BUILDING A PRAGMATIC KNOWLEDGE BASE:
New Approaches to Understanding What Works
and to Acting on What We Know
drawing on the writings of Daniel Yankelovich, Lisbeth B. Schorr, and Mark H. Moore
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The national conviction that "nothing works" owes some of its salience to the fact that the research and evaluations that are most widely considered credible produce very few findings that intentional, societal interventions, whether operating in the public or private sector, achieve dramatic improvements in valued outcomes, especially for large numbers and among high risk populations. This is true whether their purpose is greater academic achievement, safe streets, or children protected from abuse and neglect. The public consensus about what does work (with a few exceptions, like Head Start) is rarely based on research, but does reflect widely shared values -- such as the effectiveness of punitive measures. (Thus higher rates of imprisonment and more aggressive policing are thought to result in lower crime rates, and denial of welfare benefits are thought to increase the number of welfare mothers entering the job market.)

Significant progress in designing, implementing, evaluating, and holding accountable the most promising non-punitive interventions to improve outcomes for disadvantaged children, families, neighborhoods and communities are being hindered, in our view, because society's approach to understanding the interventions and the most promising links along the many pathways that lead to improved outcomes is weak, unrealistically narrow, and outmoded. (See Appendix 1 for a summary of the many ways that traditional methods of evaluation are mismatched with the probable attributes of effective interventions.)

If we had better methods of understanding "what works" we might see a large payoff in more effective social policies and programs. This would be particularly true when an upsurge in the will to act gets ahead of the existing knowledge base, as seems currently to be the case with respect to efforts

> to build on the new brain research and the new understanding of the damaging effects of parental isolation to make sure that all children have early childhood experiences that keep them safe and allow for their optimal development in the pre-school period

> to rebuild communities and bring about civic renewal

> to assure greater neighborhood safety

> to restructure schools to increase school achievement

> to meet the non-academic needs of children that interfere with school success

> to reduce the incidence of teen pregnancy

The energy behind these ideas is often powerful enough to burst the boundaries of the constraints imposed by the existing knowledge base. Legislators, funders, local agencies and planners are hungry for better guidance than is now available. Individuals and agencies charged with running programs or implementing new initiatives in each of these areas are frustrated with having to choose between creating their own personal cause-effect models through guesswork, and depending on the very meager information that is considered credible and respectable because it comes out of randomized trials.

Because they have so little good information, they risk implementing initiatives that fail, thereby wasting resources, defaulting on implicit or explicit promises (to participants and funders) to improve outcomes, *prematurely discrediting a promising field,* and adding to the growing cynicism that nothing works. Or, if the lack of information discourages them from acting, they risk *dissipating a brief surge in the will to act and in the availability of resources.*

We believe it is possible to reduce both sets of risks, if we are willing to leave behind some strongly entrenched convictions and conventions, and if we are willing to undertake systematic new efforts to build a pragmatic knowledge base, by accumulating and analyzing the knowledge that comes out of both research and experience in radically new ways.

I. IF CURRENT APPROACHES ARE OUTMODED, WHY ARE WE STILL STUCK WITH THEM?

What accounts for our having so *narrow and rigid a view of what constitutes knowledge valuable enough to use in confronting urgent social problems, and about where and how useful knowledge accumulates in the society?*

The traditional approach to social science research and to evaluation is suffused with a "scientific" aura that makes it attractive to social scientists, government and foundation officials, and the public.

Social science research and evaluation became increasingly influential in U.S. social policy over the last thirty-five years as policy makers came to believe that the methods of science could and should guide their decisions about social programs and social policy. The idea that social policy could be understood, analyzed, and shaped using the methods that had proved so successful in the natural sciences proved overwhelmingly seductive. The idea was that social policies and social programs could be reduced to standardized "treatments" that could be held constant across sites and over time, and that need only to be administered in the right dosages to ensure success for passive, interchangeable customers. "What is given is presumed equivalent to what is received, and what is received is equal to what is used. Use is then equated to gain." (Williams, et al, 1991). Best of all, randomized recruitment and selection of subjects would allow for clear comparisons between those receiving the "treatment" and a statistically similar group who do not -- and therefore unambiguous and certain answers to the question of what works.

