An alien parachutes into economic research on low-income populations

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This article is about how applied microeconomics and applied econometrics have changed over the last 20 to 40 years, as reflected in their conceptualization and evaluation of initiatives to improve the welfare of lower income populations. I define such initiatives broadly to include research on: welfare provisions, poverty alleviation (including family income support), job training, education for poorer families, housing, and community development.

I am neither an economist nor a historian of science. I have not been trained in economic theory or in any of the econometric methods microeconomists use most. And while professional historians can construct a coherent narrative and remain sensitive to the many nonrealized futures that might have emerged, I cannot do this and will doubtless construct a history whose causal links seem more inevitable than they really were. To add to the embarrassment, I have not conducted systematic content analyses of past writings in applied microeconomics or applied econometrics. Reported here are merely personal impressions.

So why read on? Voltaire was fond of criticizing conditions in France. One way he did this was by inventing creatures from outer space who visited France and reported on what they saw there, as in *Zadig* or *Micromegas*. Underlying this version of the comparative method is the assumption that outsiders are particularly able to identify the big assumptions that insiders take for granted. My conceit is to pose as an outsider parachuting into certain parts of microeconomics and econometrics in order to identify and comment on big assumptions and how they have changed.

Substantive changes

In this first section, I discuss what we might call *substantive* issues in poverty and welfare research.

The shift away from labor economics to a broader set of substantive topics

The substantive concerns of applied microeconomists have become broader and less dependent on labor economics and studies of family income support, welfare rolls, job training, and other aspects of the low-end labor market. Up to about a decade ago, issues like the above were dominant, and labor economics was king. More recently, studies of welfare and family income support have declined in frequency, perhaps because of the implementation of Temporary Assistance for Needy Families, expansion in the Earned Income Tax Credit, and decreases in welfare rolls. Studies of the low-end labor market have also become less numerous, especially evaluations of the effectiveness of job training strategies—a major enterprise until about 1995. Individual job training evaluations are now rarer, perhaps in part because they generally failed to find large, sustained effects on subsequent employment or family life. Recent work on job training has gravitated towards synthesizing the policy and methodological lessons learned from past studies in job training.[1]

National policy agendas have now changed, and topics such as education, criminal behavior, and housing are receiving considerably more attention from economists. An institutional change has also accompanied this shift in fields of interest. Many of the economists doing this work are to be found in Schools of Public Policy, Schools of Social Work, and Schools of Education, rather than in Departments of Economics. Microeconomists long ago made the shift into Schools of Business, where they work on issues of labor, organizational, financial, and institutional economics.

The shift to other countries

Poverty research topics have also shifted away from research in the United States to research abroad. Some of the international welfare research has taken place in Western Europe or the Organization for Economic Cooperation and Development (OECD) countries. Much of this work is survey-based and descriptive-comparative, like the Luxemburg Income Study, but some is also experimental.[2] However, the most fundamental changes are taking place in developing countries. The last two decades have witnessed an explosion in the vigor and rigor of developmental economics, as well as in the willingness of developmental economists to structure their work around microeconomic theories and methods. This has weakened the formerly dominant tradition based on designing and evaluating large capital improvement projects. More recent are smaller, locally grounded initiatives often evaluated using randomized control trials. More and more developmental economists are using theories and methods I associate more with microeconomics than macroeconomics. Playing a role in this internationalization of research is probably the internationalization of graduate education in economics over the last 20 years. Many of the economists leading the charge are not American-born, though most were trained in the United States.

The findings of this international work have not gone unnoticed in American welfare policy. One example is the substantive interest in providing poorer families with microcredit, following the model of the Grameen Bank. Another is using conditional cash transfers to shape the education-relevant behavior of both students and their caregivers. Indeed, the influence of Progresa/Opportunidades from Mexico on the New York conditional cash transfer program is publicly acknowledged.[3] We should hope that North American policymakers continue to scan foreign research for leads to improving American poverty policy, realizing that many specifics will need to be adapted to fit our unique national circumstances.

The multidisciplinary shift

A third change is worth noting—the growing use of study outcomes and substantive explanatory processes that are not traditionally considered economic. Some of this change comes from within economics, particularly from behavioral economics. As a subfield, it mirrors two concerns that other disciplines have long voiced about neoclassical theory: the assumption that individuals have access to, and fully use, all the information potentially pertinent to a decision; and the assumption that profit-maximization is the prime motivator of individual human behavior. Often inspired by cognitive psychology, the many demonstration studies in behavioral economics reveal individuals who consistently fail to respect formal logic when making decisions and who instead rely on many different cognitive shortcuts.

