

## Change of directors: Interview with Eugene Smolensky

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Having served his rotational term as director of the Institute for Research on Poverty, July 1980-July 1983, Eugene Smolensky is stepping down, to be replaced by Sheldon Danziger. Danziger, a professor of social work at the University of Wisconsin, received his Ph.D. in economics from MIT in 1976. He has been on the staff of the Institute since 1974. During this decade his research has focused on the measurement of inequality and poverty and the evaluation of how government transfer programs affect poverty, inequality, and economic behavior.

Smolensky will return to teaching and to the completion of a monograph that analyzes the changes in the distribution of economic well-being that have taken place in the United States over the past two decades. He will retain his staff appointment at the Institute.

Smolensky's tenure as Institute director coincided with fiscal retrenchment for the Institute and the nation, as well as changing attitudes toward poverty programs and poverty research. He answers here a few questions bearing on these changes.

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*In a discussion of the effects of social security on savings, Henry Aaron states: "Using the best that economic theory and statistical techniques have to offer, they [economists] have produced a series of studies that can be selectively cited by the true believers of conflicting hunches or by people with political agendas that they seek to advance."<sup>1</sup> That being the case, is there any reason to think that economic theories and studies—such as those produced at the Institute—are of any practical value to policymakers?*

I think Aaron is a very sensitive viewer of the policy-analysis scene, but that quote misses a key point. There is a difference between what policymakers need to know, even about numbers, and what the discipline needs to know. What's important about the debate that Aaron is reporting on is that it is possible for competent economists to come up with very different empirical results. That it is *possible* is

a very important matter for the discipline, because the discipline wants to be able to narrow down what is possible. Irwin Friend and Larry Klein taught me a long time ago, when I thought my career would be in making macro models, that a lot of professional economics consists of restricting the domain of expected theoretical and empirical results to the range expected by practical men from business and politics.

As a practical matter, I think that everybody—even the people who are getting extreme results—would agree on what is important for policymakers to understand, which is that social security probably does affect savings and does affect the allocation of work over time, but not in an important way; that is, not in a way that's important for making policy. What's important about those big numbers is that economics as a discipline ought to be able to do better, but I don't think that it's important for policy. So, in short, I think the Poverty Institute walks a tightrope: it has to advance the disciplines and has to be relevant for policy. Taking on that dual problem is hard, but certainly doesn't lack practical value.

*What is the present role of IRP in the current economic/political world?*

*What should its role be?*

That picks up from the last question. The idea is to find ways to pose policy questions so as to be of interest to researchers whose future rests on their capacity to advance the disciplines of which they are a part without sacrificing the policy-relevance of their answers. That gets harder and harder to do, I think. But the important problem for IRP is to avoid a situation where all the excitement of the problems they are interested in is initiated by policy changes that emanate from Washington or the state governments. Trying to do these two things will lose us, really, intellectual vitality, if some of the momentum is not coming from questions arising in the disciplines. The danger now is that the disciplines are turning inward, worrying about methodological issues and about their own future directions to such a degree that it is getting more difficult to attract the attention of smart young people to opportunities to carry out this dual function of both advancing disciplines and advancing what we know that is relevant for poverty.

*Is this situation different from the way things were in the past?*

It's more difficult than it was when the Institute started, for two reasons. One is the whole history of conceptual issues in poverty policy—and income security policy more generally. There were just more opportunities to advance the field because so little had been done. All the conceptual work, of the simplest kind—definitions—hadn't been done yet. Now all of that is done. It's a mature field, so advancing it means first you have to learn a lot about what's been done in the past—which you didn't have to do fifteen years ago—and then you have to see opportunities to make progress, built on a lot of prior work, which wasn't an issue before.

So it's harder because the field has developed. It has a history and a literature. And the easy questions have all been addressed. It's also harder because, as I said before, I think the disciplines, particularly economics, are somewhat in disarray. Economics has lost some of its self-confidence, and there's a lot of looking inward, looking into the discipline—its tools and objectives—trying to decide once again what it is we really believe regarding these tools and methodologies, and that absorbs a lot of the profession. More than it did when we thought Keynesianism worked and the fact that budget constraints were kinked was a refinement, not central to the labor supply problem, to take some examples. Now we have a fundamental concern about macro policy and about the shortcuts we've been taking in microstatistical methodology.

Young people, I think want—are very much drawn to—the priorities that are being set by the discipline. And those priorities are not congruent at the moment with advancing policy. They were much more congruent fifteen or twenty years ago. It's a harder problem now to get young people committed. One way you can get them interested is through policy initiatives. Everybody has a little bit of Potomac fever, but unless the policy initiatives provoke questions that will lead to an advancement of the discipline, doing that kind of research is going to keep those people from getting tenure in the major departments, and that means that if they're very good, they won't take those problems on.

Policy-relevant research is now publishable in the major journals only if it advances the discipline. And, as I said, it's much harder to advance the discipline than it was fifteen years ago because the conceptual work has been done. People who do state-of-the-art empirical work to get a number we need to know can probably get it published in a journal, even a respectable journal, but if all you have is a number that people wanted to know, that probably won't get you into one of the major journals. For the major general-purpose journals, you have to advance theory or econometric methodology—in economics—and theory and methodology in the other fields.

Are you going to ask me now what the big problem is that the next director of the Institute faces?

Yes.

It will be just that: getting very good people to continue doing policy-relevant research. In addition to the history of the field and the histories of the disciplines making it more difficult to meet this dual requirement of the research, the director has much less flexibility in allocating funds. And one of the key things we've lost in the reduction of funding on the average and increased inflexibility all across the board is that we don't have a way of finding smart young researchers and then saying, "Come to the Institute, think hard about our problems, and see what you come up with." We've lost this generalized funding for postdoctoral researchers, which has been a large part of the intellectual vigor of the place. It's essential to restore that, but I don't know how to do it. I've approached several foundations without success so far. And the government's not going to do it. Even five years ago a guy could be out of graduate school a year or two, you'd see that he had some research potential, we'd lock him in a room, and something good happened. Now we can't find the funds to do that. ■

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<sup>1</sup>*Economic Effects of Social Security* (Washington, D.C.: Brookings Institution, 1982), p. 51.

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