As long ago as 1976, Alice Rivlin, then a Brookings scholar, later budget director, now member of the Federal Reserve Board, warned, "Maybe the whole evaluation movement started off on a couple of false premises -- that there is such a thing as a social program in the sense of a treatment, which applied (equally) to (all) people, which can then be evaluated to see if it works or not. Most of the evaluations ... assumed that we were providing something to people, that we could say what it was, that we could define some sort of output, and that we could measure whether it took place or not."

However, suggestions that this "scientific" approach might not be the one and only reliable tool to understand the effectiveness of interventions that were intended to improve outcomes in family settings, early childhood, schools, crime prevention, or job training, went unheeded.

Social scientists support the traditional approach because they yearn for an orderly process to link policy development more closely to the knowledge needed to guide it; they envision what Lindblom has labeled "a scientifically guided society," with "science, including social science, at center stage." The advantages of what Mark Moore calls the Social Research and Development model, are clear: it is rational; no big commitments are made until one has developed confident knowledge that the interventions will work, and without significant side effects. And social scientists have the key roles.

Government and foundation officials support it because it provides some protection against the consequences of misjudgments. It has also often served as an instrument to slow the growth of public programs.[1] Both in the public and private sector, policy makers and managers find it useful to be able to cite scientific evidence to support their decisions -- especially their decisions to act, *for society punishes public leaders more harshly for errors of commission than of omission.*

The Social Research and Development Model also appeals to a public which likes to think it is committed to being "scientific" in its decision-making. The public shares with decision-makers a leeriness at the prospect of departures from the scientific, which might entail becoming subjective, speculative, and tainted by fringe movements like pop psychology and New Age mysticism. There is a sense that no one who believes in reason as a way of solving the world's problems dares to embrace an approach that may be seen as less "scientific." The fear is that once you introduce judgment and try to cross disciplinary boundaries, you can no longer guarantee quality and guard against untrammeled subjectivity, against prejudice, against ideology.

Because all the participants in this elaborate dance fear nothing so much as being labeled "unscientific," we have made do with a meager supply of information about what is promising and what works, have allowed ourselves to be persuaded that findings that are less than certain are worse than no information at all, and taken a painfully constricted view *of how knowledge might best be diffused and deployed in aid of both immediate action, and continued learning.*

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[1]An example of the growing insistence on evaluation methods that produce certainty comes from a 1998 GAO report on Head Start Accountability, which complains that while HHS will, in the future, collect data from all Head Start programs as to the extent to which participating children have improved their vocabulary, literacy, and social skills as well as the extent to which families have become economically and socially self-sufficient, "HHS' planned analysis of survey results will not allow it to determine with certainty that Head Start participation cause children's or their families' improvements.... [Their] approach will not allow HHS to isolate Head Start participation as a causal factor in children's and families' progress."

II. ARE THERE OTHER APPROACHES THAT ARE MORE PRAGMATIC AND INCLUSIVE BUT STILL RELY ON REASON?

The flaws in our prevailing conception of knowledge are so ingrained that we have been unaware of how deeply destructive they are. In the unexamined belief that they are the price of living in a complex technological society, we do not seek alternatives.

But once we take a fresh look at the weaknesses in the traditional approach to knowledge building, it may be possible to *loosen* the prevailing *commitment to the more conventional approach, especially if there were reasonable alternatives.* Once it becomes clear how much the society sacrifices in sticking with the clumsy *apparatus that we've got, the world is open for an expanded image of how society might try to link the development of knowledge to the development of policy, and a new negotiation of* the relationship between knowledge-building and policy development as we search *for more realistic and better ideas about how these relationships might be ordered."*

We can apply reason and logic without confining ourselves to knowing only what can be learned through controlled experiments, to knowing only what can be known with certainty. The wisdom that comes from understanding "natural experiments," from understanding the recurring patterns that appear in apparent successes in many different domains should not be devalued. Rather it can be made part of a body of rich knowledge that could be useful in guiding both program design and program improvement.

In our search for alternatives, we are not advocating an atheoretical-approach, we are not advocating making do with lower standards of evidence. Rather we are arguing for an alternative to the currently dominant knowledge paradigm, for a knowledge building paradigm that would include a richer body of data and could become a more sturdy foundation on which to build more effective policies and programs.

The time may be ripe for a conceptual shift in what we think of as reliable knowledge. There is increasing interest in a more generous approach to systematic learning than we have seen in the past, combining theory, logic and evidence with intelligent judgment, analogy, insight, and creativity -- a combination that Nobel Prize biologist David Baltimore identifies as the basis for "the most fundamental progress in science."

