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## Asking Questions Well: The Role of Theory in Applied Social Research

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### I. Asking Questions Well

By “asking questions well” I refer to selecting an issue of importance and framing a research question that will be productive. The subtitle of my lecture, “the role of theory in applied social research,” indicates that, in my view, theory has a great deal to do with asking questions well. This is a large topic, confused by considerable miscommunication and misunderstanding.

Asking questions well is the hardest part of applied social research.<sup>1</sup> It has two principal components. The first is selecting an issue of sufficient importance, i.e., an issue with a likely “pay off” in knowledge and/or application. In this regard, not all questions are of equal value (Merton, 1959). Unfortunately, we are often uninterested or unwilling to make judgements about quality of questions, and instead focus most of our attention on research methodology. But research costs a great deal of time and money. Years are required to even begin to address most questions. A scholar can address only a small number of questions in her entire career; and therefore -- if she wants to make a meaningful and lasting contribution, and who of us does not? -- she must choose her questions carefully. The second component of asking questions well is

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<sup>1</sup> Some may prefer to think of social work as a profession rather than an applied social science. Certainly social work has elements of both. The emphasis in this paper is on the use of theory in knowledge building for application. In this sense, social work faces issues similar to other applied social sciences such as public administration, community development, urban planning, and public health, as well as applied branches of academic disciplines, including psychology, sociology, anthropology, political science, and economics. As I discuss in this paper, the fact that social work is also a profession does not change the nature of knowledge, or the requirements for knowledge building.

to frame a research question that will be productive. It is possible, indeed common, to have an important issue but a research question that does not lead anywhere worthwhile. Toward the end of the lecture, I suggest that, for the purposes of the applied social sciences, certain structures of inquiry may lead to theories that are more productive than others.

These two requirements--selecting an issue of importance and framing a productive question-- are enormously challenging and never fully achieved. How can we do better? To begin, advice from C. Wright Mills in his classic work, *The Sociological Imagination* (1959), is still very good. Mills suggests that applied social scholars should critically address an issue that: (1) connects private troubles and public issues; (2) is not limited by the artificial boundaries of a single academic discipline or applied profession, but rather draws upon multiple perspectives; (3) brings a fresh, imaginative perspective; (4) advances social scientific understanding of the topic; (5) is theoretically informed; (6) is firmly based in empirical reality; and (7) contains explicit implications for courses of action.<sup>2</sup>

In Mills' last standard, the key word is action. Mills emphasizes that applied social research questions, in their very structure and content, should point to social action or intervention. In other words, the challenge in the applied social sciences is not simply to find out what is true, but to find out what is both *true and useful*. In this regard, I would add several standards regarding inquiry for social action or intervention. To every extent possible, a research question should point to an intervention that: (1) is simple, understandable, communicable, and doable; (2) is highly explanatory, i.e., has meaningful effects; (3) is adaptable to multiple forms in multiple situations, fitting a wide range of circumstances, people, institutions, and conditions; (4) can be framed in terms of core values in society; (5) is ethical; (6) is affordable; (7) is politically within the realm of possibility; (8) is subject to multiple tests; (9) has benefits that exceed the costs of intervention; and (10) can be implemented by an average person or organization.

I have developed this list in teaching doctoral students how to think about questions and theory in applied social research. It is important to note that the intellectual and practical demands are extraordinarily high, especially the last standard, which says that the implied action "can be implemented by an average person or organization." An intervention will be of limited value if only special people or organizations are able to do it. Unfortunately the implied actions of many of our theories fail to meet this standard, yet without it little can happen beyond showcase events or demonstrations.

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<sup>2</sup> The reader may note that I cite a number of social scientists from the 1950s and 1960s. I do so because their work is relevant to the topic at hand and has not been surpassed or made obsolete. Looking back, it was a particularly thoughtful period for the social sciences; many of the people cited in this lecture remain intellectual giants. In recent times we have become more sophisticated in data collection and analysis, but without as much reflection about how we go about our work.

## A Long Way to Go

Regrettably, I do not think there has been enough emphasis on asking questions well in doctoral programs in social work. I have heard several directors of doctoral programs say something like: "It doesn't matter very much what questions doctoral students ask. The important thing about dissertation work is that students learn research methods." On other occasions, I have heard social work researchers say in reference to their empirically-based studies: "What theory can we find to put on the front of this?" Such comments reflect a narrow view of scholarship as consisting predominantly of research methods and empirical findings. From this perspective, theory is something that is distinct from empirical work, and not very important.

