Notes on the Methodology of Economics
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Economics is an attempt to create images of the economy in the minds at least of economists, and perhaps some others, which are in some sense “maps” of the real world. Methodology in economics relates to the question as to how these images of the economy are changed in ways that lead towards “truth”, that is, more accurate correspondence between the image in the mind and the real world which it is supposed to represent. No image can represent in its entirety the immense complexity of the real world. This is particularly true of images of social systems like the economy. The world economy includes over five billion human beings, each with about as many neurons in the head as there are stars in our galaxy. Even though the economy only consists of a small part of all this complexity, we cannot possibly imagine in our mind the billions of transactions, acts of production, and patterns of depreciation and consumption, which take place every single day.

We achieve these images of the economy partly by “thinking”, that is, using the extraordinary capacity of the mind for imagination, and for producing generalizations from experience. Thinking produces something that might be called “models”, which have some resemblance to space-time maps. These models do not have to be mathematical. Adam Smith’s model of the equilibrium of the relative price system was not really much improved on when it was mathematized by Walras. Part of the thinking process involves a perception of taxonomies, that is, classifications - what things are alike and what things are different - and a perception of identities, like the famous Fisher identity PT = MV, which follows from the definitions of the concepts involved. Mathematics is useful here, as it is fundamentally a study of identities, that is, relationships which by definition cannot be otherwise.

In order to have an image of the real world, however, we must go beyond identities and discover the parameters of the system, using the form “How much does x change when y changes by one unit?” Things like the gravitational constant or the velocity of light are examples from the physical world. Price and income elasticities of demand may be an example from the economy. We have to be careful, however, in systems which involve information as an essential variable, as the economy does, in that the parameters of these systems are likely to change quite unpredictably. Thus the predictions of econometricians, based on the parameters of the 1930s, were quite falsified in the period after the Second World War because some of the essential parameters of the economy had changed. The system that economists are studying is a little bit like what celestial mechanics would be like if the planets were moved by angels who didn’t like astronomers.

Nevertheless, we can perceive certain patterns over time in the economy. Here the development of regular observations like national income statistics has done for economics rather what the careful measurements of the positions of the planets by Tycho Brahe did for astronomy. The problem of data collection and reduction is a very difficult one in these complex systems. Measurement is valuable, but has to be done with extreme care, for measurement always involves a loss of information. We say that the population of the human race is 5.1 billion, but this assumes that all human beings are alike, which they are not. It is very easy to get into false taxonomies, as already did. It is perhaps unfortunate that economics tended to get divided into methodological schools - an historical school, an econometric school, and so on - whereas a system of the complexity of an economy requires many different methods in varying proportions. All the different methods, however, begin with thinking and go on to some form of testing.

On the Attribution of Causality and Responsibility in Macroeconomics
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Introduction
Economic theories often involve, or appear to involve, statements about causality. As economics is a social science, the elements of a theory that appear to play a causal role are often familiar human activities, for example, making business decisions or consuming. If the theorist or his audience have not been involved in such activities themselves, they have almost certainly been exposed to them in some way, and have attitudes towards them that pre-date the formulation of the theory. The elements of a theory that play the role of consequences are likewise matters about which the theorist and his audience have opinions formed prior to the appearance of the theory. As a result, a statement that in a strictly theoretical sense appears to be causal may come to be interpreted as a statement about moral responsibility. That is, if a theory seems to identify a certain activity as a cause of some desirable consequence, there is a tendency to use that theory as a basis for giving the people engaged in that activity credit for the consequence. In the same way, those whose activities are causally linked to an unpleasant economic outcome may be assigned blame for that outcome.

The ease with which an argument about causality can become an argument about responsibility is often demonstrated by discussions of economic policy. A change in economic policy will almost inevitably lead to a redistribution of wealth and power, whether or not redistribution is the putative reason for the policy change; and conjectures about the nature of this redistribution are usually derived from some economic theory. Economic theory also often provides justification for a policy change and associated redistribution, justification based on an assignment of responsibility to certain classes of people for the situation the policy change is meant to address. Such assignments of responsibility are often based on, but go beyond, causal links identified by the theory.

In this article we discuss some of the complex features of the relationship between the identification of causality, the assignment of responsibility, and the formulation of economic policy. The relationship is complex. First, because the question of causality is itself complex. Considerations of multiple causation, alternative causation (a consequence that can result from several different causes), reversibility, length of the chain of causal reasoning, the relationship of temporal sequence to causation, and so on, are extremely important and severely condition and limit what is acceptable as knowledge. On top of these complexities, as we shall show, are further complexities surrounding the way in which assertions about causality can metamorphose (or fail to metamorphose) into the assignments of responsibility that often appear in discussions of economic policy measures. We argue that in the midst of these complexities, selective perception guided by values and ideology operates to determine what sort of policy propositions is derived from a given economic theory, and also which theory among competing theories is chosen as a basis for making policy decisions. We attempt to make our points with reference to the standard Keynesian model of income determination.
Consider the equation \( A = B + C + D \). A varies both as the sum of \( B + C + D \) varies, and as any one of \( B, C, \) or \( D \) varies, insofar as that change is not offset by an equal and opposite change in the sum of the other two, and so on.

