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**A Space Odyssey: The Future is Not What It Used to Be—
A Babyboomer's Travel Guide and Challenge to Young Explorers**

Andrew M. Isserman^{*}

Andy was a dear friend and colleague to many. His curiosity often challenged the way prior work defined an issue or approached a problem and to this effect, he frequently solicited opinions about the facets of his teaching and research from colleagues. Andy's attention to detail and his pursuit of clearly expressing ideas often was the impetus that drove him to seek input from others. A draft report might undergo a dozen or more rewrites to strengthen the articulation and fullness of his ideas—and this Fellow's Address is an example. Andy sought feedback from many while creating this document, sharing his work multiple times with us, and others, throughout its preparation. While this is certainly not the final version Andy would have submitted to *The Review of Regional Studies*, the following is Andy's work, with only minor edits and the addition of citations. Andy will be missed but not forgotten on our own space odysseys by following this advice that he offers.

Sarah A. Low, Economic Research Service, U.S. Dept. of Agriculture
Mallory L. Rahe, Oregon State University
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In his Fellow's Address, David Barkley (2008) made the case for additional case-study research by regional scientists. I agree with his arguments and will try today to heed his call by presenting a case study and show you David Barkley was right. For this lofty purpose I have chosen to do a case study of me. Those of you who heard me give SRSA's longest presidential address (Isserman, 1993) might be relieved. If the history, status, and future of regional science took me an hour, as it did, the history, status, and future of me ought to be pretty quick. (At least that is what regional scientists used to fixed-proportion production functions might think.)

First, let me defend this preposterous idea of doing a case study of me by citing some precedents. I sat spellbound as William Shaffer told us the story of his family and his rural Georgia village, revealing the roots that drive his very Southern regional science (Schaffer, 1993). Charlie Leven helped me to understand myself when he gave his 1995 Isard Award address and described himself not as an economist, regional scientist, or policy analyst, but as a measurer. Carol Taylor West (1997) said much when she spoke of Glass Ceilings and Black Boxes, a wonderful title for the adventures of a female econometric modeler. Brian Berry's stories of the early space cadets and the revolution in quantitative geography added to my appreciation of the evolution of regional science (Berry, 1995). I have appreciated Jim Hite's stories, too, his Southern histories of our intellectual pursuits, always informed by his love of both intellectual history and the farm (Hite, 1985).

^{*} When this piece was written and presented, Andy Isserman was Professor of Regional Economics and Public Policy Professor of Urban and Regional Planning and Institute for Government and Public Affairs at the University of Illinois. He left us all suddenly on November 4, 2010. (editors)

I have also learned that the advantages of a pedagogical path rarely taken. We read research in cross-section, that is, different scholars on the same topic. We rarely read in time series, a single scholar on different topics. When I tried the longitudinal approach as an assignment for doctoral students in regional economics, I learned that we scholars are similar to artists. Many of us have our own research style, our way of asking questions and doing things, our own set of skills and tricks. Like at an art museum, we can distinguish an early Giaratanni from an early Gerking, two of our colleagues my students studied to learn how the cohort ahead of them successfully made the transition from doctoral candidate to tenured associate professor.

One of the many uses of case studies in social science research is theory development. Case studies can shape a field and launch 1,000 dissertations. Consider Michael Porter's case studies in *The Competitive Advantage of Nations* (1990) and Robert Putnam's in *Making Democracy Work* (1993). Dealing explicitly with regional development, they gave us clusters and social capital, regional competitiveness and civic engagement, important foci of subsequent regional science research. So the hunch here is that perhaps we can take a step toward developing and testing theories about space odysseys by doing case studies of space explorers. I take that term from William Miernyk's Fellow Address in 1982, in which he asked, regarding the transition from conventional economics to regional science, "Has it been a linear evolution or does the transition more nearly resemble the wanderings of Odysseus before he finally returned to Ithaca a decade after the end of the Trojan War?" And besides space odyssey is a great improvement over my own "Lost in Space?" Presidential Address (Isserman, 1993).

As a member of the vanguard of the baby-boom generation, I am a good choice for a case study. We are the bridge between the founders of regional science and SRSA and its next 50 years. Walter Isard, founder of regional science and the first SRSA Fellow, made me a journal editor within an hour of my dissertation defense. William Miernyk was the discussant for the first paper I presented at an economics meeting on my first trip to California. Bill Alonso, the first PhD in regional science and arguably the intellectual founder of urban economics, became a good friend who told me on a walk in the woods how Leontief led him to the concept of rent as the unifying principle for understanding urban structure. I knew personally many of the scholars who attended the organizing meeting of the Regional Science Association, but what is most important here is not the personal links but that my space odyssey began shortly after the space ship's launch. Hence, if my journey was influenced by the pioneers of a half century ago, my students will influence the Fellows 50 years from now when SRSA celebrates its 100th anniversary.