Behavioral analysis also reveals that individuals and groups do not act only, or sometimes even primarily, to maximize their own financial benefit. For example, nearly half of the families offered a housing voucher in the Moving to Opportunity for Fair Housing demonstration program (MTO) did not obtain a lease.[4] While some of this was attributable to delays in finding suitable housing, some was also because families realized that they did not want to leave their social ties and face the possible social isolation of more affluent settings (which often have limited access to affordable transportation).

I do not want to exaggerate the extent to which concepts from other disciplines have already seeped into microeconomics. To most social scientists, economists still speak a strange language, while most microeconomists believe that their training arms them with flexible theoretical and methodological tools that can quickly get them to the heart of any research matter. Bringing knowledge from other fields to bear on economic research requires a great deal of cooperation between individuals from disparate disciplines.

Methodological changes for causal analysis

Methodology is a broad construct and in the space available it must be dealt with extremely selectively. I concentrate here on causal issues. The biggest change over 40 years in econometrics applied to poverty research has been the movement away from statistical control to design control. A large part of this is the movement away from multivariate modeling towards methods that test the impact of one or a few potential causes while making assumptions that are as few and as transparent as possible. In this movement, the role of the randomized clinical trial has been fundamental, and over the last 40 years many more studies have been conducted using this method.[5] Random assignment is also the foundation of Rubin's influential causal model cited by many microeconomists.[6]

Over the shorter period of the last 20 years or so, the main changes have been within the experimental agenda. One change is towards marginal improvements in the theory and practice of randomized experiments. Another is towards exploring nonexperimental design and analysis options that result in minimal selection bias.

The randomized control trial

Randomized control trials crept increasingly into welfare studies during the 1970s and early 1980s and then became quite common.[7] Partly it was because of their impeccable statistical pedigree, and their high reputation in the eyes of many policy officials already acquainted with them through agriculture and medicine. The acceptance was likely advanced by work done by the social policy research organization MDRC, and to that organization's effective strategy for disseminating what it learned about poverty programs and their evaluation. The surge of randomization was also partly attributable to increasing evidence that other nonexperimental strategies were often ineffective in controlling for selection. Simple regression methods came under attack for reasons of hidden bias. Instrumental variables were faulted for their failure to show that the exclusion restriction held in real research examples. And Heckman-type selection models consistently failed to reproduce the results of randomized experiments. As old certainties were undermined, randomized experiments seemed to be one of the few things worth doing in order to determine causality.

Of course, critics of the flight to random assignment emerged, particularly among old guard econometricians and microeconomists with decades of experience using the very methods now being denigrated. They believed their own preferred methods to be more generalizable, since they were not limited to manipulable causal agents and to settings and persons willing to volunteer for random assignment. Their methods could often be used with extant datasets, particularly longitudinal ones, seeming to make them less expensive than launching a randomized trial. They also saw their own model-based methods as producing results that could be validly extrapolated to populations, settings, and times different from those actually studied.[8]

Advocates of randomization took some of these objections seriously. They too were concerned that many individuals do not take up treatments offered them, leading Angrist, Imbens, and Rubin to prove that random assignment can be

used as an instrumental variable for estimating the effects of actually receiving a treatment as opposed to being offered it.[9]

Other issues taken seriously by randomization advocates included differential attrition, the proper computation of statistical power, and the need to be specific about the populations to which results could be generalized. Some of these practical problems in implementing or analyzing experimental data were completely or partially solved. Other problems have been acknowledged yet remain unsolved. The most serious of these is the inability to explain why or how effects occurred using conventional statistical methods—except where random assignment is the instrumental variable for examining the effects of a single mediator.

One continuing criticism of randomized experiments is the lack of external validity; in practice, many randomized trials are limited to a single historical time, one set of human populations, one set of social settings, one way of implementing the treatment, and often one way of measuring the outcome. Moreover, for ethical reasons, most experiments are limited to those who volunteer to be in the study and who know of all the treatments to which they could have been assigned. An obvious response is to note that randomized experiments aim to maximize internal validity and, if properly implemented, they do this well. They were not designed to foster external validity, and so why blame them for not doing what they are not meant to do? However, this merely admits the method's limited range. It acknowledges that, by itself, random assignment is irrelevant to selecting the cause and effect variables worth study, to conducting correct statistical tests, and to incorporating a useful set of persons, settings, and times into the study sampling design. It even acknowledges that certain biases might be repeated across a broad array of experiments—for example, all are limited to volunteers.