My message in this lecture is that *theory and method are integrated and inseparable aspects of explanatory inquiry in the applied social sciences, and this is as true for applied social science as for basic social science*. By definition, applied social research requires scientific explanation, i.e., that one thing (an "intervention") causes another (an "outcome"). Whether specified or not, this is a theoretical structure. For this reason, the strongest and most lasting work in applied social science— as in all science— is theoretically based. Some social work researchers appear not to regard this basic structure as theoretical, suggesting that theory is sometimes not necessary for "outcomes research" (e.g., Thyer, 1999). However, I do not believe that is really what they intend to say, because it would indicate a failure to recognize the basic requirements for explanatory inquiry in a scientific framework. What I believe they intend to say is that a *pre-existing, well-developed* theory does not always have to be tested, which is a point we can all agree with. In social work practice research, there appears to be a reaction against the use of theory as received wisdom, because it does not always fit the topic to be studied. This is entirely understandable. But the discussion sometimes goes too far in tossing theory out altogether.

In social work research, theory is often poorly specified, or misapplied, or not used at all, and I suspect that most PhD programs in social work either omit or give short shrift to theory as part of method. We give considerable attention to data analysis, but probably not enough attention to data analysis within the context of a well specified question, i.e., one that is theoretically explicit. As I suggest in this lecture, in the absence of well-specified theory, research tends not to be productive. Some of the main points of this lecture are: (1) theory is essential to knowledge building in applied social research; (2) applied research is not less theoretically demanding than is basic research; and (3) social work scholarship is not a special category with unique requirements for knowledge building; the basic theoretical requirements of inquiry still apply.

## Aiming for Productive Work

Most applied social research, perhaps 90 to 95 percent of it, is of little consequence.<sup>3</sup> Although it may be funded by grants and published in journals, most applied social research has little lasting impact on the way scholars think or the way professionals act. Fortunately, this is not as large a waste of time and resources as one might think. It is not always possible to know which 5 or 10 percent of scholarship is going to turn out to be productive. The beauty and wonder of open universities with freedom of inquiry is that they shelter and facilitate all kinds of work. Even though productive work occurs only occasionally, new knowledge is so powerful that even infrequent success makes the entire enterprise worthwhile.<sup>4</sup>

Nonetheless, we cannot be complacent about low quality work. No matter how high the odds against success, we are never off the hook. It is always required of scholars to aim for productive work. In exchange for the extraordinary privilege of living an academic life -- protected, free to ask questions, nurtured with resources and opportunities -- a scholar owes a great deal back to society. The obligation is to do one's best to make meaningful contributions to a body of knowledge.

I would like to discuss improving the odds that one's scholarship will be productive. The pathway to such contributions is through theory. Within a scientific framework, every explanatory inquiry should be guided by clear questions that are specified as theory (deduction), or it should be aiming to specify questions better (induction). There is no such thing as atheoretical explanatory work in the applied social sciences.

## A Word on Application

Also, as indicated above, there is no meaningful difference in the nature of inquiry in "basic" vs. "applied" social science. Instead, the major difference is in application -- applied social science carries the *additional* burden of having to be directly relevant for action. We try to do work that has both intellectual content and potential impact in the world. As social psychologist Kurt Lewin (1951) famously observed, "There is nothing so practical as a good theory." The notion that applied social science can or should be atheoretical is unfortunately common, but it leads nowhere. As a research tool, a theory simply means a carefully thought out and specified idea.

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<sup>3</sup> I do not mean to single out applied social research. Quite likely this is true for all academic work, though I do not have as much first-hand knowledge of the basic social sciences, the natural sciences, or the humanities.

<sup>4</sup>This is in part why the reputations of universities, or schools, or departments, tend to be based on the work of a small number of outstanding scholars. Although everyone is working, only a few are generating most of the value in new knowledge.

One cannot pick and choose from the scientific method, selecting the portions that are most convenient. For the purposes of application or intervention, theory must be as explicit as possible and *causal* in a social scientific sense, because it is in effect an attempt to *predict outcomes*. Without this, it would be hard to know the basis for action, and in most cases it would be unethical to act at all. In brief, *application requires prediction, and prediction requires theory*.

### Theory as an Integral Part of Method

The word “theory” means many things to many people, ranging from a common understanding to a paradigm, from a specific hypothesis to a theory of everything, and every sort of conceptual device in between. First, it may be helpful to say a few words about what theory is not:

Theory is not philosophy, ideology, or values. Although normative statements often can be specified into a normative theory, the essence of theory in a scientific sense is not to state or interpret what should be, but to ask what is.

Theory is not discourse. Sometimes in social work, an article is called “theoretical” if it does not have numbers in it. (Conversely, if it has numbers in it, it is called “empirical.”) Fortunately we are seeing less of this in recent years.

Theory is not reality. Theory is a simplification, a device for ignoring information. Some academics seem to have the idea that every possible aspect of a phenomenon should be represented in a “theory.” The representations of such thinking appear like a plate of spaghetti and meatballs with circles and lines going everywhere. This kind of “theory” is of little value conceptually and typically has limited implications for application.

