HOW DIFFERENCES IN ANALYTIC PARADIGMS CAN LEAD TO DIFFERENCES IN POLICY CONCLUSIONS--TWO CASE STUDIES

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This paper presents a conceptual framework for understanding the influence of alternative paradigms on policy conclusions. Two types of assumptions are associated with mathematical models--meta-assumptions or methodological priors and specification assumptions. Because two different paradigms must assume two different sets of methodological priors, the possibility exists that different problem definitions and hence policy conclusions may emerge from two parallel studies of the same area. In each of two cases presented here, a single problem area has been analyzed with two different methodologies. In each case, different policy conclusions have been reached as a result of the different methodological priors of the two paradigms. The first case involves two models used to analyze changes in retirement policies within the military enlisted system of the United States Armed Services. The second case involves two models used to analyze the determinants of equal educational opportunity in the United States. The dependence of policy conclusions upon the analytic paradigm employed in a given study has important practical implications for the use of quantitative models in the analysis of social policy situations.

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I. INTRODUCTION

In each of the two cases presented here, a problem area has been studied from two different analytic perspectives. In each case, the differences in analytic paradigms has led to differences in policy conclusions. Such dependency of policy conclusions upon the mathematical perspective of the analyst has profound theoretical and practical implications. It places a responsibility upon the analyst to continually examine his selection of technique as well as his specification and execution of a study from within a given technique. Furthermore, there is a subtle interaction between the selection of a methodology and the definition of a problem. The constraints of different mathematical methodologies force the researcher to precisely define problems so that his analysis will be tractable within the framework of the methodology chosen for the study. This accomodation of the problem to fit the methodology produces generic methodological biases whereby certain methodologies tend to "discover" policy implications well suited to, and defined in terms of, the paradigm's own a priori constraints. Different policy conclusions may be arrived at in separate studies of the same system because the different paradigms guiding each study define problems and discover conclusions that in some sense fit within their respective frames of analysis.

Finally, the dependence of policy conclusions upon the analytic paradigm chosen should remind the analyst of the inherent inconclusiveness of mathematical analyses of social policy. No research can definitely settle a difficult policy question. A different policy study launched from a different analytic paradigm

may always unearth conflicting policy conclusions. The analyst must stay aware of the inherent inconclusiveness of any one study and the continuing need to evaluate one's analytic frame of reference as well as the detailed specifications and assumptions made within a given study.

II. A CONCEPTUAL FRAMEWORK FOR UNDERSTANDING THE INFLUENCE OF ALTERNATIVE PARADIGMS

Figure 1 illustrates the conceptual framework to be used in this paper. The analyst's methodological preferences influence both the way in which difficulties become defined as problems and what methodology is chosen for a study.* Problem definition is not a simple and straightforward matter. In a circular process, problems are defined to fit the methods available and the assumptions associated with a given methodology are more or less rigorously met as the problem is forced into the methodology's analytic framework. Once the problem has been defined and

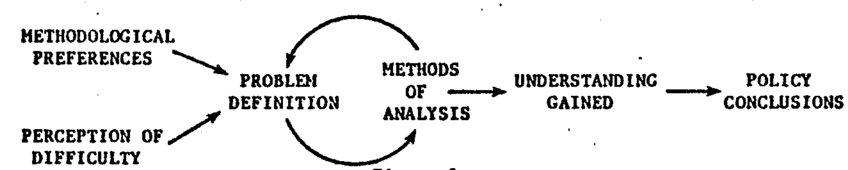

Figure 1.

* A distinction is made between a difficulty and a problem. A difficulty is a generalized concern that draws attention to a given substantive area. A problem is a more precise specification of a difficulty. Of course, a given difficulty may lead to several interesting and fruitful problem definitions.

the methodology chosen, the ensuing analysis generates understanding of the problem area and substantive policy conclusions. The key point of interest in this study is how different methodological preferences can lead to different policy conclusions by influencing the entire problem-solving effort.

