The Central Characteristics of Professor Friedman's Analysis and the Issue of Normativism

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I. Introduction

Professor Friedman's "analysis" is normative; his policy discussions are exhortations of what one ought to do, without an explanation of how to implement these desired ends. That is, no specific behavioral rules of policy or conduct can be inferred from Friedman's arguments. This normativism arises from a combination of methodological errors: a confusion of simplification with falsification; a confusion of direct and indirect observation with the distinction between real and false statements, a discussion of laws which suggests an omission of long sets of links in the causal chain under examination; and an analysis which suggests that axiom sets are sources of unknown but discoverable relationships, rather than being elegant compressions of known information.

II. Causation

A generally acceptable definition of causation might be: causation is a regular, predictive association between directly or indirectly observed events which is expected to continue and which can be assimilated into an extensive superstructure of families of related associates, these sets being organized by theories of event relationships. This definition of causation is in harmony with a desire that theories be simpler: the formulation of a theory out of a set of causal associations requires that the explained event be placed into a relationship with some small number of other events, according to our ideas about the connections among these events. However, if some of the event sets which are placed in the matrix of logic surrounding the explained event are false, i.e. if we have no direct or indirect evidence of their existence, or if we have evidence of a discrepancy between our re-construction of an event; and our observation of it, then we either isolate the event set we seek to "explain" by failing to provide it with connectors to other events, or we distort the linkages which we posit will connect it with other events, nearby it in the matrix of logical and empirical inter-connection. Simplification of the logic matrix which surrounds an explained event is quite different from falsification in its effects: when we simplify in a causal discussion we abstract much from the infinite number of measurements and observations which could be made of the process under investigation, and concentrate our interest upon a sub-set of those possible measures, the particular subset suggested by the ideas we have developed about similar processes investigated in the past: which events in particular are chosen to group with the explained event are not a priori obvious. For example, there are no events which are "obviously" remote and unconnected with the speed of some experimental falling body. The position of the sun in the heavens might be thought to be remote from the speed at which a lead ball falls in a vacuum chamber in New York. However, if we believe that gravitational attraction accounts for the speed of a falling body, and if we believe that the sun has a gravitational attraction for all things in the universe, the speed of fall of the lead ball might be expected to change slightly when the sun's gravitational attraction was added to or subtracted from the earth's relative positions of sun, falling body and earth were altered. Thus, our answer to the question "how or why does the body fall?" seeks in seeking for events and dimensions of events which are associated with a body falling. Many events, indeed most events, will be related to any specific falling body in so tension and indirect a fashion that we eliminate them from our explanation of falling bodies, and from our predictions of calculating for falling bodies. However, the process of eliminating is not easy: it depends upon the superstructure of hypotheses about other related events and upon the hypotheses we have about the speed of falling bodies.

The evidence "for" a theory is, then, not just the failure of its predictions to be contradicted, but its ability to fit into the family of theories which are associated with closely related phenomena. The insertion of false observations into the set of notes we make with respect to some falling body can only distort the relationships we would then calculate among our accurate observations. Those distortions would make this most recent observation of falling bodies less trustworthy than previous observations of similar events, and isolate and make useless our experiment. Framing the falsifications in the "if" manner does not close the logic gap.
III. Observational Distance

However, Professor Friedman often reminds us that economic data is "indirect" and remote from the "real" events under study. He then suggests we analyse some relationship as "if it" if its existence is posited some characteristic we suspect it does not actually have (in one of his examples, he suggests that leaves on a tree behaved "as if" they made a conscious decision to follow the sun).

Following Friedman's advice would cause us to frequently falsely relationships we observe indirectly. However, it would be clear that although observation is rarely, if ever, "direct" and although existence is always tested for by looking for interaction between extant thing and environment, "intermediation" does not falsify data. For example: we observe electrons just as "directly" as we observe billiard balls through intermediation of eyeball, optic nerve, light waves, particles, accelerators, etc. An observation is not made true or false or empirical or empirical simply because we observe it "added" or "unaided." A billiard ball becomes no less empirically verifiable if a weak-eyed academic needs spectacles to see up on it; an electron does not become metaphysical because it is too small to pick up with our fingers. Both billiard ball and electron interact with their environment in complex ways—reflecting certain wave-lengths of light, contracting with green felt, leaving trails in bubble chambers, and bouncing into eight balls and/or protons. It is these interactions we observe (our internalized perceptive devices being part of the interactive process, of course) and not the entity itself.