1. THE LAUNCH

As I look back now to the beginning of my space odyssey, the journal articles I wrote as a tenure-tracked professor, seem simple and tidy, nice childhood memories and mementos. I measured the accuracy of alternative population projection methods and drew conclusions about which ought to be used when. I examined the theory underlying the location quotient and other measures of the export base and made suggestions for better practices. I did some algebra to demonstrate the fiscal incentives to change local government behavior and organization that were embedded in the supposedly neutral general revenue sharing program. I gathered data at HUD regional offices and demonstrated that funding decisions under the community development block grant program resulted from the discretionary points awarded by the regional offices, despite the adoption of a complex funding formula intended to diminish that

discretionary role. Marilyn Brown and I also demonstrated that the use of city distress measures would lead to perverse and unintended outcomes if extension to suburbs was contemplated. I used the multivariate statistical techniques of my graduate courses to test theories on the effect of interjurisdictional fragmentation on local government expenditure and, with Shelby Gerking, on whether the export base model was a short-run or long-run model. I created a variant of shift-share analysis to analyze energy consumption by manufacturing and concluded that price-sensitive variations in consumption patterns dominated state industry mix, so our economic system had the capacity and incentives to react to energy price changes.

I was well equipped for this work by my graduate school education and by what I was learning from my graduate teaching assignments. Many of the problems I solved were brought to me by external sources. A former student in my regional analysis course recommended me to his employer, the Illinois Department of Transportation, which led to the contract that led to the population projection article, which led to an American Statistical Association Fellowship at the U.S. Bureau of the Census. The revenue sharing paper led to a HUD contract that produced the CDBG and suburban distress papers, which led to some changes in the program on Capitol Hill, but the revenue sharing paper itself resulted from a student's opportunity to work for a local task force studying the fiscal implications of a merger of Champaign and Urbana. My suggestions to another student funded to study the impacts of a new dam led to her master's thesis and my first try working with control groups and quasi-experimental research designs. Another student's master thesis, two joint articles, and a decade later led to a National Science Foundation grant to develop more sophisticated control-group methods. The energy paper led to an appointment at the Center for Advanced Computation, which led to a federal contract that led to the series of papers on economic impact analysis after solving some data suppression problems, which led even 30 years later to a 2006 paper on the latest method for overcoming data suppression.

Looking back, I see the wonderful synergies of teaching and research and a world which poses research problems and pays for their solution. Way back in the beginning, noting that time was short, I adopted a rule of thumb: Only undertake funded research that interests me, strengthens my courses, produces journal articles that help my students, and can be run through the university. Those filters have served me well.

2. NOT SO EASY PIECES

Now I work on the same topics, (1) regional demographic and economic analysis, (2) policy analysis, and (3) regional development, but the pieces do not seem so easy, straightforward, clever, or authoritative. Today's research challenges seem more difficult to solve. After outlining them, I'll turn to possible explanations for this turn toward complexity and difficulty. They range from life cycle theories to wicked problem arguments.

Population forecasting is a good example. I started by measuring accuracy and designing a trend extrapolation system adopted by Illinois for projecting the populations of 1,789 townships for use in air-quality maintenance planning mandated by the EPA. The project at the Census Bureau--with Paul Beaumont, David Plane, and Peter Rogerson working on their dissertations--produced a 35,000 equation economic-demographic model of the states. Our main concern was building good theory into the model for birth and migration equations as well as the economic and trade components. In 1993 I was still writing articles about getting demographic models right and measurement issues, but today I am trying to figure out how to get beyond issues of model building and consistency to help planners think of forecasts as tools to help

communities engage the future proactively. My emphasis has switched to coming up with compelling visions of the future with persuasive arguments about the steps to be taken, the plan that will create this more desired future, in short, convincing data-driven, imaginative story telling.

This odyssey in methods of regional analysis is perhaps easier to see in terms of economic impact analysis. Once I was absorbed by matters of model design, such as the treatment of consumption, the regional purchase coefficients, and the most accurate and efficient way to build non-survey and hybrid models. Now I am more concerned with the use of the model, the logic of the scenario being modeled and the interface of the scenario with the peculiar logic of the input-output model. Most of the interesting work in an economic impact study must be done outside the model with no help of the model. That is trickier and puts an emphasis on understanding how the world works. I think the challenges are illustrated well in a paper by Sarah Low and me (2009) on ethanol and the local economy. David Swenson's work also illustrates beautifully the dangers of getting very wrong how the world works and letting the model run without paying careful attention to the assumptions that are exogenous to the demand-driven model (e.g., Swenson and Eathington, 2006).