II. A. Meta-Assumptions and Specification Assumptions

Quantitative paradigms differ from qualitative paradigms in that the former represent social realities as mathematical expressions instead of less rigorous verbal expressions. These mathematical expressions may take on many forms, such as a closed functional form, a set of logical propositions, or a computer program. In general, some highly abstract functional form can express the generic form of a given methodology. For example, the generic least-squares regression problem would be formulated as:

$$\min_{\text{all } \underline{\theta}} (\hat{\underline{Y}} - \underline{Y})^2$$

$$\underline{Y} = \underline{F}(\underline{\theta},\underline{X})$$

where one searches for the parameter vector, $\underline{\theta}$, that minimizes the squared residuals between the predicted value of $\underline{Y}$, denoted $\hat{\underline{Y}}$, and the observed $\underline{Y}$. The predicted $\hat{\underline{Y}}$ is computed as a function, $\underline{F}$, of the parameters, $\underline{\theta}$, and the observed independent variables, $\underline{X}$. Likewise, the generic system dynamics problem could not be formulated as:

$$\underline{R} = \dot{\underline{L}} = \underline{F}(\underline{L},\underline{\lambda})$$

where $\underline{R}$ is a vector of rates associated with each level. These rates, in turn, are some nonlinear function, $\underline{F}$, of the levels, $\underline{L}$, and a vector of parameters, $\underline{\lambda}$ ($\underline{\lambda}$ may include the parameterization of table functions).

The analyst who sets out to complete a study within the framework of a given methodology knows <u>in advance</u> that his final project will conform to a certain generic form such as those sketched above. Therefore, he must assume in advance that the social reality in question, or at least some significant portion of that reality, fits within the constraints of his chosen generic form.

On the other hand, the analyst has literally infinite degrees of freedom in specifying the model(s) for a given study. Given a reasonably robust generic form and the immense latitude of specifications offered the analyst, the analyst is all but assured that some aspect of almost any complex social situation can be treated within a given methodology.

Consequently, any quantitative study must be underwritten by two quite different forms of assumptions. The first set, called specification assumptions, are what one usually thinks about when speaking of a model's assumptions. These are evoked in a particular specification of a given generic form. They are explicitly stated and usually backed by evidence of one sort or another. They are easily reformulated and consequently subject to manipulation and adjustment by the modeler. They are usually subjected to sensitivity testing and close public scrutiny. For example, in a system dynamics study, the selection of levels, the identification of causal paths, and the formulation and parameterization of the rate equations are all specification assumptions.

The second set of assumptions are methodological priors or biases (see Randers, p. 43). These "meta-assumptions" are usually implicit in the generic form associated with a given methodology. Unlike specification assumptions, the analyst is not readily free to change a priori meta-assumptions (short of leaving a given methodological perspective and either inventing or adapting a new one). As such, meta-assumptions are closely connected to the generic form of a mathematical methodology and tend to dominate the world-view of a given analytic paradigm.

II. B. The Role of Different Assumptions in Model Building

Meta-assumptions and specification assumptions play different roles in the problem definition stage of a study. Specification assumptions must be molded so as to best "fit" the problem statement at hand. In turn, the problem statement must be adjusted and modified so that it may be analyzed within the paradigm defined by a set of meta-assumptions. For example, within a given area of difficulty, a decision analyst will be obliged to uncover utility functions and a system dynamicist must look for closed feedback loops. However, given such constraints, the decision analyst may organize his utility measures along whatever dimensions seem most appropriate, and the system dynamicist will search for the dominant feedback effects.

The mutual accommodation between problem definition and methodological priors seems to be an inevitable characteristic of quantitative social analysis. The skilled modeler is the one who can best merge his problem definition and specification assumptions so as to capture the underlying social reality in an insightful and useful manner. Unfortunately, two skilled modelers of two different methodological persuasions may cast the same area of difficulty into subtly different problem statements, and thereby arrive at conflicting policy conclusions.

Unless one is prepared to argue conclusively that the underlying social reality conforms to a given set of meta-assumptions (for example, the wildcat oil drilling industry conforms to probabilistically-branched decision trees weighted with utility functions, or simple blending problems conform to the assumptions of linear programming), or that a given practitioner is clearly more skillful, it becomes difficult to argue which analysis is best since both are probably solving different problems or focusing on different evidence.

III. MODELING RETIREMENT POLICIES WITHIN THE UNITED STATES ENLISTED MILITARY FORCE

The first case considered involves two models used to analyze the effects of a shift in retirement benefits upon the total personnel costs for the enlisted personnel system of the United States Armed Services. Personnel may advance through a possible thirty years of service within the enlisted personnel system. During the thirty years of service, they may advance through nine enlisted grades (E-1 through E-9). Hence, personnel progress through the system in two ways, both by accruing longevity (length of service) and by being promoted vertically. At any point in time, personnel may separate for one of several reasons--including voluntary separation, force-out, death, or retirement. The particular policy option studied by both quantitative models embodied a proposed cut in retirement benefits.

The problem for both analyses was to compute the cost savings or dissavings associated with such a policy change.