The importance of historical reality has been presented by the logical empiricists. Their main contention is that things without existence cannot be the subject of empirical discussion. A sentence makes a cognitive meaningful sentence, and thus can be said to be either true or false, only if it is either (1) analytic or (2) capable, at least in principle, of experimental verification, or of cognitive significance, or of both. Many of the formulations (theoretical) are devoid of cognitive significance.

A sentence is empirical meaning that if only if it is not analytic and follows logically from some finite and logically consistent class of observation sentences.

The empirical criterion of meaning is initially at least, that objects or qualities of objects are capable in

Each and every element in a scientific hypothesis, if it is presented for testing, must be testable: at least indirectly. We may not be able ourselves to "touch" or "see" an electron but we can "touch" or "see" its interactions with its environment; which is, after all, all that we ever touch or see. We must never confuse observation remoteness with non-existence or with " falsification" or with the absence of reality. Such a confusion is understandable of the three concepts: falsification, observational distance and reality.

A causal chain may be falsified in a causal distance way if we posit "cause in state of hunger plus environment containing cheese minus cheese eating" and then test with a kangaroo. A result (no cheese eating) or a result (cheese eating) tells us nothing about the validity of the original proportion: we simply didn't test it, and we have yet to learn about it.

Observational distance may require us to watch the kangaroo via television or by mirror or on film: but this would not thereby change the kangaroo into a mouse: nor would we deny the existence of the kangaroo simply because we couldn't "really" observe him—whatever that means. The picture of the kangaroo on film is not the kangaroo—but the kangaroo is not the chemical change that goes on in our brain when we "really" see him either—the kangaroo is the kangaroo... (but will never be "really" or "directly" observed save by the kangaroo himself). Only a kind of solipsism would cause the rejection of a realistic test for assumption because of the "indirect" nature of the economist's data.

Non-reality, or non-existence, in the positivistic sense, means an inability to observe all ("directly" or "indirectly") and any attributes possessed by that thing which doesn't exist.

Things which are unreal in this sense, of course, cannot be part of a causal chain because they do not exist: not even in our imagination.

"Things" without any existence are a contradiction. Such "things" certainly could not add to the predictive or explanatory power of a causal chain or matrix, nor would they find any place in the families of related logical structures which provide us with our picture of the world.

IV. Friedman's Reality

Therefore, Professor Friedman falls into a rather complex error in his discussion of these preceding points: first, he confuses abstraction with falsification, and then his related "as if" statements lead him into further falsification. That is, in his remarks about "indirect testing," Friedman, who knows that cause-effect relationships fit into a structure of similar relationships, attempts to solve the "observational distance" problem by inserting falsified elements into the cause-effect relationships which he examines. Now related families of substance.

"Many writers begin with Eugen B. Kornrath have made this point and developed it carefully. I will not repeat their work. Kornrath's initial review of "The Methodology of Positive Economics" appeared in the AJE in 1953, placed by him, Kashou, and Gershom in Knapp (ed.) The Structure of Economic Science and dealt with it and many related issues.

Here is the offending Friedman quote: "In so far as a theory can be said to have "assumptions" at all, and in so far as their "reality" can be judged independently of the validity of predictions, the relation between the significance of a theory and the "reality" of its "assumptions" is almost the opposite of that suggested by the view under criticism. Truly important and significant hypotheses will be found to have "assumptions" that are widely inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). The reason is simple. A hypothesis is important if it "explains" much by few, that is, if it accounts for the essential elements from the mass of details and the distinguished circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone. To be important, therefore, a hypothesis must be described falsely from its assumptions. It takes account of, and accounts for, some of the other causes in the phenomenon, or at least it shows them to be irrelevant to the phenomena to be explained."

effect relations may be consulted by an experi-
menter if he wishes by contrast with the studied
act. To test for the reality or currence of particular data. But
the experimenter may not get his observation of
some critical event-element from situation B if
he is experimenting in situation A; this would falsify.
A by inserting into it extraneous ma-
terial. This form of falsification is doubtless
the most dangerous sort, since if the erotic material
has been transferred from an event set very
similar to that under study, the illogical
sentences may be difficult to uncover later.
Friedman may in part agree with this last
analysis, although he does not seem to under-
stand its inconsistency with falsification:
"another way in which the 'assumptions' of a
hypothesis can facilitate its indirect testing is by
bring ing out its kinship with other hypothe-
sest here in question. For example, a hypothesis is
formulated for a particular class of behaviour.
A hypothesis can, as usual, be stated without
specifying any 'assumptions.1 But suppose it
can be shown that it is equivalent to a set of
assumptions including the assumption that man
seeks his own interest. The hypothesis then
joins indirect plausibility from the success for
other classes of phenomena of hypotheses that
can also be said to make this assumption: at
least, what is being done here is not completely
unprecedented or unuseful in all men. In
clude, the statement of assumptions so as to
bring out a relationship between superficially
different hypotheses is a step in the direction of
a more general hypothesis.2
A 'kinship' between an examined event set
and those examined in the past can only be
established if real links connect the new set
with the old: falsifying the new set in the
manner Friedman has elsewhere suggested
makes those links impossible to fashion. To
say that an event set and a sub-set of the
same set, must be placed in the logic matrix which
connects it to all other cognitively meaningful