Theory is not a grand formulation. In social work we sometimes embrace grand frameworks that are useful as a general way of seeing the world, but are not useful scientifically because they do not yield testable hypotheses. To take a well-known example, systems theory, which is often touted in social work, is so abstract that it does not yield hypotheses, and as a result has been largely unproductive for knowledge building.

What then is theory? As a tool for scientific inquiry, theory can be defined as a set of logically interrelated constructs, such that the stated constructs can be operationalized, measured, and analyzed in relation to one another, and in this way the theory, or portion of it, is subject to empirical test. Theory in the social sciences, including the applied social sciences, has the same essential characteristics as theory in the natural sciences. The major difference is not the nature of theory but its specification and formalization. In the natural sciences, theory is likely to be formalized as a mathematical equation. In most of the social sciences, with the exception of economics, we are not as precise or formal, although it is a standard that we should aim for more

than we do. My meaning today is *theory as an integral part of method or theory for use*. In a practical sense, it is the specification of how we think things work, so that this thinking is subject to empirical test.

## Types of Inquiry

To be sure, theory is not required for all types of inquiry. At risk of oversimplification, inquiry in the sciences, social sciences, and humanities can be divided into a small number of main categories, as shown in Table 1.<sup>5</sup>

**Table 1. Classification of Types of Inquiry**

- I. Description
  - A. Non-positivist
  - B. Positivist
- II. Explanation
  - A. Non-positivist
  - B. Positivist
    - 1. Unique pattern
    - 2. Repeated pattern
      - a. Induction
      - b. Deduction

The first major category is description, which refers to current or past conditions, and can also include trends and patterns, but not explanations. Good descriptive work is highly important, often a necessary first step in building a body of knowledge, but it is often underrated (for example, a doctoral student would quite likely not be allowed to write a descriptive dissertation). No theory is required for descriptive inquiry. Description can be non-positivist when it no objective reality. For example, the identification of a social problem can be seen as socially constructed more than as an objective reality (Best, 1989). Or description can be positivist when it assumes an objective reality. For example, FBI-reported trends in suicides are usually considered to represent an objective reality.

The second major category is explanation. Explanation seeks to say why or how. It is relational, attempting to tie one set of circumstances to another. Explanation can be placed into two major categories, non-positivist and positivist.

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<sup>5</sup> This classification serves the purposes of this discussion, but it is far from perfect. Any classification of types of inquiry will have shortcomings. In this case, the non-positivist approaches are not elaborated in detail. Some of the examples are debatable. My purpose in presenting this classification is only to identify where predictive theory is an integral part of inquiry.

Non-positivist explanation takes in a wide range of approaches to inquiry, but in general refers to a pattern of events from a given viewpoint. It is a particular story from a particular perspective, and often seeks to be a full and rich story. It is frequently used to study social phenomena in the form of biography, social relations, and social history.

Positivist explanation assumes an objective reality and comes in two forms: unique pattern and repeated pattern. Unique patterns occur in much of history and natural history; for example, the French Revolution and the path of starfish evolution may be considered objective realities, but they are unique patterns. General principles might apply, but the outcome cannot be predicted from the underlying principles. The explanation seeks to tell a relational story, but does not seek to predict that the same set of relations will apply elsewhere or in the future. Therefore, theory is often not required for this type of explanation.

The search for repeated patterns within positivist explanation is an attempt to predict relationships among constructs regarding other actors, and at other times. The explanation is not considered to be unique. For example, controlling for all else, people with low wealth are predicted to have lower educational attainment. Or to take a treatment example, cognitive therapy is superior to psychoanalytic therapy for outcomes in obsessive-compulsive disorder. Positivist explanation in search of repeated patterns is only a tool, one "way of knowing," but it has proven to be an extraordinarily useful and productive tool. This is the way we build predictive knowledge in a scientific sense.

Positivist explanation in search of repeated patterns comes in two main forms, induction and deduction. In induction, the researcher does not know what to expect, and the challenge is to try to learn something about the likely pattern, so that this can be specified as theory (Glaser and Strauss, 1967). Regarding deduction, the essence of knowing in a scientific framework is to be able to specify what one thinks is happening, gather evidence, and ascertain to what extent one is right or wrong. Deduction is required for this, and it appears to be underrated these days, perhaps especially in social work scholarship. Many researchers fail to specify their questions as testable propositions. Many who in fact have some idea of what they are looking for claim instead that they are only being "exploratory" or are "building theory." Whenever one has an idea what is happening, it should be specified and tested. This is the essence of knowledge building in a scientific sense. With few exceptions, applied researchers should strive to say what they think may be happening and ask through careful methodology if indeed it is happening. This requires specifying theory and stating hypotheses so that they are testable.