Query: If one of \( B, C \), and \( D \), say \( B \), should fall, does it make sense to say that the fall in \( A \) is due to the fall in \( B \)? Does it make sense to say that the fall in \( A \) is due to the failure of \( C \) and/or \( D \) to rise sufficiently to offset the fall in \( A \)? As a matter of strict mathematical logic, the answers are, yes. However, to the extent that one tends to think in economics from a *ceteris paribus* position, then the fall in \( A \) is traced directly to the fall in \( B \). The fact that \( C \) and \( D \) have failed to compensate is, in this context, a secondary issue. Discussion, however, does not always follow *ceteris paribus*, especially with regard to the attribution of responsibility as distinct from causation (although the difference between the two may be muddied).

Consider next the conventional income-expenditure equation, \( Y = C + I + G + X \), in which income is equal to the expenditure from which it is derived, the latter disaggregated to consumption plus investment plus government plus net export expenditures. Let us assume that either (1) consumers become uncertain about the future and reduce their purchases of durables, thus a fall in \( C \); (2) an increase in interest rates due to Federal Reserve tight money leads to a fall in purchases of consumer durables and investment, thus declines in \( C \) and \( I \); (3) an increase in liquidity preference leads (either directly or through changing interest rates) to portfolio rebalancing away from real investment, thus a decline in \( I \); (4) a reduction in government spending on either military or domestic programs, thus a decline in \( G \); or (5) a reduction in exports, thus a fall in \( X \).

Queries: Given the fall in \( Y \) due to the decline in \( C \) in (I), does it make sense — analytically, in terms of moral responsibility, and/or for purposes of policy — to say that \( Y \) has fallen because \( I \) and \( X \) have not increased? Given the fall in \( Y \) due to the declines in \( C \) and \( I \) in (2) consequent to the rise in interest rates, does it make sense to say that \( Y \) has fallen because \( C \) and \( X \) have not increased? Given the decline in \( Y \) due to the increase in liquidity preference in (3), does it make sense to say that \( Y \) has fallen because \( C \) and \( X \) have not increased? Given the fall in \( Y \) due to the decline in \( G \) in (4), does it make sense to say that \( Y \) has fallen because \( C \) and \( I \) have not increased? Given the fall in \( Y \) due to the decline in \( X \) in (5), does it make sense to say that \( Y \) has fallen because \( C \) and \( I \) and/or \( X \) have not increased? (And the reverse in situations in which income increases either in real and/or nominal, perhaps inflationary, terms.)

The answers are not so clear. If income, output, and employment fall due to a decline in one category of spending, it does not a priori seem to make sense to place responsibility on the failure of the other categories to rise. Or does it? Are the categories equal or comparable in this respect? If consumption should fall, it might be due, through the multiplier, to a fall in income consequent to a fall in investment. But does it make sense to place the responsibility for an autonomous decline in consumption on the failure of investment to rise in a compensatory manner? It would seem not. If an increase in consumption leads, through the accelerator, to an increase in investment, and the latter is inflationary, then does responsibility fall on the increase in consumption or the increase in investment? Or on the monetary authority for failing to control the supply of money? It is not clear that responsibility is unequivocal but judgment as to responsibility will likely have some impact on the choice of government policy instruments for combatting inflation, assuming an activist anti-inflationary policy program.

If investment should fall, through either a decline in expected profits and/or a rise in interest rates, does it make sense to say that the consequent recession is due to the failure of consumers to consume (or to)? If the decline in the marginal efficiency of capital is due to a fall in consumption, then it might make sense. What if the decline in the MEC is due to businessmen's expectations of a fall in consumption? This seems to be a quite different matter — one involving self-realization if the fall in \( L \) leads to a fall in \( Y \) and

then, through the multiplier, a fall in \( C \). If liquidity preference should rise and investment fall, does it make sense to say that \( Y \) has fallen because consumers have failed to spend more and thus make real investment more attractive? One thinks not. If income, output and employment should fall for any of the reasons stipulated above, does it make sense to say that the unemployed are unemployed because of their own actions, that unemployment is voluntary in the sense that they could putatively secure employment if they reduced their reservation wage rate? Alternatively, if income, output and employment should fall because workers increase their reservation wage rate, does it make sense to say that the declines in income etc. are due to the failure of liquidity preference to fall so as to lead to portfolio rebalancing leading to increased investment? Or to the failure of consumers to spend more? Or to tight monetary policy?