Robert M. Solow, the MIT Nobel laureate in economics, says that being scientific means that we think logically and respect fact, and that facts include the opinions and generalizations of experts, attitudes, and our judgments of plausibility. Another Nobelist, this one in physics, Murray Gell-Man, urges that we look at problems by putting many perspectives together, that we engage in more interdisciplinary knowledge building, that we get around academic measurement of excellence based only within disciplines, rejecting the idea that the only valuable work is increasingly detailed studies of ever smaller pieces. He says we must be willing to "take a crude look at the whole," a look will never be as elegant as what you can say about circumscribed pieces, but may give you better problem-solving guidance. He calls it a risky enterprise, because

"charlatans hide in the crevices between systems." But the answer is not to abandon the quest, but to build in safeguards through a constant emphasis on logic and facts, and the broadest possible participation and review.

The prevailing approaches to knowledge-building and dissemination have assumed that the results of research and evaluation should be able to allow social scientists, policy analysts, legislators, and philanthropists to determine what people at the local level, at the front lines, should do, in the sense of giving them models to clone. Nirvana, in this model of social change, would have the world of policy research and analysis tell federal, state and local policy makers, United Ways, and local agencies and civic coalitions how to invest their funds and energies.

But there is more and more evidence that this model of social change in entirely unrealistic and unrealizable. Increasingly, participants in the policy process are becoming persuaded that we must recognize the limits of how far researchers and policy makers can really go, and how precise they should aspire to be, in figuring out what individual agencies and specific communities should do. It is probably unrealistic for policy people -- be they at a university, a state capital, or at RAND, Brookings, or Abt Associates -- to be charged with figuring out precisely enough what works and for whom, to pre-determine choices

> between investments in family support through home visiting or family support centers,
>
> between investing in child welfare through family preservation or foster care or group homes for at-risk babies and mothers,
>
> between investing in early childhood through Head Start or HIPPY or the training of family day care providers,
>
> between investing in teen pregnancy reduction through family planning or abstinence education,
>
> between investing in helping the families stuck in persistent poverty by rebuilding depleted neighborhoods or by making it possible for them to escape their poor neighborhoods.

It is unrealistic to expect that those choices can be made on the basis of research because the research will never be good enough to provide the answers.

But it is also unrealistic -- and undesirable -- because this is not a job that can or should be left exclusively to the experts.

When we finally learned that mandates imposed from above don't work, we over-learned that lesson. Once funders and legislators and business leaders figured out that interventions can't be crafted centrally and parachuted into local communities, too many did a complete flip and acted as though nothing that was offered from outside had any validity, as though the only alternative to top-down mandates was simply leaving local people to figure everything out by themselves.

On the basis of the experience of the last three decades, we now know with some confidence that not every starting point is as good as any other, that some activities are more likely than others to accomplish specified purposes, and that some theories of change are better than others. Leaving local communities to painstakingly make these discoveries on their own, or to never make them at all, has been a wasteful process. Leaving every local school superintendent or county manager or citizen coalition to create their own personal cause-effect models through guesswork will result in lost opportunities and will interfere with further progress in spreading initiatives aimed at strengthening families and neighborhoods and modernizing our social infrastructure.

This means that we must develop new approaches to building a more pragmatic knowledge base that has the potential of moving the whole intervention field away from oversimplified yes/no, success/failure judgments about what programs work. We could build a richer, more complex knowledge base about strategies that are plausible, promising, or proven. Practitioners, program designers, and communities will be able to make use of the lessons learned from both research and experience, to construct ever stronger theories, and ever more effective interventions. By using intelligence and judgment to understand existing research and experience, we can construct a sturdy knowledge base on which to build more effective interventions, programs, and policies.

The pragmatic knowledge base we envision would collect information from and analyze a richer set of experiences and research than is now available to those who seek to strengthen the society's capacity to tackle our most pressing social problems. Our pragmatic approach to knowledge building draws on the rich traditions and roots that pragmatism has in American philosophy, going back to William James and John Dewey.

We see our approach as getting and producing more, not less. We see the departure from the traditional approach as enriching and enhancing, not diluting, *as involving not only losses, but gains, and that using alternative approaches to building knowledge may eventually produce better answers, and produce the answers sooner, than a much smaller number of more carefully controlled experiments.*

We will be able to probe a wider variety of inquiries to extract the information that may be less conclusive but more useful than the information produced by the traditional model. *We aim not for certain solutions (knowing they cannot achieve a full grasp of all the relevant complexities), but for steps toward amelioration, that provide feedback and illumination that then lead toward other, now better informed steps; giving up certainty about (or focusing less on) causal attribution* in favor of suggestive data about links between interventions, interim milestones, and long-term outcomes. We seek no compromise with high standards of rigor, but do seek to change the criteria of what is considered useful knowledge.