### **A Word on Research Methods**

Allow me to clarify that type of research methods is not the issue under discussion in this lecture. It is an entirely separate matter. There is a bewildering tendency in some quarters of social work scholarship to confuse research methods with

inquiry structures. For example, some ardent critics of positivism insist that quantitative methods are for positivism, while qualitative methods are for narrative inquiry. Likewise, some ardent proponents of positivism make similar arguments. These viewpoints are wrong-headed. Any type of research method can be used with any of the type of inquiry. Quantitative methods can be used in non-positivist inquiry and qualitative methods can be used in positivistic inquiry. An example of the latter would be undertaking a series of detailed case studies to ask if holding economic resources protects women from physical abuse from partners. The choice of research methods should depend on the question being asked and a thoughtful decision about what type(s) of data will best shed light on this question at hand. Unfortunately, some researchers let their preferred methods, rather than nature and demands of the question, guide their research. This is like learning how to use a hammer and then using it for everything, even when a screwdriver is sometimes the right tool (Kaplan, 1964).

## **II. Example: Theoretical Foundations for Asset-Based Policy**

I will illustrate these points with an example of applied work that is carried out with an effort toward theoretical development. At the Center for Social Development (CSD), George Warren Brown School of Social Work, Washington University, we have proposed and are undertaking research on asset building as a new direction in anti-poverty policy. CSD is currently engaged in a very large, multi-method, six-year policy research project on individual development accounts (IDAs), which are matched savings accounts for the poor. This is perhaps the largest applied social research project in the country at the present time. The Corporation for Enterprise Development (CFED) in Washington and CSD are partners in this demonstration. We are testing a very practical policy innovation. Of importance in this lecture, we have theories and hypotheses about how the intervention works and what the outcomes are likely to be. The success of this policy innovation to date is due as much to theory as to application. Indeed, we cannot conceive of doing this applied social research in the absence of theory. With no theory, it would not have occurred at all.

### **Applied Impact**

IDAs first began in community organizations in the early 1990s, including housing organizations, community action agencies, microenterprise programs, social service agencies, and community development financial institutions. Today there are at least 250 operating IDA programs and many more in the planning stages. We have been successful in including IDAs as a state option in the federal "welfare reform" act (U.S. congress, 1996), and achieving funding for a five-year, \$125 million federal demonstration of IDAs (U.S. Congress, 1998). Almost all states have raised asset limits in TANF, and at least 27 states have included IDAs in their welfare reform plans. Some states plan to use federal TANF dollars to fund IDAs; several states have committed

state general funds for IDAs; and legislation is active in many other states.<sup>6</sup> IDA legislation in the states typically has broad bipartisan support.

Several prominent networks of IDA programs have been or are being established. A national program of IDAs was initiated by AmeriCorps VISTA, with volunteers working at community development credit unions and other community organizations. The Eagle Staff Fund of the First Nations Development Institute has initiated IDAs on several Indian Reservations. The Neighborhood Reinvestment Coalition has started an IDA program. United Ways in Atlanta, St. Louis, Denver, and perhaps elsewhere have funded multi-site IDA programs. Several states have organized IDA networks.

Universal Savings Accounts (USAs) were proposed by President Clinton in his 1999 State of the Union Address in January and spelled out in greater detail in a White House presentation in April (U.S. Executive Office of the President, 1999).<sup>7</sup> Clinton proposed using 11 or 12 percent of the budget surplus, an estimated \$38 billion per year at the outset, rising with the rate of inflation, to create a progressive system of accounts for retirement. The federal government would make annual deposits plus matching deposits into accounts of low and middle-income workers, taking in most of the working population, on a progressive basis, i.e., the largest subsidies would be at the bottom. Some have described this as a 401(k) available to all workers. It would be the largest anti-poverty initiative since the Earned Income Tax Credit.

In his State of the Union address on January 27, 2000, President Clinton offered a similar proposal, and the White House referred to the success of IDAs in shaping this policy (U.S. Executive Office of the President, 2000):

Tens of millions of Americans live from paycheck to paycheck. As hard as they work, they still don't have the opportunity to save. Too few can make use of IRAs and 401(k) plans. We should do more to help all working families save and accumulate wealth. That's the idea behind the Individual Development Accounts, the IDAs. I ask you to take that idea to a new level, with new retirement savings accounts that enable every low- and moderate-income family in America to save for retirement, a first home, a medical emergency, or a college education. I propose to

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<sup>6</sup> See Karen Edwards (2000) for summaries of state IDA policies.