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curacy cannot be depended upon, especially in
a changed environment. Professor Friedman's
method of taking a macro-relationship, which
crudely relates money and income, which
has not been specified at the macro level of the
causal links, and which refuses to consider the
"reality" of the micro links, thus leads him
into a fundamental contradiction. I assume
that Friedman wants the predictive relationships he
has "discovered" to be used to design controls
which will then modify the behaviour of some
of the elements related in the predictive schema.
That is, he desires that his long study of the
predictive relationship between money and in-
come be used in order to determine the desired
behaviour of the money stock, which is assumed
to be determined by the desired income.
However well we might be able to predict income
by measuring the money stock, we will never
gain any knowledge, any understanding,
any explanation of the determination of
either the money stock or of national income.
We can talk about a "predictive association
between money and income, but we cannot be very certain of obtaining
predictions in which facts can be put
This analysis suggests that predictive events
which are not imbedded in this context of re-
lated events and related hypotheses about
events, perhaps cannot be used to provide
guidance to public policy. We do not know the
size or direction of whatever cause influence
might exist between a methodologically "iso-
lated" predictive event and the rest of
the economy.
Taking Professor Friedman's relationship be-
the money and income, we may note that an
observed relationship between the money stock
and the level of income, if provided with no
macroeconomic explanation, gives little hope for
the success of a policy rule which suggests vary-
ing the one variable in order to determine the
other. If we have no understanding of the com-
plete micro-economic set of connections which
(may) link money with income, we have little
faith that controls will be successful, and less
knowledge of appropriate control design.
VI. Lags

In the "lag" and in velocity, Professor Friedman has aggregated many of the micro variables which affect income. The resulting aggregates do not exhibit stable relationships with other macro variables, they do not seem consistent with known, real behaviour of the individual units of aggregation nor do they imply very much of the complexity which we feel exists in the relationship between the stock of money and income at the micro level. Also Friedman generally chooses to calculate his "lags" between two endogenous variables, money and income. As has been pointed out above, the nature of the relative movement exhibited by these two events will depend upon the exogenous disturbance which has induced the change in equilibriums. Thus the "lag" will vary in duration with respect to the type of exogenous disturbance which occurs. Once again, the discovery of such predictive instability is a signal that two endogenous variables are being used as if they were an exogenous-endogenous pair of events: such procedure is in methodological error. Friedman does not find a stable relationship between the stock of money and the level of income in the short-run: the relationship is not even very specific: it is characterized by a long and variable lag.\footnote{The issue is whether our knowledge of the "circumstances" of past associations between money and income produces expectations about future associations between them. It is in this sense that the "predictive" power of the hypothesis is important; however, a hypothesis which explicitly rejects the possibility of the application of such a test by asserting "long and variable" lags between saw money and the induced change is (say) NNP cannot be said to be an empirically verifiable hypothesis. To verify such a thesis by determining that our knowledge of the conditions prior to a change in the money stock is insufficient to give us a finite estimate of the type of response which the "unconditioned" variable may take, could be said to be an empirically verifiable hypothesis. To verify such a thesis by determining that our knowledge of the conditions prior to a change in the money stock is insufficient to give us a finite estimate of the type of response which the "unconditioned" variable may take, could be said to be an empirically verifiable hypothesis. To verify such a thesis by determining that our knowledge of the conditions prior to a change in the money stock is insufficient to give us a finite estimate of the type of response which the "unconditioned" variable may take, could be said to be an empirically verifiable hypothesis.} But the "lag" may be an analytical artifact.