Chart 2 summarizes key points of difference between the old and new approaches.

THE OLD WAY

In determining effectiveness, relies most heavily on evaluations designed in an experimental mode, utilizing random assignment; is severely constrained in the kind of knowledge considered worth having; devalues interventions that are hard to evaluate by traditional methods because

   * they are complex, individualized and flexible, evolve over time, and employ of interactive and synergistic components that cross disciplinary domains

   * they depend on relationships of trust, respect, shared values, and staff that believes in what they're doing

   * they vary from one site to another to reflect site-specific needs, strengths, and preferences

Produces too narrow a data base, assuming that a little bit of certain knowledge is more valuable than a wider range of knowledge about what is probable and promising; *fails to identify the best possible way for a society to deal with problems whose answers are not yet well known; treats knowledge as more precious than the will to act,* and therefore misses opportunities when there is a surge in the will to act on urgent social problems

Takes too long to provide information that would be useful in program management, program improvement, and resource allocation decisions

THE NEW WAY

By employing multiple methods and perspectives, building on a strong theoretical and conceptual base organized around valued outcomes, provides reliable information about the links in the causal chain that are hypothesized to connect interventions to the desired results, with enough process documentation to assure that the intervention was implemented with the intended intensity and quality. Leads to better understanding of the effects of interventions that have been hard to evaluate by traditional methods because

   * they are complex, individualized and flexible, evolve over time, and employ interactive and synergistic components that cross disciplinary domains

   * they depend on relationships of trust, respect, shared values, and staff that believes in what they're doing

   * they vary from one site to another to reflect site-specific needs, strengths, and preferences

Identifies and devises plausibly effective interventions that can guide prudent investments and experiments. Encourages experimentation to reduce the areas of ignorance about interventions; makes provision for collecting, analyzing, and disseminating knowledge from the "natural experiments" that evolve. *Recognizes the importance of the will to act, and supports action in the absence of precise and complete knowledge, when the problem is urgent, when effort and commitment may be as important to solving the problem as knowledge, when public will is strong, when the solution of the problem is not heavily dependent on specific technical knowledge, and when variety in responding to special circumstances is likely to be valuable*

Produces good information promptly enough to be useful in program management, program improvement, and resource allocation decisions; has the capacity to harvest lessons from field experience promptly, entertaining many plausible solutions in order to *learn while acting,* especially when acting in response to public will and in the absence of complete information; *captures the experience initiated by decentralized actors and thereby speeds up the rate at which society learns from its own experience.*

THE OLD WAY (cont.)

Gives too much control to the experts and not enough to those most affected; *limits the agents who are recognized as having a legitimate stake in judging what works, and limits experimentation.* Relies on experts to use scientific methods to identify and devise solutions that will work everywhere, and can be mandated for local implementation

Protects against bias by emphasizing adversarial, value-free, "disinterested" role for evaluator

Expects evaluators to arrive at yes/no, success/failure judgments about what individual programs or program components work; invests heavily in the quest for certainty about causation

Relies on evaluation methods that neglect such practical considerations as questions of local capacity, politics, and relationships, and larger social forces etc. that cannot be captured by centrally devised cause and effect models

THE NEW WAY (cont.)

Assists "non-experts" to make well-informed judgments; sets up a two way conversation between those whose wisdom comes from experience and those whose wisdom comes from research; encourages new relationships between outsiders and insiders (between professional "experts" and practitioners, residents, community leaders, etc.) to allow non-experts into the mix, and to *broadly democratize the authorization to act;* social scientists become one, and not necessarily the dominant, set of participants in the search for what works. *Lay people are at center stage, with a powerful supporting role played by social science.*

Allows for better understanding of the intervention being evaluated by emphasizing shared interests rather than adversarial relationships between evaluators and program people, protect against bias, subjectivity, prejudice and self-interest in arriving at conclusions by assuring broad participation of individuals representing multiple perspectives (such as through consensus conferences pioneered by the NIH) and other mechanisms to assure disinterest, dispassion, and impartiality etc., but without attempting to make the enterprise "value-free."