In policy analysis, too, answering the questions has become trickier. Supported by the NSF grant, Terry Rephann, Paul Beaumont, David Sorenson, and I pushed with the control-group methods and published two papers: one on highways (Rephann and Isserman, 1994) and the other on the Appalachian Regional Commission (Isserman and Rephann, 1995). The latter became something of a hit, winner of a national award from the American Planning Association, featured in ARC lobbying and federal Budget documents, and perhaps the only time that an article in the *Journal of the American Planning Association* was cited in the *Journal of Political Economy*.

But it was also hit with demands for more direct evidence, which will not be easy to provide. The Government Accountability Office (1996, p. 21) argued

The Isserman and Rephann study does not establish a strong statistical association between ARC's investments and growth. The study only examines differences in growth between ARC-aided and non-ARC-aided counties; it does not include any variables that capture ARC's investments at the county level. It does establish that ARC counties grew faster than similar non-ARC-aided counties, but does not correlate those differences in growth with ARC's expenditures. The study also provides evidence that some of the measured differences in growth may be associated with coal production.

Edward Glaeser and Joshua Gottlieb (2008, p. 199) argued,

[I]t is unlikely that the effects of a \$13 billion program spread over a giant swath of America over three decades can be accurately evaluated. Far too many things were affecting regional growth at the same time for a relatively modest government program to have had clear positive effects. Powerful economic forces are driving people to the Sunbelt and to coastal cities. Current spending on the ARC is no more than the cost of a few large Manhattan buildings. Could such a program really have changed the course of a region considerably larger than California?

I envision a dual response. Multivariate statistical work, such as in the 1995 study, but with now-available county data on government expenditures, might help explain differences in growth rates between Appalachian counties and their twins more directly as GAO would like to see. Case studies on whether and how the ARC funding leverages additional funds might reveal

the far larger iceberg underneath the visible \$13 billion. In both cases, the research design challenges are not so easy to solve.

The importance of such deeper inquiries is illustrated by research on areas of persistent poverty. Mark Partridge and Dan Rickman (2005) identified stimulating job growth and increasing human capital to reduce poverty in even persistently high poverty areas. Yet, Cynthia Duncan's (1999) case studies argue that control over local jobs and schools systems is a powerful instrument for maintaining the status quo in persistent poverty counties. My students this semester argued that Duncan's understanding of persistent rural poverty was more advanced and the two research approaches are complementary. Policy implementation by regression coefficient, without knowing how systems work locally, is not good enough. The same demand that GAO and Glaeser-Gottlieb asked of our ARC research are relevant. We must learn how the world works to be able to have faith in our statistical prescriptions.

The new case-study literature distinguishes between research designs that focus on "the average net effects of causes," such as most regression studies, and those that focus on the "causes of effects," such as case studies intended to figure out how and why something works (Ragin, 2006). Precisely that how and why knowledge is necessary to design good job stimulus and human capital programs in places with persistent poverty.

Case studies are also important for our research on place prosperity. Ed Feser, Drake Warren, and I identified ways in which prosperous rural counties differ from those that are not prosperous (2009). The strongest variables included jobs and education, but also more homogenous populations in terms of income and ancestry and more adherents to civically engaged religions. Again, how does the world work? What produces these statistical results? What goes on in persistently prosperous places, which leads to those outcomes? How and why are they persistently prosperous, and can other places adapt what they do?

Third generation case study research is not merely a matter of going somewhere, talking to people, and spinning a tale that matches one's biases. Rigorous case-study designs cannot only generate theories but also can assess, modify, and even verify them. Mallory Rahe and I are trying to formulate and test theories regarding civic engagement and local actions that contribute to local prosperity (Rahe, 2008). The new social science literature on case study methods, including pattern matching and selecting cases, is thoughtful and stimulating (Gerring, 2007; Mahoney and Goertz, 2004; Bennett and Elman, 2006).

Putting all these strands together, the common denominator is telling convincing stories about how the world works. From nice, precise, clean, bounded research at the launch at home, I am now in a peculiar world where knowledge is less certain, science at best is a partner of story, and there are many ways of learning and knowing. None of this is easy.

3. THEORIES OF THE ODYSSEY

As I look at this tale, this initial case study, I can offer three theories to explain the odyssey from neat and precise analysis to broader storytelling.