It can be eliminated by relaxing the level of aggregation, and allowing more of the world of content's complexity to show. Friedman's lag exists because the money stock and the level of income are not connected in a proximate way. A non-Friedman connection between "money" and income might be: as a change in the monetary authority's conception of the need for action (this is the exogenous event) results in a change in the Fed's bill/bond portfolio, which changes security market liquidity, which changes bank liquidity and market interest rates, which changes desired portfolio positions, which changes the balance between expenditures upon securities and goods, which changes the level of output (latter changes are all endogenous). This analysis is also aggregative. Whether it is excessively so depends upon the use to which we put the analysis. For the analysis to be useful, relationships must be stable and predictable and they must fit into a larger hierarchy of relationships. Associations that occur only with long, variable, unpredictable lags may not often be found to be useful, since the existence of the "lag" may be an illusion which may mislead our attempts at prediction or control. The "lag" may not really exist. Causal sequences must always confine their elements in time. We cannot speak of Event A causing Event B unless they exist serially in time. Billiard ball B rolls away from ball A after A strikes B; we may speak of the pool player's beginning the game in the "cause" of his player's initial attempt to strike ball B with ball A, but the

entire useful sequence between the pool player's initial attempts and the final roll of B exhibits no gaps or lags. Only if the observer ignores certain elements in the complete chain of events will gaps or lags in the chain "exist." Ignoring certain elements in the complete micro chain of events or aggregating some of the events in the chain, therefore creating gaps or lags in the chain, may be perfectly appropriate and necessary to certain analyses. However, it always is incorrect to misaggregate or ignore certain elements in the chain in order to "prove" their unimportance or non-existence.

For example, Professor Friedman aggregates many non-monetary forces which operate upon income into his concept of "the lag" and velocity. He then tries to show that these non-monetary forces are of small consequence in determining income. This is not correct procedure.

VII. Axiom Formation

Intimately related with all of the above is another point that Professor Friedman does not appreciate that a test of the reality of assumptions is never different from a test of a hypothesis implication at long or the assumptions are real, and that if the assumptions falsely reality, it is of no use looking for the "reality" of the hypotheses' implications; because no causal significances can be found between the false assumptions (non-existent assumptions) and their implications.\footnote{See Bear & Orr J. P. E., 1968.} An empirically grounded axiom set is simply an elegant summary of the state of our knowledge in some area. Such aggregates are built up by first accumulating many diverse but related observations of event sets: this is a statistical procedure. Then the relationships discovered are so simplified in their presentation, that all observed event sets may be "discovered" in the resulting highly condensed statement of relationships: if this is a logical procedure. The resulting axiomatic statement of relation has no "as-sumptions" different from its predictions. In some deductive event relations one particular statement may be "prior" to another and thus in some sense may be an "assumption" which, in combination with the axiom set may "predict" some other, previously implicit, statement; however, in other contexts, these two statements may reverse their roles, one becoming assumption, the other implication. In all cases, no falsification may be present in the axiom statement since it is composed only of logical rules and event set combination observations, one may there be said to be any "truth difference" between any "assumption" and any "prediction" contained within the axiom statement.

That the "reality" of assumptions is necessary to understanding, explanation and to useful prediction is clear from this Hilbertian notion of axiom formation. In the Hilbert formulation the assumptions of a theory are simply an elegant and condensed statement of all the "predictions" of that theory. The assumptions of a logical system is a model simply codify in a simple and elegant manner, the extent of our present knowledge or understanding of a phenomenon. Thus if the predictions of a model have an empirical existence, the assumptions of that model must also have an empirical existence. Any assumption without empirical existence is thus incapable of generating any testable empirical predictions and can be cut away from a testable hypothesis with the Razor of Occam. Or more accurately, since no assumption is built into a model without having been given birth out of an empirical event, then any "false" assumptions are the result of faulty hypothesis construction.

That is, taking the Hilbert view, the importance of reality is at great for an assumption as for a prediction. From a purely formal point of view, the places of assumption and prediction could be reversed and would be reversed if the result would be a more compact or more elegant statement of the theory.
However, although the predictions of a model have an initial a-priori validity and existence, there must also be an independent existence from the axioms, assumptions and initial statement of the model. As we have said, the assumptions of a model codify in a simple but elegant way, the extent of our present knowledge or understanding of a phenomenon. This is not to say that the predictions which give us new information must not have a separate existence from this known structure, but rather that they must be related to it in an integral way. Useful predictions cannot simply be assumptions in disguise, else at best we gain no more understanding or information than we presently possess, and at worst we produce an elegant but structureless dream. As in the statistical methodology, when we have used data to establish some model, "further tests of the model" consist of "predictions" (which see) then made about the phenomenon under study. New data must be collected to test these predictions. The data should not include any of the observations utilized in making the original estimates or first revisions of the model."14

Thus these new data are the new information which may modify the earlier model.