The first model is a static optimization model. The model was developed by an organization within the Department of Defense whose mission was to produce detailed analyses of the costs and benefits associated with various force sizes and compositions. The results of the cost-benefit studies were used to determine short-run force-management policies (such as how many personnel in a given grade and year of service should be promoted next year), as well as to provide detailed information on costs to Congressional committees. This particular organization tends to view force policy questions in static and very detailed terms.

To operate the static optimizing model, the Department of Defense force analysts fed the model a host of hypothetical parameters concerning desired force characteristics. The model subsequently solved for many steady-state conditions that would produce an optimal force structure given the constraints of the hypothetical force characteristics. For example, force analysts could specify how they wanted the force to be distributed by grade, as well as what might be the lowest permissible year of promotion into each grade. The model would then compute the optimal year of service distribution and the hundreds of static promotion and advancement rates necessary to attain that optimum. It would also determine if a given set of desired force characteristics yielded no feasible optimum. The model contains an immense amount of detail and is capable of answering highly disaggregate questions. The model contained details costing equations, and can also discount future force costs into current dollars. Policy analysts responsible for providing cost estimates to Congress for the proposed change in retirement policy perceived these detailed break-outs of costs as essential to adequately performing their missions.

However, because the model contains such immense detail, the analysts had to make several approximating assumptions to retain a tractable level of analysis. For example, promotion, retirement, and quit rate percentages had to be considered constants for the steady-state analysis. Furthermore, the model could not adequately cost out transitions from a present disequilibrium force into a hypothetical equilibrium. Under many circumstances (such as annual force management decisions), these constraining assumptions did not seem overly restrictive, and the benefits of having detailed cost and force-profile analyses outweighed any bias that these assumptions may have produced.

The second model, a more highly aggregate system dynamics model, deals with the identical policy question. The system dynamics study was commissioned precisely because policy analysts suspected that significant interactions might exist between changes in force retirement policies and the quit rates or retirement rates within the enlisted force. For example, smaller retirement benefits might make military service less attractive with respect to outside employment. Consequently, the quit rates, assumed as constants in the static model, might tend to increase. Quite predictably, the feedback emphasis of system dynamics led the second team of modelers to search for and find such effects inherent in the proposed policy revisions.

The resultant system dynamics model aggregates the thirty year-of-service cells into seven aggregate categories. The nine enlisted grades are aggregated into only three categories. Following their prior notions of how such a system should be treated, the system dynamicists redefined the problem as considering how changes in retirement policies might feed back to affect quit rates, retention, enlistment, and promotion rates. Shifts in these rates bring about unanticipated shifts in force composition. Such a redefinition of the problem, made possible by viewing the difficulty from a system dynamics approach, permitted the modeling of the interactions in a fashion not possible within the static optimization approach. Changes in retirement policies fed back to influence the current decisions of personnel to quit or to continue service. In turn, changes in quit rates affect both the actual force profile and the number of personnel reaching retirement.

A trade-off emerges between the two methodologies. The system dynamics model provides richer description of feedback interactions because the system dynamicists define the problem in terms of feedback between aggregate system variables. However, the system dynamics model does not provide a highly disaggregate analysis of force profile and costs. On the other hand, because the first team of modelers initially chose a different form of analysis, the static model answers detailed questions concerning force profile and costs, but cannot capture feedback effects. That is, the static model cannot predict how a change in retirement policies might change the final system equilibrium or affect the model's transition into equilibrium.

Not only did the selection of two different methodologies lead to different definitions of the problem and different approaches to the analysis, but each study produced qualitatively different policy recommendations. Principally, the system dynamics study isolated several shifts in final force composition as potential results of implementing the proposed policy. These shifts in force composition resulted from dynamic shifts in quit rates. Both models showed long-run cost savings from the proposed change, but the system dynamics model produced a short-run cost dissavings over the first ten to fifteen years due to the transient shift

in force composition immediately following the implementation of the policy. That is, higher quit rates would lead to higher recruitment rates and training costs. However, the system dynamics model suffers as a tool for policy analysis because it could not provide the detailed cost analyses usually expected from such a cost-benefit study.

Finally, an attempt was made to synthesize the two approaches; that is, to construct a highly disaggregate model that would also contain feedback between retirement policies and quit or promotion rates. The principle obstacle to this effort was conceptual, although many technical problems would also have to be worked out. In the case of the aggregate system dynamics model, force managers were prepared to estimate the magnitude of the impacts of retirement policies on retention for major categories of personnel--for example, senior enlisted personnel near retirement. However, when confronted with a highly disaggregate structure, the managers encountered extreme difficulty in assessing the myriad interactions between all of the cells in any useful fashion.