<sup>7</sup> I first articulated the concept and name USAs, and this proposal has been presented by CFED and CSD over the past several years (e.g., CFED, 1996). At the time of the President's State of the Union Address, CFED and CSD were meeting in Washington on Universal Savings Accounts. Early experience with IDAs was influential in the White House decision to propose USAs. In designing USAs, the Treasury Department asked CSD for early data from the American Dream Demonstration (ADD) showing that, with matching funds, at least some of the poor are able to save.

match their contributions, however small, dollar for dollar, every year they save (Clinton, 2000).<sup>8</sup>

### Based on Research

The most important research initiative on IDAs at the present time is the "American Dream Demonstration" (ADD), funded by a consortium of eleven foundations.<sup>9</sup> There are 13 IDA demonstration sites across the country, with a four-year demonstration (1997-2001) and six-year evaluation (to 2003). CFED is carrying out the IDA demonstration, and CSD is designing and directing the research. Abt Associates is undertaking the experimental design survey. ADD has an intensive research agenda that includes the following methods: implementation assessment (case studies at all 13 programs), monitoring of all sites and participants (software created for this purpose, see Johnson and Hinterlong, 1998), individual case studies (N=18), brief cross-sectional survey (N=300), experimental design survey (N=1,100, three waves), supplemental in-depth interviews (N=90), a community level evaluation, and a benefit-cost analysis.

As indicated above, this intensive research agenda has yielded high returns in informative results and influence on public policy. Implementation assessment has informed many starting IDA programs. Case studies of participants have yielded detail on savings experiences and perspectives of IDA participants. Monitoring and periodic reporting on all participants and their savings patterns has been important in shaping policy development. Using information technology to the fullest, we are able to download data immediately on savings patterns of all participants in all 13 programs. The payoff of demonstration sites on policy cannot be underestimated. Monitoring data show that low-income IDA participants save a mean of \$33 per month and the very poorest save as much as others (no statistically significant difference). In other words, the very poorest are saving at a much higher rate (i.e., savings compared to income) than others (Sherraden et al. 2000). When senators and representatives know that an IDA program is succeeding in their district, they are much more likely to become advocates. In this regard, research is not something that happens after policy, but is integral and essential to policy development at each step along the way.

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<sup>8</sup> As I revise this lecture in June 2000, Vice President Al Gore has proposed a system of Retirement Security Plus accounts, patterned after the Clinton's USA proposal and approximately at the same scale.

<sup>9</sup> The foundation supporters of ADD are the Ford Foundation, Charles Stewart Mott Foundation, Joyce Foundation, Citigroup Foundation, Ewing Marion Kauffman Foundation, F.B. Heron Foundation, John D. and Catherine T. MacArthur Foundation, Fannie Mae Foundation, Levi Strauss Foundation, Rockefeller Foundation, and Moriah Fund.

## Not Without Theory

It may be helpful to describe the policy and intellectual context into which asset-based policy was introduced. Income has been the basis of social policy in the "welfare states." Unfortunately, most income-based policy research is atheoretical. Theoretical specification has been avoided in income poverty research by way of assumption. The two core assumptions are: (1) consumption is, by definition, equivalent to well being, and (2) income is a good proxy for consumption. Therefore, income can be taken as an indicator of well being. The use of these two assumptions conveniently obviates the challenge to specify or demonstrate that income has any effects at all. (This is a little like hitting the ball and then saying you are on second. In a real game, players have to run the bases.) As it turns out, income transfers have not lifted people out of pre-transfer poverty (Danziger and Plotnick, 1986), and other than consumption support, there is no specified rationale for income-based policy. Of course income is necessary, and we do not advocate reducing income transfers, but the assumptions underlying this policy have hindered inquiry into the nature of well being and the best policies to achieve it. Looking back, this is perhaps the greatest intellectual failure in social policy in the twentieth century. Fortunately, we are now in a period where scholars are beginning to ask if well being might consist of something more than income. A notable example is Amartya Sen (1993), who theorizes about well being in terms of capabilities.

Into the income-dominated policy discussion, I introduced the possibility of asset building (Sherraden, 1988, 1991). Following the example of income poverty researchers, I might have asserted that assets are equivalent to well being and let it go with that. This would have been much easier than trying to gain a theoretical handle on the ideas. But instead, with the very able research staff at CSD, we have tried to ask key questions and have begun to specify the theory. The first question is: How do assets accumulate? The general theoretical statement is that saving is due to institutional structures as much or more than individual preferences. We have identified four types of institutional structures that matter: incentives, information, access, facilitation. For each of these we have developed specific hypotheses (Appendix A).

