl osazones, and osatriazoles. Dr. Khadem was nominated by DR. NINA M. ROSCHER.

Mathematics and Computer Science

DR. DANIEL B. CARR of the George Mason University Center for Computational Statistics was selected to receive the award in MATHEMATICS AND COMPUTER SCIENCES for his distinguished research in the development of tools and methods for statistical graphics. Among his notable contributions are the use of stereo plot, introduction of ray glyphs and other forms of glyphs, particularly stereo ray-glyph plot, the introduction of skeletonizing for summary representation of data using erosion technique, and most recently the use of binning techniques for representation of data density. This work is highly important for its immediate implications as well as for its future impact and visualization of scientific data. Dr. Carr's methods have solved some long-standing visualization problems. His techniques are generalizable and lead in natural ways to the development of similar solutions in high dimensions. Dr.

Carr is also recognized for his outstanding service to the scientific community as an organizer of professional activities in the area of statistical graphics and statistical computation. Dr. Carr was nominated by Professor EDWARD J. WEGMAN.

Engineering Science

PROF. KYU YONG CHOI was selected to receive the award in the ENGINEERING SCIENCES for his innovative and theoretical studies in polymer reaction engineering, for his demonstration of low molecular weight oligomers in the first stages of polyester synthesis, for application of bifurcation analysis of non-linear reactor dynamics, and for his modeling of industrial reactor dynamics of olefin polymerization. Professor Choi was nominated by JAMES W. GENTRY.

Leo Schubert Award for the Teaching of Science in College

DR. BARRY G. SILVERMAN was selected to receive the LEO SCHUBERT AWARD for the TEACHING SCIENCE in COLLEGE for his enthusiastic and direct exposure of both Undergraduate and Graduate students in meaningful and ongoing research projects and for his creative efforts to prepare students in computer literacy and introduction to artificial intelligence tools in science. Dr. Silverman is Professor of Engineering Management at the George Washington University. Dr. Silverman was nominated by MARYLIN KRUPSAW.

Bernice Lamberton Award for Teaching of Science in High School

Dr. W. ALLEN BARWICK was selected to receive the BERNICE LAMBERTON AWARD for TEACHING of SCIENCE in HIGH SCHOOL for his innovative and dedicated service to the teaching of Physics in High School and for his creation of a climate promoting an enthusiastic interest in Science in High School Students. Dr. Barwick is also recognized for his dedicated efforts in both the Local Science Fair system and in the National Science Fair. Dr. Barwick teaches physics at the Wilson High School in the District of Columbia. Dr. Barwick was nominated by MARYLIN KRUPSAW.

Following the presentation of the awards the President of the Academy, Dr. Walter E. Boek, gave the annual Presidential Address entitled, *How Scientific is Survey Research? A Comparison of the Results of Surveys on the Same Topic.*

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