Another criticism of random assignment studies has to do with causality; it is nearly impossible to identify all the contingencies on which any particular cause-and-effect relationship depends. Thus, it is extremely likely that in other circumstances, similar manipulations of what appears to be the same cause will lead to a different effect. While we need practical new ways of thinking about causal contingency at the research design stage, this only highlights two other deeper problems. One is the mismatch between the manipulability theory of causation that underlies random assignment, and the more explanatory theory of causation to which most philosophers of science and many practicing natural and social scientists adhere. Randomized experiments serve to describe causal connections and not to provide an explanation of them. A very large experiment is still a single case study of a quite particular treatment, under a restricted set of all the possible conditions that might affect the size and direction of its effects.

Although a single causal mediator can be tested using random assignment as the instrumental variable, testing more elaborate causal models is only possible by relaxing current standards of evidence. While such relaxation would be unacceptable to most microeconomists, Manski has questioned the value of unbiased causal findings if achieving them requires selecting populations of persons, settings, and times that have little or no relevance to policy actors.[10] In this view, policy research should first determine who the policy audiences are, and then learn what each needs to know about a given population in a given setting before choosing methods that depend on the kind of questions asked. If they happen to be causal, questions should be addressed using whatever blend of experimental design and statistical manipulation permits capturing all the cause and effect constructs, as well as all the populations, settings, and times that surfaced when formulating the policy problem. Implicitly to be avoided is deliberately limiting oneself to questions that are causal, or to only using experiments to test cause, or to tailoring the experiment's sampling and measurement design solely to accommodate implementing the randomized experiment, come what may. Then, the desire to do an experiment takes precedence over meeting the necessarily contextually embedded causal knowledge needs of specific policy audiences. The cart precedes the horse.

I suspect that very few young microeconomists would be open to this criticism, so strong is the current ethos to privilege internal over external validity. Logically, it is correct that one should be sure of a causal connection before seeking to generalize it. But in the real world, trade-offs are common and one might benefit from asking: If a particular randomized experiment marginally increases certainty about cause, is this worth the more limited generalization that often results? Of course, no perfect answer to this question is possible, and enlightenment comes more from examining specific instances of the trade-off than from abstract ruminations about it. Nonetheless, I suspect that forging any kind of a compromise with younger microeconomists will not be easy. Without randomization, they can rarely be sure of no or minimal causal bias; and Manski's bounds require more assumptions and create less "certainty" than many applied microeconomists are now willing to accept. Still, it is worth having a debate about the conditions under which internal validity is more important than external validity in concrete research applications. Before this can happen, at least two preconditions would be helpful; clarification about the many meanings of external validity, and the willingness of applied microeconomists and econometricians to consider a quite broad range of alternatives to the randomized control trial.[11] I report on some of these alternatives below.

Nonexperimental alternatives

Most microeconomists interested in poverty came to believe early in the virtues of various forms of causal modeling, that they were more flexible, comprehensive, and theory-linked than a tool like random assignment. Also, they were attentive to their econometrician colleagues who, from the 1970s to the later 1990s, pursued a very visible agenda to discover ways of justifying causal inferences from observational data that, to varying degrees, required substantive theoretical assumptions. One direction this agenda took was towards the

use of instrumental variables, given convincing proof that strong instrumental variables can reduce all selection bias on observed and unobserved variables. This led to countless and often unfruitful debates about the dependability of individual causal conclusions, especially about whether the restriction assumption was met. Other econometricians took to linking instrumental variables to various other assumptions designed to deal with selection bias. The foremost theorist of such selection models was James Heckman who, between about 1980 and 2000, created many such models in hopes of discovering a general theory of how to achieve unbiased causal inference from observational studies. However, this agenda failed to fulfill its promise, and the considerable excitement about it had abated by the turn of the century, when many microeconomists came to feel the need to explore a different and more modest causal agenda. First, they advocated doing more randomized experiments. When these could not be carried out, they next turned to the causal techniques discussed below that are of limited scope when compared to the earlier econometric agenda. Nonetheless, they were gradually able to develop a toolbox with many different tools for causal design and analysis, each limited in its range of application but collectively covering many situations where causal knowledge is needed.