In sum, because of their respective differences in world view, the static optimizers were solving a detailed cost problem, whereas the system dynamicists were solving a problem centering on how quit rates change due to feedback interactions among aggregate system variables. Where one study excelled, the other fell short. Their respective policy conclusions were based upon different dynamics in the short run and different equilibria in the long run. Puzzling enough, any attempt to rank-order the studies as to which is "best" would inevitably result in each type of practitioner inventing a set of criteria flattering to his model. Then, of course, comparisons using two such sets of criteria would lead to a complex snarl of contradictions.

IV. THE COLEMAN REPORT ON EQUALITY OF EDUCATIONAL OPPORTUNITY

The second case study, dealing with equality of educational opportunity in the United States, provides an interesting variation on the first case. The two models are less strictly comparable. They were constructed nearly ten years apart, and the conclusions of the first study form the basis of the problem definition for the second. Taken together, these two studies of equality of educational opportunity reflect the interactive nature of quantitative social policy research. They illustrate the role that different paradigms and conflicting policy conclusions emanating from such paradigms can play in a dialectic development of social policy.

In 1964, the United States Congress directed the U.S. Office of Education to:

> conduct a survey and make a report to the President and Congress, within two years of the enactment of this title, concerning the lack of availability of equal educational opportunities for individuals by reason of race, color, religion, or national origin in public educational institutions at all levels in the United States, its territories, and possessions, and the District of Columbia. [Mosteller, p. 4, quoted from Sec. 402 of the Civil Rights Act of 1964]

At that time, the study's principal author, James S. Coleman, as well as the liberal political coalition backing the law expected to find large differences in per-pupil expenditures between predominantly white schools and predominantly black schools. The implicit assumption behind the Elementary and Secondary Education Acts was that a massive infusion of federal funds could reverse gross and unequal discrimination in American schools.

Two years later the report on Equal Educational Opportunity, or the Coleman report, was submitted to Congress. One of the most comprehensive social scientific research efforts every undertaken, it was completed by a task force of undisputed skill and prestige. The report's conclusions were devastating to the prevailing liberal mythologies concerning the import and significance of schooling in eliminating racial inequality. Contrary to popular expectations, the report discovered only small differences in measurable educational inputs between white and non-white communities in the United States. Furthermore, school and teacher variables were found to have little effect in determining student achievement. These results surprised both Coleman and the liberal political coalition that had commissioned the study.

The most controversial sections of the report were contained in its third chapter where regression techniques were used to estimate an educational production function relating educational achievement (the "output") to various educational inputs such as family background, individual IQ, and aggregate indicators of social class, schooling facilities, and teaching quality. Using the regression methodology, Coleman discovered that school and teacher variables appeared to have little impact upon educational achievement.

In its original mandate, the Congress did not specify a particular approach, such as development of a production function, to the question of determining educational equality, nor the use of a regression methodology. Instead, Congress generally asked for an analysis of a broad difficulty. The initial dilemma facing Coleman was to define a problem that would be tractable within a recognized methodological paradigm. He settled on the problem of evaluating measurable educational inputs and outputs. Precisely put, what measurable educational policy variables best explain educational achievement? In part, such a problem definition was chosen because it fit nicely within the regression paradigm. Once he settled on the use of a regression paradigm and the "input-output" problem statement, the dilemma then centered on designing a research strategy that would violate as few of the statistical assumptions underlying the regression paradigm as possible. As illustrated in Figure 1, Coleman was involved in the process of defining a problem to fit within his chosen analytic paradigm.

Following the distinction between classes of assumptions made above, once Coleman selected his approach—use of an education production function with parameters to be estimated by linear regression, he then had to assume a priori that significant policy-making inferences with respect to the national educational system could be captured within the meta-assumptions of that methodological paradigm. For example, he had to assume that major policy variables were measurable with minimal measurement error, that the covariances of such measurable variables were, in some sense, well-behaved, and that it was sensible to aggregate some policy variables (such as indices of school quality).

Next, the Coleman team had to make a host of difficult methodological decisions in their treatment of specification assumptions. As samples of a few, they decided on a purely linear form for a production function, that percent of variance explained should be the measure of the impact of a variable, that cross-sectional data could serve well enough to justify causal inferences, that variables should enter the regression equation according to certain well-defined rules, that nonrespondents should be treated in a certain manner, and that aggregate indices should be constructed according to certain rules.