The second question is: What are the effects of asset holding? The general theoretical statement is that asset holding has multiple positive effects. This is certainly not the only rationale for an inclusive asset-based policy. A convincing case can be made on grounds of social justice and equity alone, and differential historical and current treatment, especially by race (Oliver and Shapiro, 1995). However, the policy-analytic question is: if people accumulate assets, what happens? The viewpoint here is that assets may have important effects on well being, in addition to their potential for future consumption. This perspective is deeply embedded in social philosophy in America, with roots in Jeffersonian agrarianism. This very American perspective has not been the focus of integrated, systematic inquiry. Nonetheless, bits and pieces of research in many different fields can be organized into general propositions on positive impacts of assets. A beginning list would be that assets: (1) improve household stability, (2) create an orientation toward the future, (3) stimulate enhancement of assets, (4) enable focus and specialization, (5) provide a foundation for positive risk taking, (6) increase personal

efficacy, (7) increase social connectedness and influence, (8) increase political participation, and (9) enhance the welfare of offspring and allow intergenerational development (Sherraden, 1991). These propositions can be further specified as hypotheses in various categories regarding the effects of assets (Appendix B).

This is a long list of hypothesized asset effects, far from a well-developed theory. On one hand, many of these may seem like common sense. On the other hand, this may seem like a long list of exaggerated claims. In fact, my purpose is not to claim that asset holding has all of these effects, but rather to lay out the academic terrain on which applied research can proceed, and if warranted, a convincing rationale for asset-based policy might be established. I certainly would not say that this is the "right" list. Some of these hypotheses will be substantiated; others will not; many will become more specified. At the moment, we are far from definitive answers, however, reviews of research from various fields is generally supportive of the Jeffersonian viewpoint that asset holding is good for people and good for the community (Page-Adams and Sherraden, 1997; Boshara, Scanlon, and Page-Adams, 1998).

Although crude, it is important to reiterate that these ideas and their specification are driving both policy development and research. Well-stated ideas can influence application, and therefore the applied scholar must develop applied and conceptual aspects of her work *at the same time*. Theory is necessary not only for inquiry, but to relate successfully to people outside the university, including participants, the general public, the press, and policy makers. Specification of theory is part of the knowledge building process and can, especially with empirical evidence, influence application.

### III. Meanderings and Pathways

In this section, under the heading of meanderings, I take up three issues that may be getting in the way of specification and use of theory in social work research. Following this, under the heading of pathways, I mention three issues that may over time improve the use of theory.

#### Meanderings

Rank empiricism. In some circles of social work scholarship there appears to be an extraordinary faith that many, many empirical but atheoretical studies will add up to something. I have heard this described in various ways, as pieces of a great unknown puzzle, as building blocks to make a wall, and so on. Unfortunately, it does not work this way. Knowledge builds only within theoretical structures. To make this point more concrete, pieces of a puzzle make a coherent image only when there is an overall design, and bricks make a wall only when there is a plan for the wall. Thousands of studies with no theory are not likely to add up to much; they will be like random pieces from many puzzles, or like bricks of many dimensions strewn haphazardly across the yard.

This is not to say that rank empiricism is entirely useless. Facts can sometimes be useful, and eventually insight (induction, theory building) is likely. The problem with rank empiricism is that it is *hugely inefficient*. If one puts enough bricks and debris out in the yard, eventually there will be some kind of barrier, but an intellectual structure beforehand will help build a thinner, stronger, and more beautiful wall, and build it much faster.

**Practice ideologies.** Direct practice in social work is struggling to establish an empirical base, and is making considerable progress, but practice is for the most part not based on established evidence. In the absence of an empirical foundation, social work practice has been defined more by practice ideologies. These may be called "theories," as in Freudian theory, although they are not well specified and subject to test. They tend to operate more as general frameworks for interpretation.

To their credit, some scholars of direct practice are rejecting "theory" in this ideological sense in favor of empirically based practice.<sup>10</sup> These efforts are to be applauded. However, in some cases the very idea of theory is being dismissed, and in this sense the baby may be going out with the bath water. While practice ideologies are not desirable, well-specified theory *is* desirable. The best replacement for practice ideologies would be not rank empiricism, but specified theories that are subject to test.

**Different kind of knowledge.** There has been an extended effort to sort out how social work knowledge or professional knowledge or intervention knowledge might be distinctive. A recent exposition is by my colleagues (Proctor and Rosen, 1998), where the term "control knowledge" is suggested as a basis for "active prediction" and intervention. Control knowledge is said to be different from descriptive knowledge and explanatory knowledge. Clearly I am not an expert in treatment evaluation, but I wonder if the distinction of control knowledge from explanatory knowledge is useful. As pointed out above, *intervention requires explanation*. Conceptually, an intervention is an operationalized construct (or constructs) that is hypothesized to be related to another construct (an outcome). Whether it operates actively or passively certainly matters in application, but it does not in any way change the nature of the knowledge. For example, a chemical equation can be written showing that two hydrogen atoms and one oxygen atom can combine to form one molecule of water.