The questions of the preceding paragraph illustrate an important feature of the relationship between causality and responsibility in the theory of national income determination or in any macroeconomic theory. The elements of a macroeconomic theory are aggregates, and the causal links identified by the theory are links between those aggregates. However, attributions of responsibility derived from theory often run in terms of the people whose activities give rise to the aggregate value. Indeed, it sometimes makes little sense to assign blame to an aggregate, e.g., to hold "gross investment" culpable for falling when such a fall initiates a recession. It is a different matter, however, to hold individual investors culpable for their decisions not to invest, and it would not be unusual to see discussion of the redistributive effects of a recovery policy revolve around such an assignment of blame. An important issue, then, is the shifting of the locus of causation/responsibility from macroeconomic variables to the economic agents whose behaviors determine those variables. There are two quite different contexts of responsibility, however great the tendency for the discourse of one to flow over into the other.

What all of the foregoing suggests is that the identification of causality and the assignment of responsibility (and praise or blame) is essentially a matter of subjective attribution — the selective attribution of responsibility itself, often comprehended in terms of causation, is at least partly subjective and goes beyond any strict notion of causation. Further, it seems that the subjective attribution or perception of causality and/or responsibility is often driven by ideology. It is ideology which places responsibility-blame on consumers for failing to spend more in the face of declines in \( I \), \( G \) and/or \( X \). It is ideology which places responsibility-blame on government for the failure to compensate for a contractionary decline (inflationary increase) in one of the other categories of spending. It is ideology which places responsibility-blame on workers for not lowering their reservation wage rates in the face of increased unemployment due to declines in \( C \), \( I \), \( G \), and/or \( X \). Conversely, we have never heard of responsibility-blame being placed on asset managers for not increasing their liquidity preference and thereby reducing real investment in the event of inflation; nor have we ever heard of responsibility-blame being assigned to workers for not increasing their reservation wage rates in the face of inflation. It seems that the ultimate policy decisions are not the choice of policy instruments per se but the assignment of responsibility-blame for the economic performance considered undesirable (itself a policy judgment).

Judgments as to responsibility in these matters, however, affect not only considerations of policy. They also affect the practice of thory, especially the introduction of lines of reasoning as to how the economy does (often how the economy should) work. But as with policy, this is a matter of selective perception driven in part by ideology.

What Governs the Attribution of Responsibility?

We have suggested that the question of causality following a change in a variable, especially insofar as it involves the attribution of responsibility, can be answered in a number of ways, and that selective perception helps to determine the answer actually chosen.
We also believe that it is common for people to see in a theory or analytical construct causal statements which strictly speaking are not there: that attributions of responsibility are often based on beliefs about causality even though the two concepts are analytically distinct; and that the allegedly positive (as opposed to normative) process of identifying causal flows is often colored by the tendency to base attributions of responsibility upon beliefs about causality, so that this process is to some extent subjective and normative. In this section we expand on these three assertions, and consider the question of whether there are any regularities in the derivations (correct or incorrect) of causal statements from theories and analytical constructs and the associated assignments of responsibility.

Consider again the abstract mathematical statement A=B+C+D. Suppose B falls and A fails. If you asked whether the fall in B caused the fall in A, many would say that it did. When you mentioned the failure of C and D to rise, some might change their initial responses, but many would feel they had been asked a trick question, and in a sense they would be right. Properly speaking, nothing can be said about causality in this example. The example involves nothing beyond mathematics, and causality is not a mathematical property. Mathematical properties, as opposed to scientific or causal properties, remain invariant under operations that produce logically equivalent expressions.

However, people will generally jump to conclusions about causality when considering the question about the joint movement of A and B, because they will employ certain prior assumptions they have about how to handle a question of this form. They may assume that because of the way the equation is written (A=B+C+D rather than B=C+D-A^2), B should be regarded as an "endogenous" variable and B, C, and D as "exogenous" variables. They may assume a sort of inertia: objects at rest tend to stay at rest in the absence of some intervening force. B violates inertia, so attention is drawn immediately to B's movement, and whatever might be behind it, when an attempt is made to explain the movement of A. Or, it may be assumed that the question of what caused the movement of A is a question about an efficient cause, a cause that in and of itself could be responsible for the movement in A. "C and D stayed the same" could never by itself be an explanation of movement in A.