The rule which instructs us to test a model with data other than that used to construct the model results from a concern that we avoid "discovering" that the relevant relationships used to construct it are "structured" by the axioms of the model (see Hiltz-Morgenson on Axiom formulation as a synthetic process) do indeed exist. The practice of ignoring the issue of the "reality" of assumptions would allow us to test a model with the very data used to construct it. Indeed, Chissom seems to come close to this error in testing the relationship between the quantity theory, rather than making "real" predictions for the demand of (say) a particular subset of consumers based upon present knowledge of the stock of money, they prefer to estimate such connections for (say) a period between fifty years in the past and ten years in the past until yesterday. Nothing is wrong with such a procedure, but the fact remains that the results of this "test" are known in advance.

Thus the assumptions of a model are synthetic—they may be empirical-synthetic or analytically-synthetic. Although conclusions can be drawn from a model irrespective of whether or not its structure of assumptions represents a codification of empiric or analytic statements, predictions which give new information can result only from a synthesis of empiric statements about the real world which are then "put through" an axion act.15

This analysis of synthetic nature of assumption formation again leads us to a showing of the necessity of reality in understanding, to the establishment of causal chains, to dependable predictability in the face of change, and to the design of control systems. Axioms are built up out of the external world. They are not some set of self-evident a-priori "truths." Rather, a set of axioms corresponding to some areas of our understanding is simply an elegantly and efficient way of writing down the comprehensive state of the art. The assumptions, the axioms thus must be "realistic" in that they comprise our knowledge, our understanding of some "real" phenomena—the axiomatic statement of our knowledge of the behaviour of firms comes from our observations of real firms. The axiomatic statement is a translation, elegant and simple, of those observations into a spatial language, which enables us to store information efficiently.16

Axioms do not lose their empirical foundations upon transformation—they may gain elegance, simplicity, and efficiency, but they cannot thereby become self-evident truths nor can they become independent of "reality." An assertion that the reality of axiom is irrelevant exhibits either a misunderstanding of the new Heurifront nature of the axiom or an ad

turance to the Old Euclidian notion that they contain self-evident non-empiric "truths."

If one is dealing with an empirical field, the axioms will be statements about some part of the real world—for example, one might say that the speed of light is constant. In no case have the axioms any superior truth value to that of their implications as they are brought to light in the deductions which can be made upon the axioms. The latter are chosen for reasons of convenience, because they are intuitively acceptable and express in agreeable and perhaps aesthetically satisfying form some basic knowledge of the field in question. But they are not self-evident truths as the old and now completely superseded view of an axiom asserted. The axiomatic method is simply a superb technique for summarizing our knowledge in a given field and for finding further knowledge deductively.17

Therefore prediction in which we have confidence and from which we gain sufficient information to design control systems can only arise out of an analysis where both antecedent and consequent have an empirical existence.

The set of axioms which comprise our knowledge of some aspect of economic behaviour is what we call a model of that behaviour. All the elements of that model are truth relevant, regardless of whether those elements are called goals, controls, assumptions, data or whatever. The vital characteristic of a set of axioms viewed as a model is, from my point of view, the fact that there are inter-determined nature of the elements of the axioms. The causal loops which give the model its interdependence also make optimal exogenous control behavior specifically dependent upon desired endogenous goal behaviour and the model's data. Thus the optimal control statements, viewed as part of an axiomatic structure, cannot be made independently of real world data, descriptive of the complete set of statements which make up the model.


fall into the "single goal error"—our analysis becomes utopian.

It is the main characteristic of utopian thinking that if only a particular utopia is adopted, no hard choices between values and objectives are necessary, since the particular utopia will provide *all* real desiderata, liberty, equality, and fraternity; or freedom, social cohesion, stability, economic and political justice in distribution, the highest rate of economic growth full employment—and if there are any other desiderata they are really not worth desiring.19

The monetary proposals of Chicago are utopian in this sense, since, they arise out of an analysis which either ignores the existence of or the incoherence of empirical objectives of trade-offs, on both the utility and technological sides.

A genuinely positivist policy designer, who wishes to moderate the costs of unemployment and price instability would proceed in the following way. First, he would estimate the technological trade-off between the endogenous rate of price change and unemployment; second, he would estimate at least in some gross way, the utility trade-off between these same elements; third, he would make a determination of the difference between the actual mix of unemployment and inflation and the constrained utility maximum mix. The exogenous policy device chosen to close the gap between the actual employment-inflation mix and the desired mix is the one which does the job quickest, with the least interference with other utility values. No single "best" policy device will exist.