Researcher Life Cycle 1. Old folks tell stories. They have been doing that for centuries in town squares and taverns throughout the world. Tribal elders tell stories when they can no longer hunt. Likewise, when graduate school tools are obsolete, turn from science and statistics to softer methods and exploit your privileged position in society and keep on talking as long as someone will listen.

Research Life Cycle 2. Old folks are in demand. Society brings it tough, broad problems to older scholars, leaving the easier, narrower technical questions in the hands of newer scholars. This phenomenon might result from the life cycle of bureaucrats and policy makers. Older themselves, they approach older scholars who speak their language and speak slowly enough to be heard. The division and branch chiefs, younger themselves, are entrusted with easier questions, and they call on scholars of their own age.

Wicked Problem. Old folks understand. Introducing the concept of wicked problems, Rittel and Webber (1973) used words that arguably sum up well a century of regional and rural development research: “The search for scientific bases for confronting problems of social policy is bound to fail, because of the nature of these problems. They are ‘wicked’ problems, whereas science has developed to deal with ‘tame’ problems. Policy problems cannot be definitively described... Even worse, there are no ‘solutions’ in the sense of definitive and objective answers” (p. 153). A rural economist’s solution 80 years ago sounds contemporary in recommending good schools, good roads, electric power (today read broadband), and small hospitals and clinics with resident nurses and visiting specialists (Carter 1927), but that prescription illustrates how rural development answers are reasonable but often not scientific, definitive, or objective. Jim Hite, SRSA Fellow, and one of the most thoughtful of us all wrote insightfully a quarter century ago on the limits of social science and rural development.

Real Students Keep Learning. Some old folks never lose their curiosity. Restless minds remain active and use their intellectual freedom and time to explore new things. The new is always more exciting than what has been mastered already. Answers lead to new questions, and new questions often require different approaches and methods.

4. GUIDE FOR YOUNG SPACE TRAVELERS

- Keep traveling in intellectual space.
- Learn new ways of learning.
- Surround yourself with good students and colleagues.
- Teach so you can learn.
- The future won’t be what the future used to be.
- Read more case studies. They have the power to transform your worldview.
- Always behave like an assistant professor, reading, writing, eager to find your place in the vast scholarly universe.
- Study what you care about and what excites you, not what you think you ought to study, not what others think you ought to study, or what you think you must do to get funded or get published. Ignore this advice, and you may never leave the village of your scholarly birth and its parochial ways.
- SRSA is a wonderful port of call. It will embrace all modes of inquiry. Its subject-driven, problem-driven members will help you explore new territories.

5. WHAT WOULD THE FOUNDERS THINK

So I stand here a literally bridge from Bill Miernyk to Randy Jackson in one sense, from Walter Isard to Sarah Low in another, and from Bill Alonso to Mallory Rahe in a third. What would the trio of founding thinkers think of these ideas?

Walter Isard once told me when I was its editor, that the *International Regional Science Review* is a fine journal, but not regional science. That, I fear, is how the first SRSA Fellow would react to my odyssey. I do not seek a general theory or grand synthesis.

Bill Alonso (1971) would have been delighted. He loved to tell and read good stories that helped him understand how the world works and what scholars do. Forty years ago he wrote, "Holistic views are necessary to uncover relationships that are insufficiently recognized." Shortly before he died he e-mailed me:

I can tell you of two marvelous books (I think I told you on the phone that I have been on a marvelous run of terrific books). One, which you probably know (It has been out some years) is William Cronon's *Nature's Metropolis*, which deals with the rise of Chicago and how it shapes the spatial economy of the Midwest—possibly even the placid, happy place where you now reside. The other is John Barry's *Rising Tide*, which is a magnificent telling of the physical aspect of the 1927 Mississippi flood (which I had never heard of) and its long-term consequences for the Southern economy, the political dynamics of its competing elites, and the party allegiance of American blacks. It is a superb book, and I urge you to read it.

Let us keep in touch.

Bill.

Bill Miernyk, the third SRSA Fellow, would be pleased, too, I think. He concluded his Fellow Address (1982), arguing that "to fulfill the promise of its founders and early practitioners, regional scientists will have to break out of the constraints and methodological limits imposed by a predominantly economic focus."

Thus, I have snuck in the suggestions that (1) if you do more case studies of personal space odysseys, you'll find some intriguing patterns among regional scientists, and (2) mine might not be the case study of a hopelessly irrelevant outlier.

So, my friends, here is to your space odysseys. May you break out after you have broken in. May you travel on in this great collective space migration known as regional science. May you continue to share your tales of exploration with this wonderful, diverse, stimulating SRSA group.

I am very grateful for having been allowed to address you as an SRSA Fellow. Many, many thanks, and bon voyage in the second half century.

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