Of course, discussing causality with reference to A=B+C+D is rather different from discussing causality with reference to Y=C+I+G+X and the other equations of the national income determination model. In the latter case the equations represent processes about which meaningful statements on causality can be made. This does not, however, eliminate the possibility that people might believe the national income determination equations to imply certain causal relationships and responsibility that they in fact do not. Once again, people might read into the equations certain assumptions that are not there, and use those assumptions to establish or refute statements about causality and responsibility. The assumptions may be suggested by the form in which the equations are written, in the manner described above. However, the tendency to read additional context into the income determination theory (or any macroeconomic theory) must certainly be magnified by people's familiarity with and resulting preconceptions about the things that theory is meant to represent.

Indeed, the elements of a macroeconomic theory are not only things people know about, but things they care about. People care about whether G goes up, or Y goes down. Further, each of the macroeconomic aggregates represents the outcome of millions of activities undertaken by millions of individuals, and any given person is likely to have opinions about those activities and individuals. Such opinions will influence people's perceptions of what causal inferences can be drawn from a given theory or analytical construct, in part because of a sense in the back of most people's minds that causation is strongly correlated with responsibility.

Let us set aside for now, however, the problems involved in discerning the nature of causality, and assume that the issue of causality is unambiguously settled. How then will information about causality influence the assignment of responsibility? In the A=B+C+D example, there is little sense in distinguishing between causation and responsibility, credit, or blame. All these terms have neutral and roughly identical connotations when applied to abstract mathematical symbols. In the case of the national income equation, however, our intimate concern with those things represented by Y, C, I, G, and X makes causation and responsibility separate matters, and raises issues regarding the possibilities of transferring responsibility from economic aggregates to the people whose activities jointly generate the economic aggregates. For example, we may readily admit that a fall in autonomous consumption caused a fall in national income, but balk at placing any blame on consumers because we find nothing blameworthy in the myriad acts of utility maximization that generated the fall in consumption. We could instead hold the government and its agents responsible for failing to respond to the fall with compensatory spending. In any case, how responsibility gets assigned in any given macroeconomic situation is of more than passing interest. The policy measures taken in response to macroeconomic events have distributional consequences, and one criterion used in deciding between competing policy proposals is the extent to which the redistributions they entail are consistent with people's beliefs about the allocation of credit or blame for the events that inspired the policy proposals.

There are many forms of argument that are designed to establish or deny links between causal statements made on the basis of macroeconomic theory and the assignment of responsibility to individual actors in the economy, some more common than others. The easiest path to take when drawing conclusions about responsibility from beliefs about causality is to identify responsibility with causality at the macroeconomic level, then shift from responsibility in the aggregate to responsibility of individual actors. In short, in the event of a macroeconomic problem, the people whose activities ultimately (are believed to have) caused some problem bear the responsibility for it. They are thus expected either to act in a way that will alleviate the problem, or to bear the redistribution burden of policies designed to correct the problem. This form of argument could be called the first principle of blame assignment, and it can be used in assigning credit for desirable economic occurrences as well as blame for bad ones.

The first principle is very robust, but there are many cases when our ideologies, social philosophies, preconceptions, etc. lead us to reject the blame assignments that follow from the first principle. There are counter-arguments to be employed in such cases, arguments that often rest on breaking the link between the aggregate and the individual activities that make it up. For example, it may be argued that when individual members of an aggregate cannot be expected to understand one macroeconomic cause of those actions they should not be held blameworthy. Even if they do have a sense that these behaviors, in conjunction with the behaviors they could reasonably anticipate on the part of others, may lead to problems, we might argue that they should not be held accountable. They should not have to worry about what others might do. (The paradox of thrift gives rise to this type of argument: the savers are not blamed, even though they are understood to be the cause of the problem.)

When will people accept as reasonable an argument that short-circuits the first principle's link between macro-causes and micro-activities? We would argue that when people find an activity objectionable purely for its own sake, then if that activity is thought to cause undesirable macroeconomic consequences, those who engage in the activity will be generally held responsible for those consequences, regardless of their knowledge of having generated them. Likewise, if the activities generating an undesirable consequence are in and of themselves considered meritorious or at least unobjectionable, those engaging in such activities will be shielded from blame with arguments about unforeseen or unintended consequences.

For example, most people believe that utility or wealth maximization is unobjectionable, within certain limits provided by the community's ethical standards. So, consumers deciding not to consume cannot be held accountable for the unforeseen consequences
of this act, nor can portfolio managers who go bearish. However, if consumption behavior crosses certain lines (e.g., consumer debt gets "irresponsibly" high), or if the behavior of investors becomes "speculation" or even "over-speculation", then because gambling and spendthriftiness are objectionable in and of themselves, protection from blame is withdrawn. Protection from blame is also withdrawn from the corporate management that closes a plant not unequivocally on the basis that closure will abet profit maximization-loss minimization.