What is "best" will depend upon the particular utility function being maximized, the nature of the technological constraints, and the changing weights being applied to both constraints and values. During wartime, the losses to "economic freedom" and efficiency consequent upon the use of price controls is outweighed by the urgency of the need to eliminate an overt inflation which could re-direct resources needed to fight the war. Later on, price ceilings as a policy device are abandoned because their costs are no longer balanced by such urgent need.

The single goal fallacy, or the idea that trade-offs between goals can be neglected, is associated with his desire for simplicity and the consequent attempt of Friedman to develop a policy tool which is at one time both capable of hitting many goals and incapable of unforeseen or unplanned variation.

Single goal models neglect the central problem of policy design: goal conflict. If more employment means more stable prices, no real policy problem exists, "since the policy objectives are not all scarce."20 In the Friedman models, "economic freedom" is the single desideratum which is to be maximized, irrespective of any probable or possible trade-offs between economic freedom and a "fair" income distribution, or a "full" employment level, or a "smooth" pattern of economic development. Even if the world were simple enough that a constant rate of growth in the money stock would result in a constant rate of growth in (attained) full employment, it is unlikely that some additional goal, a desire to change, say, the distribution of income could also be attained without some modification of the Friedman rule. Full employment under the Friedman rule might well imply a rate of capital formation which is not equal to the system's desired rate of capital formation: the key-point is that the possibility of goal incompatibility resulting from too few or too invariant controls. The Friedman rule's inflexibility makes it uneconomical to change in the community's utility trade-offs amongst goals. Thus the Friedman rule can be appropriate to only some unique utility function. If we believe that an important dimension of economic freedom consists of an ability to change the goal structure or goal weightings of our society from time to time, adoption of a wide ranging reform scheme which is appropriate only to some single set of goals and goal weights may severely limit freedom.

Professor Friedman talks about the utility surface; But this hardly causes him to become normativist. He rarely if ever estimates a utility function but relies upon assertions of their shape: this may cause us some worry over his positivist status, but perhaps not enough to call him normativist. But the peculiar form of the policy proposals put forward by Professor Friedman by their very design, cause those proposals to shift, distort, and modify in a substantial way almost any utility function we may have begun with, until it agrees with the implicit Friedmanian goal set. This is sufficient cause for our labeling Professor Friedman a normativist.

IX. Motive

I am arguing that Professor Friedman adds to the list of goals and thus multiplies the number of axes of the welfare surface; he often changes widely accepted trade-offs between goals because of the peculiar design which his methodology imposes upon all his proposals for monetary reform. The addition of goals is the result of a lack of micro-specification in his theoretical descriptions of the links which connect monetary with real variables, combined with (and associated with) a desire for simplicity; simplicity in some absolute sense, independent of the complexity for analysis of a problem, both in theoretical presentation and in final control design.

Professor Friedman's lack of micro-specification of the several chains of causal links which can be thought to connect monetary control variables with goal variables and to connect goal variables with another one may also allow him to concentrate upon some small number of those links, whose reality is not examined; those links perhaps being chosen, consciously or not, for their normative implications. F. A. Lutz puts it well:

Every change of an economic condition spreads its effects fan-wise throughout the whole system. Such a change in data sets off causal chains running in all directions. It is not possible for the theorist to follow out all of them completely... He therefore has to make a judgement as to which causal chains he holds to be more important. But it is always at least conceivable that his judgment is quite unconsciously influenced by his political convictions. According to his attitude in principle to state intervention he may, possibly, from the start, search for those causal chains which justify interventionist measures, or the reverse. One economist, for example, will emphasize unfavorable frictions which will obstruct the reaching of a new equilibrium when a change in data occurs. Another economist will make light of such frictions, and will push his analysis rapidly through to the new equilibrium condition in which everything is all right again.21

However much it may be difficult to demonstrate the motivations of Prof. Friedman in order to show that his method, which leads him to insert new goals and change old ones, when combined with his clear statements of political belief, results in goal structures quite "conservative" in content, we can always consult another observation of Lutz, who noted both the difficulty of establishing proofs for normative motive, and the associated natural ease with which suspicions on the question come to our mind.

I cannot provide any concrete proofs because, of course, no economist will say straight out that he uses a short-term treatment because he favors state intervention, or a long-term treatment because he is opposed to it. These influences work at the subconscious level. But the possibility of his influence certainly exists and I personally have no doubt at all that it is actually at work in many cases. The relations between the simplificatory selection of causes and political bias, is also apt to be specifically close in those more casual theories which need

18Hutchinson, p. 177.
19Hutchinson, p. 178.
to be more influential than they are today. (Emphasis added.)