Focuses primarily on society's stake in identifying the strategies that promise to be useful in attacking urgent social problems; assumes *it may be more valuable over the long run for society to get better measurements of value and worry less about causal attribution.*

Disseminates knowledge, guidance, and assistance in ways that allow for continuing two-way communication between "outsiders" and "insiders", and can therefore take into account such factors as local capacity and politics

III. TOWARD OPERATIONALIZING A NEW APPROACH

With leadership, support, and coordination provided from a small committee with minimal staff, as described below, a number of institutions would be urged (and financially supported) to build on their extensive experience to begin to utilize the new approach in specific areas of inquiry and action.

For example -- and only for purposes of illustration:

> Zero to Three, The National Center for Infants, Toddlers and Families, would take responsibility for addressing the issue of effective interventions during the earliest years (mainly 0-3)

> If a consortium of foundations were to be created to define effective interventions aimed at school readiness in connection with the implementation of Proposition 10 in California (or if an entity performing this function already exists in connection with North Carolina's Smart Start), they might take on the 0-5 and school readiness issues.

> The Pew Partnership and/or the Aspen Institute's Roundtable on Comprehensive Community Initiatives might address the issues in neighborhood transformation and civic renewal

> The National Institute of Justice of the Department of Justice might address the issues of crime prevention, gang control, and neighborhood safety

> The Brookings Institutions, which already has a major project underway to bring together a variety of education reform proposals and education reformers, could work on issues related to restructuring schools and school systems to increase school achievement

> The Boundaries Task Force of the Harvard Project on Schooling and Children, possibly in partnership with the Aspen Institute Program on Education in a Changing Society, could address the issues of meeting the non-academic needs of children that interfere with school success

> The National Campaign to Reduce Teen Pregnancy could address the issues in the teen and unintended pregnancy domain.

These groups would be assisted, and would assist each other in developing and utilizing new approaches to connecting knowledge to policy and program. The new approaches would evolve over time and probably differ across the various domains, but would share in some measure the attributes described in Chart 2 as part of 'THE NEW WAY." At this preliminary stage of our thinking. we can anticipate only some of the activities they would undertake.

One process that we can imagine is currently in use in assessing certain cancer treatments. It is described by Mark Moore of the Harvard Kennedy School of Government and the Hauser Center on Nonprofit Organizations, in his review of alternatives to the dominant approach to building the knowledge base. *Experts develop experimental protocols, representing the most promising treatments currently available for the treatment of a particular kind of disease, say leukemia. These protocols are offered as ideas that local physicians might want to consider, with the understanding that if a particular physicians did decide to use one of the protocols, he would report back (to a central body) on the progress and results of the treatment.*[1]

Some of the activities we envision being undertaken as part of the quest for a new Pragmatic Knowledge Based include the following:

> The working groups would draw on available materials and a wide range of individuals with expertise in both research and experience to develop -- possibly through consensus conferences -- a preliminary set of "theories of change" pathways or logic models that connect interventions with interim markers, and the short-term and long-term outcomes most commonly sought. The hypothesized connections can be made both forwards from the intervention and backwards from the desired outcomes. Pathways would be labeled with respect to the confidence that a diverse and knowledgeable group believes can be attached to them (e.g. proven, promising, plausible.) The resulting compendia would be used for three purposes:

>> 1) they would be made available to people now working in that arena, who would be asked to compare their activities with the theoretical models, identifying matches where they occur, and ways in which the theoretical models would have to be modified to reflect their actual work.

>> 2) they would be made available to people hoping to work in that arena, as ways of informing their program design efforts

>> 3) in their constantly evolving forms, they would become the templates around which effectiveness information is collected and analyzed

> This process would make it possible for each local group not to have to start with a blank slate in making explicit its theory of how interventions are expected to change outcomes.

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[1] It is also worth noting that the Porter-Obey Comprehensive School Reform Demonstration, passed by the Congress in 1997, roughly reflects at least part of the thinking that went into the medical protocol model. Schools can receive federal funding for any one of several research-based whole school reform strategies. Some of the strategies are specified in the legislation, but none are mandated, and schools can also choose to implement models not specified in the legislation as long as there is evidence of effectiveness from research. However, all that is being done to harvest the lessons from these various approaches is to collect data from evaluations that meet conventional criteria.