Whether this occurs with or without human intervention, the knowledge is the same. To take another example from the assets work, we hypothesize and evidence suggests that home ownership leads to better educational outcomes for children. This knowledge is exactly the same whether we initiate IDAs for home ownership or not. Suggestions that knowledge for application is somehow a different kind of knowledge

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<sup>10</sup> I am grateful to Aaron Rosen and Enola Proctor for helping me to understand these issues and responses to schools of thought in social work research on direct practice.

may lead some social work researchers to conclude that the use of explicit theory is not required in research on social interventions. Indeed, there are puzzling comments in the paper suggesting that social work researchers have been too much influenced by their training in social science theory. I believe the authors' concern is about the difficulties of using pre-existing theory for practice research where it may not fit. This is understandable. In the process, however, they appear to downplay theory as a necessary tool and integral part of research methods.

Similar issues arise at the policy and program level, where there are discussions of "policy theory," "administrative theory," "program theory," and "logic models" for evaluative purposes (e.g., Chambers, Wedel, and Rodwell, 1992). These are typically attempts to describe policy and program processes, but without connections to more general ideas. Again, these are not entirely useless; some facts may be useful or some insight may result; but for knowledge building *it is hugely inefficient*. Whether specified or not, a policy or program is the embodiment of an idea. That idea should be made explicit and subject to test. Only in this way can we learn something in a particular situation that may be added to a more general body of knowledge. Ideally, no program or policy evaluation would occur without an attempt to specify and test a theoretical statement. The fact that most of them do indicates how far we have to go.

### **Pathways**

**Simplicity.** Especially for applied purposes, simplicity matters. It matters because interventions are expensive, difficult to implement, and difficult to test. We are in need of theories that (1) have only a few operational constructs and (2) identify key relationships. The point of good theory is not to represent a complex reality, but to capture a fundamental dynamic that is highly explanatory -- and in the applied social sciences, has large implications for action. To put his another way, we are in need strategies for ignoring the least important information, which is what a good theory does. The challenge is to identify implications for action. This is perhaps best articulated in Milton Friedman's classic *Essays in Positive Economics* (1953) in which he argues for the greatest simplicity possible. We are not looking for the whole truth, but for very explanatory constructs, or *powerful themes*.

**Middle range.** In the applied social sciences, we can benefit a great deal of the wisdom of sociologist Robert Merton's (1957) call for theories "of the middle range," by which he means theories that are not so grand as to have no operational implications and not so narrow as to have no relevance beyond specific circumstances. Although Merton is writing as a sociologist, his thinking can apply to applied social science at every level, from intra-psychic to international. Theories of the middle range have clear operationalizations and applied implications, but are at the same time flexible in responding to many different circumstances.

What are the implications for social work? On one hand, for research purposes, we should set aside systems theory and probably also the "problem solving framework"

as too general to be useful for knowledge building.<sup>11</sup> On the other hand, we should push every research project, including evaluations of specific programs and specific interventions, toward key constructs and relationships that are general enough so that results can be compared with other interventions in other places with other people.

**Structures of inquiry.** For applied purposes, it is likely that certain structures of inquiry will yield more productive theories than others.<sup>12</sup> The most common inquiry structure in social work research is “explanation of a negative” (explanation of a problem). This structure typically has multiple independent variables attempting to explain a single dependent variable (the problem). It is the structure embodied in most multivariate statistical methods such as regression and path analysis. Although it is very common, for applied purposes this inquiry structure has not been very productive, because (1) many of the independent variables are not subject to intervention, (2) the cause of a problem may not be the best way to un-cause it, and (3) it is uncommon for a single independent variable explains much of the variance.

Therefore implications for action are usually weak. A better alternative, for applied purposes, would be to work with the inquiry structure that is “impacts of a positive,” where a single independent variable has many hypothesized positive effects (outcomes or dependent variables). Even though each particular effect might be small, the total impacts across many dependent variables might be great. Therefore, implications for action might be strong. Note that this is the inquiry structure for asset building as specified in the multiple hypothesized positive effects of assets (Appendix B). Elsewhere, I have developed this thinking a little further, and applied it to youth service as a “strong independent variable” (Sherraden, 2000).<sup>13</sup>

Another possibility is to consider moving away from the static effects models, and toward dynamic models. It is hard for us to think this way because we are not

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<sup>11</sup> The point here is only that systems theory and the problem solving framework have not been useful for knowledge building in a scientific sense because they do not yield hypotheses that are testable. This does not mean that systems theory and the problem solving framework are not useful ways of seeing and understanding the world, or that they are not useful in teaching professional social work, or that they cannot be precursors to theory that is more specified.

<sup>12</sup> Although I do not directly use their work here, I am indebted to an article by Mitroff and Pondy (1974) for nudging me long ago to begin thinking about structures of inquiry.