Of course, different people will, as we have seen, draw the line between objectionable and unobjectionable self-seeking behavior in different places, and thus assign blame in different ways. The theories of market failure and of government failure, of rent-seeking, and so on, respectively define reality in certain ways so as to influence the perception of objectionability and responsibility and, thereby, policy. The workers who will not lower their reservation wages are also a case in point: By one standard, these workers are lacking in the work ethic, and are thus to blame for any problem that befalls them, as well as hardships that befall others as a result of their slothfulness. In the eyes of the New Classical economists, however, these workers are doing no wrong. They are maximizing utility through the consumption of leisure. They are not suffering hardship, and neither is anyone else, since the economy is presumably at its "natural" level. (There is obvious selective perception of injury and of evidence of injury.) An intermediate position absolves the workers and assumes away their problem by invoking the idea that they are maximizing utility, but still sees macroeconomic problems caused by this, which are then laid at the doorstep of the government, because government programs present workers with incorrect incentives.

Placing the blame on government involves a form of argument that sometimes reinforces and sometimes invalidates the first principle. The argument holds that if members of an aggregate can be thought of as having a shared goal, and if they can understand the consequences of their actions, they bear the responsibility for those consequences, and for correcting any problems they have caused. Further, if these people have an understanding of the consequences of the actions of others, they are expected to react appropriately to those consequences, and can be held accountable if they do not. For example, if government spending is held to have caused an inflation, then the government and its agents are to blame, and are expected to rectify the situation or suffer some (presumably electoral) consequence. This assignment corresponds with the first principle. However, suppose that expenditure on residential housing decreases because of rising interest rates, leading to a recession. Typically, individual contractors and potential home-buyers are shielded from blame, and the blame comes to rest instead with the central bank. After all, it is felt, the central bank should understand the deleterious effects of rising interest rates on the individual actors to which they lend, and should thus act to keep interest rates from rising. Underlying this form of argument is an assumption that there is an individual or group of individuals with a mandate to take corrective action when faced with a macroeconomic problem. Of course, the issue of who does and who does not have a mandate is a matter of selective perception, ideology, and so forth.

One implication of the foregoing discussion is that blame, responsibility, and credit are assigned on normative grounds, in part, through our opinions as to what activities are objectionable or unobjectionable, our assumptions as to what knowledge people should and should not be expected to possess, and so on. That is part of the matter, but there is more. When we enter the arena to discuss who is to blame for some unwanted turn of events, or who is responsible for setting the situation aright, we come armed with arguments we hope will be persuasive to others. We do appeal to ethical standards, but we are also willing to invoke the first principle: if you caused it, you are to blame for it. And when we point the causal finger, we often claim to have a positive, scientific theory to back us up. If we can argue, with the aid of economic science, that a particular activity J caused movement in C, I, G, or X leading to adverse movements in Y, we have seized the high ground. Our opponents may employ counter-arguments to rescue activity J from

incrimination, but these counter-arguments also presume a knowledge of a link between actions and consequences, a knowledge presumably provided by some positive, scientific theory — so that the contest can become one between rival positive, scientific theories. However, the A = B + C + D example has shown us that theories may sometimes seem to point a causal finger, when after closer inspection they do not. It is possible to see more in a theory than is there, or to warrant a theory by reference to another theory, due to the words that are chosen in its exposition, or to the listener's prior expectations (including something as innocuous as the inertia principle mentioned above).

In any case, it seems that economic theory, with all its recognized or unrecognized normative trappings, plays an important although not necessarily conclusive determining role in our assignment of blame or responsibility. But there is one more point to make, or rather to reiterate. There is not only one theory, but many, and we must choose the one on which we will base our arguments over responsibility and policy. Our conjecture (by no means original) is that if a theory can be construed (correctly or incorrectly) to implicate causality for some undesirable event to an activity we feel is blame-worthy on other grounds, then that theory will be easier for us to swallow. A theory that seems to grant the duty or ability to engage in policy action to a group we feel should be held responsible for taking or not taking such action, will likewise ring true for us. On the other hand, if we must constantly rescue some favored activity from blame assigned in accordance with the first principle after the causal finger has apparently been pointed at it by some theory, we may soon be on the lookout for a new theory. A second conjecture is therefore that a theory which passes muster because it comports with either our ideological preconceptions or prejudices or the reigning paradigm, will be readily invoked to attribute or deny causality and/or responsibility. A further example of this lies in the difference between interpreting unemployment as due to excessively high wage rates (or reservation wages) and emphasizing that the levels of employment and unemployment are derivative of the level of output.