It is the micro-causal nature of Friedman’s monetary theory that lays the foundation for the normative bias of his policy design. In a micro-causal hypothesis,

An analysis of causal responsibility or imputa-

tion is confused or combined with an analysis of moral or political responsibility, “guilt,” scapegoat hunting, or with a kind of “essential-

ist” dogmatism as to what the “real” cause is.  

X. Fixing the Blame

The macro economic theory of Friedman, one which relates the level of income to the level of the money stock (or sometimes to the rate of change in the money stock) finds a moral and political focus in money. Money, an “essential-

ist” “real” causal agent, becomes a kind of moral agent. By comparison, a theory which describes the causal determinants of the level of income as many and complex, and which views the economic system as being composed of a vast number of simultaneously determined mutually dependent events provides no moral focus, no guilty policy maker, upon whom to place the “blame” for the failure of the system’s outcome to provide an optimum distribution of income or rate of growth. One of the important con-

tinuous characteristics shared by several of the most important members of the “Chicago School,” (Knight, Mints, Simons and Friedman) is the existence of this moral focus: in Fried-

man especially it is a focus upon money managers; money managers who are made to bear most of the “guilt” for the business cycle. This concern for guilt, this “scape-goat hunt-

ing,” is not characteristic of positivist policy design.

Therefore there may be a relationship be-

tween highly simplified causal structure and political bias, and it is evidenced by the exis-

tence of the unexamined12 “moral focus” found in such presentations.

The neutrality principle requires not only that positivists scientists refrain from adding peculiar elements to the utility function, but also that they be neutral between means, between policy variants which are substitutes in their ability to maximize identical utility functions. However, policy instrument may themselves appear in the social welfare function.

“It is quite obvious that values are attached not only to “ends’ but also to “means.” Means are not ethically neutral. The value judgment must compare and choose between alternative courses. Value judgments thus refer to whole sequence, not merely the anticipated final outcome.

Now a great concern has always been felt by “Chicago School” monetary reformers for the “appropriateness” of their policy suggestions under the assumptions of a utility function possessing heavy weights for “economic freedom” and “automaticity.” Indeed, it would seem that, in arriving at their 100% money plan, Chicagoans relied almost exclusively upon an analysis which presumes a widespread repug-

nance on the part of society to discretionary policy making. Prof. Friedman says:

“Money is too important to be left to central bankers” the problem that suggests this text is the one of what kind of arrangements to set up in the context of a large scale discretionary policy. The believer in a free society—i.e. funda-

mentally fearful of concentrated power . . . (he believes) that this objective is best achieved by dispersing, that it be prevented from accumulating in any one person or group of people. (Emphasis added.)

12 “Blame” for the “failure” of monetary policy could not be left at the door of the Fed, even in a simple world, without an examination of the utility function which the Fed is attempting to maximize.


14Assassins, The Bراكson, p. 128.

counter currents that they cannot be expected to choose "appropriate" policy, would involve a complex political and psychological treatise that no economist qua economist is trained to conduct, and that Professor Friedman has not presented. Therefore, if Prof. Friedman is taking position three, that the net benefits of automatic policy always exceed the net benefits of discretionary policy, he is once again found to be either making an unsupported empirical assertion or to be inserting some special psychic costs for discretionary policy into the social utility function.

XII. Full Target Trade-Off
The point here is that norms are not ethically neutral. Thus agree with Prof. Friedman that the design of policy devices must be undertaken with the entire utility function to be maximized always in view. If it is decided that the community welfare function is sensitive to variations in "economic freedom," and that the level of economic freedom varies inversely with the degree of discretionary power in the hands of the monetary authorities, the policy designer may take this into account, just as he takes into account the increased utility level consequent upon an increase in the level of real income. This paper is thus wholly heartedly in agreement with the position that policy design may be affected by attitudes toward policy action. The Friedman analysis implies a utility function which is sensitive to the degree of automaticity in the exercise of monetary policy: this is an interesting and possibly realistic assumption. However, such a assumption cannot lead us to conclude that policy action should be perfectly automatic anymore than a statement that the utility level is sensitive to the level of employment could lead us to conclude that we must have full employment. A non-Rational (albeit simple) analysis which postulates non-infinite utility weights associated with a set of many conflicting goals, goals which have trade-off relationships with one another in both a technological sense and a utility sense, will very seldom lead us to conclude that certain goals are always to be fully attained, regardless of the level of attainment of the other conflicting goals.