One causal tool that emerged was the *natural experiment*. These are like randomized experiments in that the potential cause is considered to be exogenous to the processes otherwise generating variation in the outcome. Taking advantage of exogenous variation had long been a staple of interrupted time-series studies such as those on the effects of natural disasters or of macroeconomic shocks. However, natural experiments are intrinsically opportunistic and cannot be used often enough to function as a major knowledge-building tool about human behavior, especially when one needs to know about the effects of human-controlled interventions. This is because first impressions of exogeneity can be deceiving; some interventions occur in order to respond to prior performance. When causal agents are embedded in ongoing social or economic systems, all claims to be studying a natural experiment require close scrutiny.

The next observational study discovery was of the *regression-discontinuity design (RDD)*. As I have described elsewhere, this was discovered in the 1970s but rarely used in economics until the 1990s.[12] It is applicable when an intervention is offered to all those on one side of a quantitative cutoff score that has been used to determine treatment assignment. This design came to be seen as an unbiased causal tool for use in the many contexts where allocation to a scarce resource is based on some quantified need, merit score, birthdate, or on a "first-come, first-served" basis. By now, RDD is in every younger microeconomist's toolbox. However, it too is limited in scope, as it requires treatment allocation to occur at a single point on an assignment variable where the causal impact is estimated. While some interventions are allocated this way now, and more could be in the future, it is not yet the norm in welfare policy.

Much more flexible is *matching*, trying to use a study's sampling and measurement design to mimic the initial group comparability that random assignment achieves. With random assignment, the comparability is on all observed and unobserved variables, whereas with matching it is only on observed covariates. The matching agenda in applied econometrics has been extensive, including comparative work on different ways of creating matches. But the most visible effort has been with propensity scores, primarily developed by the statistician Donald Rubin.[13] Propensity scores entail constructing multi-item composites that predict selection into treatment. The point is to balance the propensity scores across the nonequivalent populations being examined and then adjust the study outcomes for the influence of the scores. Balance entails that all scores in the treatment and comparison group fall within the same range so that they totally overlap and create an area of common support. However, since propensity scores do not explicitly handle hidden variable bias, they cannot guarantee removing all selection bias and have received a mixed reception among applied econometricians and microeconomists. One reason for concern comes from the many within-study comparisons in job training that compare the results from a randomized trial to those from a statistically adjusted quasi-experiment with the same treatment group, but with a comparison group that was nonequivalent prior to matching. Propensity scores did not reproduce the experimental results in any of these comparisons.[14]

However, in empirical comparisons outside of job training, propensity score studies have recreated experimental results in several specific contexts.[15] A strategy for better propensity score practice is emerging, but is not yet definitive. This strategy involves first using theory, literature reviews, or pilot studies to develop several different selection models that seek to mimic the unknown true selection process. The key variables in each model are then measured reliably prior to treatment. Measures from other domains are also included among the covariates in order to increase the chances of obtaining measures that correlate with any individually unmeasured variables that might account for some part of the selection process that is correlated with the outcome. Only after covariates have been collected that seek to index *both* the true selection model *and* other forces with no known link to selection should the propensity score be computed. As the job training literature shows, propensity scores do not work universally. However, when carefully done in large sample work, they have sometimes been shown to nearly reproduce experimental causal estimates, and we now also have some idea of the conditions under which they are most likely to do so. So propensity scores rightly deserve to be in many microeconomist's causal toolbox, albeit for use with great care. The current dirty secret, though, is that propensity scores have rarely done better than careful simple regression analysis with the same covariates. The empirical case for propensity scores is therefore limited and mostly derives from the fact that they do not depend on extrapolation. Causal influence is instead limited to where estimated propensity scores overlap across the treatment and comparison groups.