Establishing which lines of macroeconomic theory are valid becomes a matter of a framework of preconception. Empirical results are not always conclusive and are possessed of subjective elements; they are a matter of selective perception and intuition and selective attribution of responsibility. Values have been one of the driving factors behind the development of macroeconomic theory. One mode of the operation of values is the functioning of ideology to place blame and responsibility. But the assignment of moral culpability always requires a moral theory. To the extent that attribution of responsibility is possible in economic discourse there is already implicit moralizing: the task is to make the moralizing explicit, in part thereby to penetrate the veil of ideology. The task will be difficult enough, especially in the face of consequences with multiple causes, fallacy-of-composition relationships, public goods (externalities), and the situation of failure to act. The point is, of course, that we already do pursue these judgments, through, in part, the application of some of the forms of arguments we have discussed above.

Normative concerns shape perceptions which then shape our positive analyses. This can lead to the inference that separating the normative and positive is desirable. Whether it is possible is another matter. It probably cannot be accomplished, at least conclusively. At the very least, we must retain a deep and abiding sense of the presence of the normative and the subjective, and avoid the pretense and hubris of scientificity. Tensions like this drive the discipline. But policy is driven by more than what transpires within the discipline: it is also affected by general perceptions, in part as manipulated by politics, and by interests reducing to income and wealth (re)distribution.

Conclusion

It is necessary to distinguish between the identification of causation and the attribution of responsibility, especially in matters of policy. Each is subjective and normative in its own way, which means in part that one must also be sensitive to the selective
attrition of scientificity. In addition, while we also must distinguish between mathematical logic and economic content, and between macroeconomic variables and microeconomic actors, it seems that ordinary practice, for better or worse, in seemingly following the guidelines or rules adduced above, necessarily makes subjective judgments in working out conclusions as to attribution of responsibility.

1. If unemployment can be caused by various things, such as changes in liquidity preference, does it make sense to say that the unemployment are voluntarily unemployed? This question brings the following points to mind. First, any observed unemployment rate will depend on both the relevant (other) macroeconomic variables and the behavior of workers. That is to say, (1) for any given behavior of workers, the observed unemployment rate will change with changes in the (other) macroeconomic variables; and (2) for any given level of the (other) macroeconomic variables, the unemployment rate will change with a change in workers' behavior. Since it is the combination of workers' behavior and the level of various macroeconomic variables that account for any observed unemployment rate, it is clear that neither variable necessarily can account for the unemployment rate on its own, but selective perception has wide opportunity.

However, the point just discussed seems quite distinct from the point being made by those who suggest that all unemployment is voluntary. This latter point is simply the reason that for any given level of the (other) macroeconomic variables, workers could change their behavior in such a way as to eliminate unemployment. The problem appears to lie in the way this idea is perceived and used. In particular, there seems to be a tendency to move from this true positive statement to the normative statement that in order to lower unemployment, the best, or only, course of action is for workers to change their behavior. Given the fact that the (other) macroeconomic variables are also subject to manipulation, there does not seem to be any particularly strong argument connecting the normative policy suggestion with its positive antecedent. But this does not mean that the social logic of attribution cannot reach this result, as it so often does, a situation which can be designated "blaming the victim".

In this context, the original question, about whether the notion of voluntary unemployment makes sense, seems almost to obscure what may be the more important issue: raising the essentially linguistic problem of whether or not one can express the fact that voluntary behavior can eliminate unemployment without at the same time implying that voluntary behavior in isolation defines the unemployment rate, or that voluntary changes in behavior are necessarily the cause of observed changes in the unemployment rate. This implies that the statement, "all unemployment is voluntary", or variants thereof, is ambiguous and not to be used to express merely the notion that behavior is relevant to unemployment, although many use the phrase intending to connote more than that. Again, its use is a matter of selective perception in the formulation and application of theory.

2. From a "causal" perspective the two expressions seem different, but from a mathematical perspective they are equivalent. Causality, exogeneity, endogeneity, dependent and independent variable status, and responsibility are matters not of mathematical logic but of a complex process the use of the mathematics is intended to model. For example, the identity, \( MV = PT \), when adopted for use in representing the quantity theory of money, in which \( P \), becomes the dependent variable and \( M, V, \) and \( T \) the independent, represents a combination of mathematical logic (the mathematical properties), economic logic (the reasoning behind the identity), and the theorizing represented by the quantity theory. When the effects of assuming Say's law or the equivalent are combined with the quantity theory, the result is to rule out \( V \) and \( T \), because whatever is unaltered or at a maximum can be treated as a constant, making \( P \) a function solely of \( M \). That is a result of additional theorizing.