Thus the true implications of the Friedman postulate that general economic freedom is reduced by policy maker freedom can be known only in a non-Friedman analysis which explicitly considers the nature of the relationships between general freedom and policy maker freedom and between these freedoms and the other conflicting goals of the community's inhabitants.

Therefore there is a broad sense in which Prof. Friedman has confused ends with means. Prof. Friedman and others have correctly pointed out that the means of policy are not value-free. We may have a utility sensitivity to the level of discretionary power in the hands of the monetary authorities. This sensitivity, however, cannot be placed in any special class of utility sensitivity: the utility weight associated with the losses to freedom consequent upon discretionary monetary policy actions is not necessarily infinite. Therefore to assert that we are sensitive to some of the means of policy is not to know the design of the optimal policy program. To say that some means have an ends commutation is not to place those means-ends in any special supremely important value category. However, the Friedman analysis makes this kind of means-ends confusion: ends associated with the exercise of some means acquire, in a strict way, not only an ends significance, but one much greater than that possessed by any other "pure" end.

XIII. Instrument Variables as Shock Absorbers
The Friedman rules for monetary reform have many serious implications. The finally, it fails because continued adherence to its inflexible rule in the face of failure to achieve target "A" on the grounds of the desirability of simplicity and automaticness means that a substantial utility preference has been...
assigned to these new goals; a preference which causes their attainment at any cost in the achievement of "A".

A positivist control designer will assure us that his proposals do not contain hidden special utility functions only if his designs allow reasonable changes in "consumer" objective functions to induce some response in his control; only if environmental changes induce similar responses; only if his proposal contains no policy-designer valuations which enforce special policy manipulations, apart from those required by changing "consumer" objective functions or changing situation environments.

XIV. Conclusion

We may employ the analysis developed above to elucidate the errors in Friedman's charge that recent inflation has been caused by an excessive increase in the money supply. 36 Since Friedman does not look to real behavior when he defines "money" he does not realize that the local quantity of the means of payment cannot be measured in a society which possesses credit cards, Eurodollars, International Consercia Bank loans, and foreign headquarter's treasuries for multinational firms. Therefore, Friedman assumes that money is an exogenous instrument when in fact it is endogenous, he cannot tell us what the necessary manipulations in the true instrument variables of government must be if we are to achieve the Friedman targets for the behavior of the money stock. Since Friedman has not specified the micro economic causal links which connect the instruments of government policy 37 with the targets of policy, his mysteriously long and variable lags prevent him from being able to predict any particular dated outcome from any particular dated exogenous monetary action.

Since Friedman has confused a possible cause of (say) inflation with its possible cure, he does not realize that although monetary events might give rise to inflation, the cure for the problem could be a non-monetary redistribution of wealth and income back to the initial state of the world via, for example, the tax and subsidy routine.

Since Friedman's world ignores the reality of monopoly power, and the reality of the constant struggle of shares of the economic pie conducted by organized groups in society, he remains unaware that inflation and increases in the stock of money may both be caused by third events.

Professor Friedman's method permits him to build models which are highly aggregative, monocausal, undirectional and much simpler than the complex goal system he implies in his discussions of optimal control behavior. Most important to Professor Friedman's ability to design control systems without consideration of the empirical validity of his implicit (and unusual) objective functions, is the a-priori simplicity with which his models are constructed, which allows him to build models which contain no explicit objective functions, and which contain no discussion of the probable rate of trade-off amongst competing goals in the objective function. If Professor Friedman were to admit to the possibility of conflicting goals and independently effective controls, if he were to utilize models of sufficient micro-specification to show the effects of such inter-dependencies, and if he were to distinguish between endogenous and exogenous events, he would have to admit that "proper" monetary policy could only be defined after a knowledge of utility functions for goals, the rates of trade-off, and the nature of the connections amongst the goals and the rest of the system, including control linkages, was obtained, perhaps in the manner of Tinbergen. I would suggest that models whose "simplicity" is truly optimal, that is, whose simplicity is related to the complexity of the goal-control statements we make about them, will not be in general show invariant control behaviour. Also, we could not evaluate the degree of success which was attained by such a systems policy makers by applying the Friedman post hoc tests. In optimally simple systems, possessing goal-control-data interdependence and simultaneity, the function of controls will be to vary in an extravagant way, thus absorbing data system variation, and thus preventing the transmission of this variation to the goal set, and thus ideally providing us with an empirical record showing the instrument variable gyrating in a most complex manner, while the target variables in the goal set are completely invariant.