When all the assumptions had been made and the analysis completed, the policy

conclusions were compelling and certainly counter to established intuition. The contribution of aggregate schooling and teacher indices to explaining inter-school variance in student achievement was small. That is, in general, the major policy variables under the control of school officials appeared to have little or no impact on student achievement. The results were positive in the sense that strong policy implications could be inferred from the analysis (i.e., many forms of direct federal aid to education would have little impact on eliminating inequality).

Both the inferences of the Coleman report and its analytic assumptions came under close scrutiny and attack. In the ten years following its publication, nearly every assumption within the Coleman report was scrutinized and several smaller replications of the study performed. Cain and Watts attacked the use of percent of variance explained as the measure of the impact of a given policy variable because such a measure gives no inkling of which of several alternative policy options open to decision-makers will produce the most benefit at a given expense to the public. For example, even though indices of school facilities have the least measurable effect on educational achievement, they may have a low enough unit cost to make them the "best policy buy" from a cost-benefit point of view. Smith examined the "effects of omitting school placement and self-selection practices on inferences about the relationships between school resources and student achievement" (Mosteller, p. 40). Jencks reexamined the allocation of educational resources in Northern elementary schools and confirmed Coleman's assertion of small differences in educational inputs between predominantly white and non-white schools (Jencks, Ch. 2).

All the analyses of the Coleman report began with an acceptance of Coleman's prior assumption that a production function estimated by linear regression is, in principle, capable of addressing the thorny questions associated with equality of educational opportunity. That is, studies critical of the Coleman methodology and conclusions began with the assumption that the dynamics and interactions inherent in the allocation of educational resources and the impacts of such allocations on student achievement could be addressed within the regression paradigm. The critical studies focused on the proper use of specification assumptions within the regression paradigm. The results of such re-analysis have been murky at best. Some of Coleman's conclusions were crippled and partially disqualified as bases for policy analysis, but no clear-cut counter-example could be raised as long as the analysts remained within the regression paradigm.

The difficulty raised by Congress in 1964 led to the definition of a problem by Coleman (the relationship between various educational inputs and achievement) and eventually to a set of policy conclusions. Coleman's analysis and conclusions were strongly debated, but little consensus was attained with respect to their validity. Eventually, however, the debates over Coleman's results gave way to a new wave of methodological research. Researchers began to ask whether it was possible to solve Coleman's problem by means of the regression paradigm. That is, the arena of inquiry shifted to considerations of meta-assumptions.

In 1975, Luecke and McGinn replicated Coleman's analysis with an interesting twist. They used a simulation routine to generate several thousand synthetic student profiles. Four separate data-generating models were used, each of which assumed a slightly different form for the causal influence of teacher, family, and school variables on student achievement. Although the specification of the four models differed in detail, all of the data-generating models took on a similar functional form (a Markov chain). At each point in time, each student was

associated with a state vector with variables representing his family background, teacher quality, school quality, the community in which he lived (the inputs), and his accumulative achievement (the output). The achievement function either remained the same or increased by one point from one time period to the next. The probability of advancement in achievement was a function of the student's current teacher, school, home, and community variables. The exact form of this causal function varied slightly from one data-generating model to the next. Also, from one time period to the next, the student's school, teacher, or community variables could change according to a predefined causal function. These shifts represented migration from one community or school to another and the student's annual change of teachers. Each model closely followed Coleman's causal specifications, except that, because they were simulation models, the modelers could keep track of the influences of individual student-teacher interactions over time as well as effects due to migration between schools that might occur over time. However, all of the specifications did assume, contrary to the Coleman report, that teacher and school variables had strong positive effects on student achievement.

The synthetic data profiles were then analyzed using four variations on the basic Coleman regression methodologies. The student profiles for the last year of the synthetic data experiment were used as cross-sectional inputs into the variations on the Coleman regression equations. By ignoring the available synthetic time-series data, Luecke and McGinn replicated Coleman's assumption that cross-sectional data was sufficient for making causal inferences. When the synthetic data was subjected to such regression analysis, results similar to Coleman's original results were obtained in most cases. That is, the regression analysis inferred results that were clearly contradictory to the known structural characteristics of the data-generating simulation model.

The regression model made incorrect inferences because it failed to account for the dynamics of a student's progression through school, as well as the wide variety of teachers, schools, and communities that could be experienced by a single child (and that might also have significant impacts on that child's achievement). By posing his problem in terms of a regression-estimated production function, Coleman was led to a view of the educational process and subsequently to conclusions that appear almost obviously flawed when viewed from a dynamic simulation paradigm.