Another causal tool is *interrupted time series*. Economists have used this almost exclusively with archival data, but it can also be used with original data collection at least in single case studies, as has been done in studies of special education.[16] Interrupted time series is the obvious methodology for evaluating changes in laws and regulations, and was recently used to evaluate the impact of No Child Left Behind at both the state and national levels and also at the national level through comparing all public school students with students in two comparison series (all Catholic private schools and all non-Catholic private schools).[17] In job training, employment and wage data have been used over many quarters both prior and subsequent to an intervention.[18]

With a single interrupted time series design, causal interpretation is only possible when the pre-intervention functional form is very clear, the effect is very large and occurs immediately after the intervention, and the time intervals are quite close to each other. These are daunting requirements. In most interrupted time series work, one or more comparison series are necessary in order to test whether intercepts and slopes change differentially from before to after an intervention. Such comparison series will not always be forthcoming, especially with changes at the national level. Comparison series can also be constructed from non-equivalent dependent variables rather than non-equivalent comparison populations. Ross et al. showed this in evaluating the British Breathalyzer data where traffic deaths and serious injuries were assessed during the hours when pubs were open and immediately after, and then compared to deaths and injuries during the hours when pubs were closed and thus drinking and driving were less prevalent.[19]

Although it would be an overstatement to claim that comparative interrupted time series studies are common in applied microeconomics (except in some areas of finance), I suspect that it is only a matter of time before they experience a renaissance like that of RDD. At present, interrupted time series is a very minor tool in the applied microeconomist's toolbox, but its potential for future utilization is high, albeit in a restricted set of circumstances where new laws or regulations are passed, where relevant administrative data exist (as is increasingly the case), where a no-treatment comparison series is available, and where no immediate policy answer about effectiveness is required.

Conclusions

In my view, we have witnessed over the last 20 to 40 years a shift in the substantive concerns of applied microeconomists interested in low-income populations. The dominance of labor economics and issues of unemployment, job training, welfare, and family income support has now ended. There is growing research interest in education, housing, criminal behavior, community and neighborhood development, and in policy lessons from other rigorously-studied countries. Also beginning to enter the field are cognitive, motivational, and social-network insights from qualitative sociologists who study poorer families at the ground level, plus an interest in individual and collective outcomes and explanatory concepts that most often come from sociology and individual and developmental psychology. Applied microeconomics seems broader today than 20 or 40 years ago, and slightly more integrated into the other social sciences.

The shift in method preferences has been considerable. Forty years ago, the use of instrumental variable analyses, complex selection modeling, and substantive modeling using simple regression were rampant. Now, they are noticeably less dominant, each subject to both theoretical and empirical attack. They have been partly replaced by the growing use of randomized control trials, and by research into improving the design, statistical power, and statistical analysis of those experiments. Also apparent is growth in observational study methods based on approximating the structure and logic of randomized experiments. Especially notable here is growth in RDD, propensity scores, and other matching methods, but we should also remember natural experiments and interrupted time series methods. The modern applied microeconomist now has a better provisioned causal tool chest than ever before. Each of the newer tools is limited in when and where it applies, but collectively the range is quite large. Microeconomists seem to have given up on developing a general theory of selection control. It is as though the field went from searching for one big arrow to fill a small but elegant quiver to requiring many causal arrows that collectively require a larger and certainly more ungainly quiver.

I suspect there are still some issues with which applied microeconomists interested in poverty and welfare need to struggle. One is the conflict between the manipulability theory of causation that underlies the field's thinking about causation, and the contrary belief that this particular theory is not as useful or comprehensive as other theories of cause. Science has always put a higher premium on the identification of novel causal mechanisms with broad applicability, compared to the identification of a specific link between a particular treatment and a particular outcome. The second challenge concerns the value of placing so much emphasis on achieving the last bit of uncertainty reduction about internal validity if this compromises external validity. In pure logic, internal validity is a necessary condition for external validity. However, practice in applied microeconomics is more concerned with satisfying the knowledge needs of specific consumers, especially policymakers, who tend to be less interested in the compulsive elimination of all uncertainty about cause, and more interested in learning whether a given causal result applies to the specific groups, settings, and times for which they are responsible. Applied microeconomics currently seems willing to tolerate many losses in external validity. But is this the right trade-off? In the case of experiments, this trade-off may mean that causal conclusions are limited to volunteers who are aware of the different treatments. In some observational studies, this trade-off favors gaining causal knowledge about discrete entities rather than about combinations of factors that more strongly affect human behavior, such as the combination of housing, family,

neighborhood, and school factors in child and youth studies. The current causal strategy in applied microeconomics seems like a way to identify mostly small or null effects, and like an invitation to study convenient causal agents instead of serving as a test-bed for truly bold thoughts.■

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