3. While we concentrate on macroeconomic examples in this article, the same problems arise in microeconomics. For example, consider an increase in the demand for a product, with a consequent rise in its price. Does responsibility reside with demanders (none of whom presumably intended the increase in price and who, under classically competitive conditions, individually did not have the market share to affect price), or with the failure of suppliers to increase supply? Much of modern discourse is channelled by invocation of the role of increasing costs, but to that can be juxtaposed such other factors as non-competitive market structures, unduly high reservation prices, and so on.

An international example, which encompasses both macroeconomics and microeconomics, is also apropos. Assume a budget deficit and trade deficit. Is the trade deficit a result of too little saving, excessive government spending, the low quality (objectively or subjectively) of domestically produced goods, foreign government policies, domestic investor behavior, and so on? Or does the trade deficit represent only gains from trade, such that criticism implies criticism of utility functions? In comparison with the usual treatment of utility functions, the "undersaving" argument about government and trade deficits, especially given that net saving is concentrated in upper income brackets, is functional with regard to (re)distribution.

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Policy Simulations with Econometric Models

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Econometric models are often used to simulate the effects of macro-economic policies. Although criticized by New Classical economists, as well as by post-Keynesians and some others, most journal editors appear to treat the results of such policy simulation as highly plausible. But recently Jeffrey Frankel and Katherine Rockett (American Economic Review, June 1989) argued that policy simulations of different models diverge too much to use them to coordinate the macro policies of various countries. If Frankel and Rockett are correct, this suggests that policy simulations of econometric models should not be treated as serious scientific evidence. To see if this is so, I compare here the simulations of monetary policy by various models in three different studies. Different models giving similar results is, of course, not a sufficient condition for the validity of equally plausible models, but it is a necessary condition.

One way to measure the differences in simulation results is to take the estimated change in nominal GNP that results from a given monetary policy in each of the models, and to calculate the coefficient of variation among these estimates. But this only measures the deviation of each model's estimate from the mean of all the estimates, and there is no reason to assume that the mean is the correct measure. It is therefore useful to employ the following stricter test: Assume that one model provides the correct estimate, but that a policy-maker is no more likely to pick this model than any of the others. Then, one way of estimating his or her error is to calculate a measure, called here the "coefficient of divergence", that is similar to the coefficient of variation. However, instead of differences from the mean, it uses the absolute value of the difference between the estimates of all the models taken two at a time. It then expresses the sum of these differences as a percent of the sum of the estimates for all of the models. To illustrate for the case of estimates from four models, \( a, b, c, \) and \( d \), the coefficient of divergence is:

\[
\frac{|(a-b)| + |(a-c)| + |(a-d)| + |(b-c)| + |(b-d)| + |(c-d)|}{6}\]

I calculated this coefficient of divergence, as well as the coefficient of variation, first for some of the models used by Frankel and Rockett. These models come from a Brookings Institution study (Empirical Macroeconomics for Interdependent Economies) that compared the simulations of the same monetary policy in several models. Since not all the models simulated exactly the same policy, I choose five models for which any differences in the simulated policy are trivial. Another source is a 1976 study by Gary Fromm and Lawrence Klein (Annals of Economic and Social Measurement).
Winter 1976) that compared the simulation results of five models. Unfortunately, in this study the simulated policies were not exactly the same. This study covered eight quarters, while the Brookings study covered six years. Since one of the models in each study was clearly an outlier I show the results, both including and excluding these outliers:

<table>
<thead>
<tr>
<th>Period</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient of Variation</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Brookings excluding the outlier</td>
<td>.79</td>
<td>.67</td>
<td>.52</td>
<td>.49</td>
<td>.39</td>
<td>.32</td>
<td>na</td>
<td>na</td>
</tr>
<tr>
<td>Fromm and Klein excluding the outlier</td>
<td>1.34</td>
<td>1.16</td>
<td>.80</td>
<td>.66</td>
<td>.63</td>
<td>.59</td>
<td>.57</td>
<td>.58</td>
</tr>
<tr>
<td></td>
<td>Coefficient of Divergence</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Brookings excluding the outlier</td>
<td>.98</td>
<td>.80</td>
<td>.63</td>
<td>.59</td>
<td>.47</td>
<td>.37</td>
<td>na</td>
<td>na</td>
</tr>
<tr>
<td>Fromm and Klein excluding the outlier</td>
<td>1.50</td>
<td>1.38</td>
<td>.99</td>
<td>.79</td>
<td>.76</td>
<td>.69</td>
<td>.66</td>
<td>.68</td>
</tr>
</tbody>
</table>