It would make it possible to get reliable information from a far broader range of intervention strategies than is currently available. It would also provide a structure for prompt feedback, on the basis of which the compendia would be constantly revised. Wherever and whenever possible and appropriate, the hypothetical connections would be examined as to whether the hypothesized impact is occurring. This information would of course become critical in further revisions of the compendia and in enabling all participants in the process to construct ever stronger theories, and -- ultimately -- ever more effective interventions.

This model, like the medical protocol model, acknowledges the pre-eminence of local decision-making, encourages local initiative, imagination, and adaptation, but without dismissing the existence of centrally available expert knowledge. It nudges the field in particular directions without blunting its initiative or over-regulating its search for solutions. It democratizes the activity of problem-solving. It allows society to act quickly and to learn while acting by accumulating at the national level the experience of local practitioners about what seems to work and what doesn't

This and other new approaches to systematic learning to be developed would not provide proof that one specific element of a comprehensive neighborhood transformation initiative caused the observed drop in crime or child abuse or infant mortality, or the increase in employment, but it could assemble some of the practical knowledge that would enable us to discern patterns across interventions and across sites. It could even combat the prevailing nihilism that holds that nothing can be known about what works because the certainty we demand is not attainable.

Certainty, indeed, would become less important if there were agreement around the proposition we advance, that the world of policy research and analysis won't ever be able to figure out precisely enough "what works" and for whom to allow "the national experts" to make ultimate intervention decisions.

We conclude that many of these choices cannot best be made centrally, by social scientists, policy analysts or legislators. But neither should local communities or agencies be abandoned to make these choices through idiosyncratic guesswork. Under present circumstances people at the grass roots and the front-lines can get information about a few isolated "proven" interventions; they can get information about discipline-bound "best practices." They can get advice how best to engage in community planning, building community coalitions and other useful processes. But when it comes to the effects of interventions, very little information has been certified as "credible" by the arbiters that most of us have looked to for guidance on these matters.

Therefore our proposal to build the new pragmatic knowledge base by supplementing the tried and true methods of the past with new, more imaginative, more inclusive, and more democratic means.

IV. NEXT STEPS

A. Paper laying out the problem and our ideas for a solution

Dan Yankelovich and Lisbeth Schorr (preferably having enlisted Mark Moore as a third participant) would expand and refine this paper

>   1) to circulate among key people who could contribute to our thinking and may want to become part of the effort

>   2) to submit for possible publication

>   3) to convert it to a proposal for funding

B. Establishing a base and obtaining support

The challenge of developing (and ultimately obtaining acceptance for) a new approach to connecting knowledge to policy and practice involves such fundamental departures from deeply entrenched norms, that the effort cannot succeed unless it is carefully positioned from the outset, and unless there is a base at one or two high-prestige institutions to provide continuing leadership and support to the effort.

Toward these ends, we would set up an informal working group (or two, one on each coast) to further develop our ideas and our plans. We anticipate that this process would evolve into a more formal committee to provide direction to the effort.

We would simultaneously seek financial support and explore various options for an institutional base. (The Kennedy School at Harvard and the University of California at San Diego seem to be the most obvious possibilities at this time.)

C. Functions to be performed at the institutional base

One possibility would be for a small staff to operate from this institutional base. With the support of the committee, the staff would be in constant communication with all of the working groups, to make sure they were learning from one another's experience. Lessons learned about new approaches to connect knowledge to policy and practice, and about the proven, promising, and plausible interventions, would be promptly disseminated to all participants, but also to others at every level of the society working to improve outcomes for disadvantaged children, families, neighborhoods and communities.

Appendix 1
INVERSE CORRELATION BETWEEN
"GOLD STANDARD" OF INTERVENTIONS
and
"GOLD STANDARD" OF EVALUATIONS

ATTRIBUTES OF INTERVENTIONS ASSOCIATED WITH EFFECTIVENESS	ATTRIBUTES OF INTERVENTIONS ASSOCIATED WITH "EVALUABILITY"
significant front-line flexibility	standardized; discretion minimized
evolving -- in response to experience & changing conditions	constant over time
intervention/program design reflecting local strengths, needs, preferences	intervention/program centrally designed, uniform across sites
intake/recruitment into program under local control within broad parameters	intake/recruitment into program centrally designed to permit random assignment
multi factor	single factor
interactive components	components clearly separable
emphasis on continuity of supportive relationships & other hard-to-measure inputs	readily measured inputs
implementers "believe in" the intervention	value-free implementation