Luecke and McGinn's conclusions raised doubt as to whether that the regression paradigm could resolve the problems posed within the Coleman report.

> Our results suggest that studies which find little or no relationship between educational inputs and achievement may be highly misleading. Our findings suggest that the combination of data and statistical techniques most often used is unlikely to reveal such relationships even when they exist [underline added] .... Researchers who conceive of education mechanistically, and use research designs which ignore the actions of individuals in schools, will find results which confirm their assumptions [Luecke and McGinn, pp. 347-348].

By stepping outside of the regression methodology, Luecke and McGinn arrived relatively easily at a clear counter-example that years of methodological debate within the regression paradigm had failed to forcefully unearth. The mathematical form of the simulation approach allowed for a more structurally rich model that could examine detailed interaction and dynamics below the level of aggregation of the Coleman model.

In fact, Luecke and McGinn self-consciously exploited the fact that, by approaching the problem of equal educational opportunity from a different

methodological perspective, they could generate a set of rigorously-derived policy conclusions to counter the Coleman results. The policy inferences of the Luecke and McGinn study suggested that school and teacher variables might still matter and that policy-makers should continue to strive to improve those school and teacher variables still under their control. These policy inferences could serve as an antidote to the rather fatalistic conclusions of the Coleman study. An even stronger policy conclusion of the study was purely methodological in nature. Even if school and teacher influences were significant, studies based upon the same assumptions as the Coleman study would be unable to discover such influences.*

Finally, the question of comparability between the Luecke and McGinn study and the Coleman study is puzzling. The problem attached by Luecke and McGinn is a product of the problem solution arrived at by Coleman. Because the Coleman model is solidly based in empirical research, it could appear to be a tool for positive policy conclusions. On the other hand, Luecke and McGinn do not argue that their synthetic data represents reality. Instead, they simply used the simulation paradigm to dislodge the empirical results of the regression study.

When taken together, the two studies paint a picture of dialectic evolution in social policy. The Coleman study, based upon the regression paradigm, arrived

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* It could be argued that Coleman's faulty assumptions were specification assumptions (non-use of time-series data) rather than meta-assumptions. However, this point is fairly moot given that nearly ten years of methodological debate within the regression paradigm failed to unearth the problems with dynamics and aggregation as clearly as did the Luecke and McGinn study. It might also be argued that the differences in policy conclusions stem principally from non-quantitative ideological differences between the Coleman team and Luecke and McGinn. This argument loses much credence when one realizes that the Coleman team was as surprised as anyone else over the counter-intuitive ideological implications of their study.

at a compelling set of policy conclusions for American educators. Leucke and McGinn, by attacking Coleman's methodological priors, inferred a substantially different policy picture. After years of empirical and methodological controversy, the debate over equality of educational opportunity in the United States remains ill-defined and unresolved. Definition of the problem derived from Coleman's paradigmatic perspective appeared to give some resolution. But when viewed from a different perspective, both the definition of the problem and its alleged resolution appeared to weaken and lose validity.

V. SUMMARY

The two cases treated here demonstrate how differences in analytic paradigms may lead to differences in policy conclusions. In the first case, the system dynamicists defined a problem focusing on feedback interactions between retirement policies and quit rates. The static modelers, guided by their prior preferences for detailed optimal solutions, defined a problem that required solving for detailed force profile and cost characteristics. Each model arrived at slightly different policy conclusions due to the differences in analytic paradigms employed.

The second case was more subtle. Leucke and McGinn used a synthetic data-simulation model to provide a counter-example to the policy conclusions of the original Coleman regression study. Conflicting policy inferences appeared as the simulation paradigm was used to launch a methodological critique of educational production functions estimated by regression.

Although Section II of this paper has presented some theoretical basis for believing that such conflicts in policy conclusions <u>might be</u> inherent in any quantitative analysis of social policy, there is no apparent and easy way of knowing in advance whether or not a given set of policy conclusions are critically dependent on the particular paradigm chosen for analysis. Consequently, the two case studies suggest that a type of empirical uncertainty surrounds quantitative analyses of social policy. Each study must be viewed as but another step in a dialectic search for social policy conclusions. The analyst must recognize both the certainty that his conclusions depend upon the methodological priors or meta-assumptions underlying his study and the probability that his conclusions may be contradicted or dislodged by a subsequent study based upon a different set of methodological priors or meta-assumptions.

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