Most of these coefficients are extremely large for the early periods where disagreement about the lags of monetary policy reinforces disagreement about the total (final) impact. (That some exceed unity does not imply that the coefficients of some models have the wrong sign; for example, the coefficient of divergence between two estimates, 0.2 and 0.6, is 1.0.) The wide divergence shown in these two studies is corroborated by a 1982 study for the Joint Economic Committee of Congress that got two big models, DRI and Chase to estimate the effects on nominal GNP of four disinflationary monetary policies. The means of the coefficients of divergence of the DRI and Chase models for these four policies are:

<table>
<thead>
<tr>
<th>Year</th>
<th>Coefficient</th>
</tr>
</thead>
<tbody>
<tr>
<td>1982</td>
<td>94%</td>
</tr>
<tr>
<td>1983</td>
<td>58%</td>
</tr>
<tr>
<td>1984</td>
<td>40%</td>
</tr>
<tr>
<td>1985</td>
<td>46%</td>
</tr>
</tbody>
</table>

These results raise two questions for methodology. The question for normative methodology is whether simulations of monetary (and presumably other) policies deserve to be taken seriously, and the answer to this question is no. The question for positive methodology then is why they are taken seriously.

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Program versus Project Aid: A Role Reversal*
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In the golden age of development aid men and women of good will tended to favor program aid, while the hard nosed, hard-boiled conservatives favored project aid. The latter was supposed to be subject to the discipline of project appraisal and could be controlled by a knowledgeable donor, whereas the former permitted the recipient to spend the money freely. Anyone who has had to collect money for a university or research institute knows the dilemma, and knows how to get out of it. You collect the money for a building or a chair named after the donor that you would have wanted to have anyway (the project), and you use the money for what you consider priority causes: improving the kitchen, or the garden, or the salaries. This is called fungibility.

The advocates of program aid were not against conditionality attached to the aid. They thought that macroeconomic policies are more important than project design. Even the best project can be ruined by the wrong exchange rate, or the wrong demand management, or the wrong prices for its products. Indeed, many thought that it is the virtue of concessionary loans that you could buy the right policies with the concessionary component. Those who argued that efficient project execution required the discipline of full commercial interest rates were wrong (it was thought). Concessionary loans or grants were more efficient, because they could be accompanied by conditions of good management that went beyond those necessary for the repayment of the loan and covered good policies for the whole economy. It was these that were important for the success of the projects.

In the present dross age the roles seem to have been reversed. Conservatives, led by U.S. ex-Treasury Secretary James Baker, are for program aid, while the people on the left are for project aid. Why this role reversal? I can think of four reasons.

First, in the current debt crisis program aid is often aid to the banks in the U.S.A. and other creditor countries, not aid for the poor or for development. The alternative may be default or debt forgiveness. It is then understandable that people with sympathy for bank managers and bank shareholders (and perhaps depositors) are happy to see money intended for development aid spent on repaying bank loans. A drawback of conditionality attached to loans that are used for the repayment of debt is that the reward for obedient economic performance goes to the creditors, and this reduces the incentive of the debtor to conform to the conditions.

Second, it may not have been immediately obvious that not only projects, but also policies are fungible, or rather substitutable. Complete fungibility would mean that the policy that is imposed as a condition would have been carried out anyway, whereas in this case ways are found to get round it. The donor makes the decision, and the recipient, while following the advice, inflates to restore the initial real exchange rate. Or open and concealed taxes and subsidies can undo policies with respect to tariffs or credit. A conditionality that embraces all possible measures would be impossible to implement or monitor. Awareness that not only ‘top priority’ pet projects, but also ‘top priority’ pet policies can permit escape mechanisms has come only slowly.

Third, the conditions imposed by lenders reflect often prematurely crystallized, but flawed orthodoxies. The current fashion of state minimalism and pricism, with its prime emphasis on getting prices right in the free play of market forces, cannot be said to be based on sound scientific principles. Dogmas have hardened into ideological stances, and the plurality of donors’ views and conditions has been greatly weakened.

In the golden age, borrowers, if they did not like the conditions, could turn to other lenders, or do with less aid. With high growth rates, booming world trade and a working financial system, this was not a great loss. Today, by contrast, recipients are in such a desperate situation that they must bow to the donors’ views against their own judgment, and a single donor, the Bank and Fund, controls all other donors so that aid from all sources is cut off if conditions are not accepted.

Fourth, confidence in the ability of recipient countries to carry out the policies incorporated in the conditions has declined. This is partly because, with the breakdown of the Keynesian consensus, the political obstacles and psychological inhibitions to the new prescriptions (e.g. dismantling the state) are greater, and partly because the training and skills are seen to be absent in many developing countries. To be critical of the narrow ideology reflected in some donor conditionality does not imply approval of the policies carried out in the recipient countries. Some of the early faith in program aid was based on excessive confidence in the capability of these countries to carry out good policies.

* I am grateful to Hans Singer for a helpful discussion.