<sup>13</sup> This thinking is still in the formative stages and many questions remain. However, it is presented here as an example for considering different structures of inquiry, in contrast to thinking only about different substantive theories. Whether or not the idea of “impacts of a positive” or a “strong independent variable” proves to be useful, the more general point merits attention. For applied purposes, some types of inquiry structures may be much more productive than others, and if so, we should seek to identify their characteristics.

trained for it. In this inquiry structure, it is not effects by *dynamics* that matter.<sup>14</sup> The question is, for applied purposes what type of dynamic are we looking for? A likely candidate is *positive feedback loops*. If we can identify and intervene successfully to create or augment such positive loops, then in an applied sense we are creating "virtuous circles" and continuous improvement. This basic structure of inquiry would seem to hold a great deal of promise for the applied social sciences. For example, using a longitudinal data set, we find support the proposition that assets have positive effects on attitudes and behaviors, and at the same time, positive attitudes and behaviors have positive effects on asset accumulation (Yadama and Sherraden, 1996). This appears to be positive feedback loop that might a target for public policy. Within this inquiry structure, the purpose of social interventions would be to support and augment such virtuous circles. The challenge for the applied social sciences would be to construct dynamic theories with feedback loops and carry out research that might confirm them.

### Conclusion

For the applied social sciences, asking questions well requires that theory to be specified and subject to test. There is no shortcut that is workable. In a practical sense, there can be no useful outcome of an intervention if one doesn't know what it is or what caused it. In an intellectual sense, knowledge does not build without theory. The fact that theory has been used poorly or not at all in much social work research is not a problem with the scientific method, but with its limited application. While some social work scholars have called for outcomes research without theory, or warned against too much social science theory, I fear that eschewing the use of theory would signal an end to knowledge development for interventions and relegate social work scholarship to a stagnant backwater.

On a more human level, I would like to say simply that theory is the way we do our work. In very important respects, theory *is* our work. It is a habit of mind. We think about our theory at breakfast, when we are exercising, and when our minds wander during lectures like this one. Our theory is what we are going for, it is our best thinking, it is what we are putting to test in our research. If we have a theoretical statement that proves to be robust, explanatory, and has applied implications in many different settings, we have done as well as anyone in applied social research can ever do.

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<sup>14</sup> I am indebted to my colleagues at Washington University, David Gillespie and Douglass North, for this discussion of dynamic models.

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## Appendix A. Hypotheses on Institutional Determinants of Savings

### Incentives:

- The higher the matching deposits, the greater the participation and savings.
- The higher the earnings on savings, the greater the participation and savings.
- The more feasible the saving goal (home purchase, microenterprise, job training, etc.), the greater the participation and savings.

### Information:

- The more the program outreach, the greater the participation and savings.
- The more educational programming and "economic literacy," the greater the participation and savings.
- The more peer modeling and information sharing, the greater the participation and savings.

### Access:

- The closer the proximity of the savings program, the greater the participation and savings.
- The more the use of electronic deposits, the greater the participation and savings.
- The fewer the organizational barriers, the greater the participation and savings.

### Facilitation:

- The more involved the program and staff in assisting with savings, the greater the participation and savings.
- The more automatic the system (especially automatic deposits), the greater the participation and savings.

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Sources: Sherraden (1999), Beverly and Sherraden (1999).

## Appendix B. Hypotheses on Effects of Assets

### Economic

- Greater effort and success in increasing asset values.
- Maintenance and improvement of real property.
- Learning and applying knowledge of financial investments.
- Decrease in financial crises in the household.
- More investments in human capital (in addition to formal education).
- Improved consumption efficiency (shopping at supermarket, buying on sale, buying in bulk).
- Decrease in use of second-tier financial services (check cashing outlets, rent-to-own stores).

### Personal

#### Affective:

- Improved self-regard.
- Improved outlook on life.
- Greater sense of personal control over life.

#### Cognitive:

- Greater knowledge of financial matters.
- Lengthened time horizons.

#### Behavioral:

- Better record in attending school, job training, or other personal advancement activities.
- More time spent on financial matters.
- Better planning for the future.

### Family and household

- More stable household composition.
- Decreased moving due to negative causes (unable to afford rent, eviction).
- Increased moving due to positive causes (move to a better neighborhood, move for a job).
- Decrease in domestic violence.

### Relationship to community and society

- Improvement in perceived social status.
- Increase in social connectedness and/or decrease in social isolation.
- Increase in caring for and helping others.

### Civic and political

#### Involvement in neighborhood/community affairs:

- More discussions with neighbors.
- More behaviors to improve public space.
- Increased involvement in community organizations.

#### Involvement in formal political processes:

Increased voting.  
Greater effort in working on or contributing to an issue.  
Greater effort in supporting or contributing to a political candidate.

Intergenerational

Social behaviors of offspring:

Improved school behaviors (attendance, grades, completion).

Avoidance of pregnancy.

Fewer arrests.

Eventual financial well-being of offspring:

Increased savings behavior of offspring.

Increased investments in education of offspring.

Increased asset transfers to offspring.

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Source: Sherraden (1999).