



**Refocusing Transportation Planning for  
the 21st Century**

Martin Wachs

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**The University of California  
Transportation Center  
University of California  
Berkeley, CA 94720**

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# **Refocusing Transportation Planning for the 21st Century**

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The University of California Transportation Center  
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# Refocusing Transportation Planning for the 21st Century

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As the director of a major university transportation research center, I am honored and pleased to have been included in this program in which we are exploring the contributions that research can make to the refocusing of transportation knowledge and planning practice. It is actually quite rare that line agencies or federal funding programs try to assess what research can provide and what it cannot do. But it is important to think strategically about research just as it is to think about planning and policy matters that hopefully are informed and improved by good research.

It is probably useful to conceive of the world of research as being analogous to a market like any other market for goods and services. We have suppliers who offer goods and services for sale, research studies can be thought of as a product like any other commodity or service; and those of us in universities, think tanks, and consulting firms want to sell our research services just like other purveyors of good things.

There are also potential consumers of research results. In our case, these customers include federal, state, and local agencies, and private-sector purchasers of research results. The sellers of research products have ideas, concepts, and perceptions of need in mind, and the buyers of research, whom we may prefer to think of as sponsors, have research questions in their minds. We therefore should understand research as the range of activities in which the two sets of interests come

together and are able to make a deal to actually get something done.

At conferences like this one, the vast majority of participants are customers in this research marketplace—the people who labor to solve complex problems in the world of policy and planning (e.g., public officials, consultants, or representatives of interest or advocacy groups). The problems and issues that planners and policy makers address typically include many dimensions—for example, technological, organizational, political, and fiscal. They involve conflicting objectives, such as providing more cost-effective transportation service at the same time as minimizing environmental damage, providing social and economic opportunities to disadvantaged populations, or promoting the political agendas of those who pay our bills.

Most people in these problem-solving roles make decisions and promote progress by relying on a very wide variety of resources in support of their complex assignments. They have to rely to a great extent on (a) technical analysis conducted by their staffs, (b) modeling and software that are produced by consultants, (c) federal regulations and rules, (d) critical comments and advice from citizens' committees and elected boards of directors, (e) skills and knowledge acquired from their own education, (f) information from the latest journals and technical reports (when time permits), and (g) instinct, experience, judgment, and political pressures to guide them in particular situa-

tions. Researchers have to think broadly, integratively, and synoptically so as to synthesize strands of insight from many fields, studies, and experiences so as to address their current questions.

People working in the policy world view research as finding answers to their most immediate and pressing questions in a short time. An example of such a question is "What could we do in this region to increase the modal share of public transit from 5 to 25 percent over the next 30 years?" This is really not a question that is amenable to a meaningful answer by a researcher, but it illustrates the type of questions that is often posed to researchers by agency directors and other clients of research. Many of the questions that were identified as potentially promising areas of research at the first refocusing conference in Washington, D.C., had this character. They were statements of the most pressing problems that face real-world decision makers—for example, that research is needed on ways to better engage the public in the planning process and how to more accurately measure the effects of transportation investments on economic development. These are broad, strategic questions of great importance in public policy, but are they really effectively addressed by research?

Most of us researchers, in universities, think tanks, or consulting firms, enter this research marketplace for a number of reasons. First, we have an interest in some specific body of knowledge. Second, we believe that there is a great deal more to learn in that area, and third, we want to sharpen and hone our knowledge in that area.

Whereas policy requires synthesis, we specialize in analysis. Researchers are good at breaking problems down into component parts and looking at those parts one at a time. We're not very good at building up complex answers by blending together a lot of component parts. Don't ask a researcher to design a policy, but ask a researcher to identify the implications of one dimension of a policy in one specific context.

Researchers specialize, and thus go deep and narrow, in travel demand forecasting, traffic operations, geographic information systems, maintenance and replacement of pavements, or transportation demand management. Research in these areas tend to make us not only smarter and more insightful but also narrower. If you ask a researcher what is the most pressing research need at the moment, he or she is likely to say that funding is desperately needed to study the distribution of error terms when the log normal form of the multinomial logit model is applied to non-home-based work-related trips!

We could say that the researcher comes to the research marketplace wanting to gain more and more knowledge about narrower and narrower subjects. But of course the logical consequence of trying to learn more and more about ever more narrowly defined prob-

lems is that, in the limit, we will know everything there is to know about nothing. The public policy maker or decision maker who hopes to make better decisions comes to the research marketplace wanting to gain a little more useful knowledge about more and more subjects. But of course the logical consequence of trying to learn a little bit about a growing number of issues is that, in the limit, we will know nothing about everything. There is danger in either going ever broader or ever deeper.

Somehow our process of defining research topics, allocating funding, gaining support for research, writing research proposals, and so forth, tries to start from these different perspectives of finding a meeting of the minds so that a transaction can take place in the research marketplace. You want me to offer the use of my research background, skills, and data to solve your particular, most pressing, current problem, I know only a little about your particular problem, but I want your money to address the problems that are of intellectual and professional interest to me. We barter, negotiate, and agree on the terms of a research contract. In the end, we are both dissatisfied because you find my work too abstract, too intellectual, and not quite specifically helpful to your current and pressing problem, and I find you unappreciative of the sophisticated analysis I have done.

In a marketplace you can sometimes find quality goods and sometimes you can find junk. You can find genuinely crafted products, and you can find cheap imitations. There is often a demand in the marketplace for each. This is true in the marketplace for research as well. Very often, I find that people in the public policy arena want to call upon me as a researcher to "validate" something that they know in their gut is true or to "prove" that some particular approach to resolving a problem is consistent with findings in the research literature. You are often most delighted with research results when they provide you with a vote of confidence for what you instinctively know to be true, and decision makers are often outraged and disappointed in research results when they tend to suggest the opposite—that some carefully constructed public policy is not likely to achieve its intended and hoped-for objectives. There is a natural human tendency to define research as good in quality when it supports your preconceived notions and as "deeply flawed" when it does not.

Yet researchers by nature are trained to be eternally skeptical. We are always trying to test findings that appear to be promising by trying out generalizations in new circumstances and by testing the limits of what appears to be true to find out the conditions under which these generalizations are no longer true. Often, this proves enormously frustrating to policy makers. It's easier to buy junk research—quick and dirty studies that

prove something we intuitively know to be correct—than to stick with high-quality research that remains skeptical and goes ever deeper to try to test the limits of truth. I want to illustrate this assumption by using as a case the relationship between transportation and economic development. Virtually every supporter of a proposed highway project, a subway project, a port expansion, or an airport renovation wants to make the argument that this project, if built, will contribute to the economic wellbeing of the area—that is, economic development benefits will make the project worthy of the costs, and it will be an investment in jobs, economic efficiency, and so forth.

Invariably, research is commissioned to prove that the economic benefits of an intended project are indeed significant. One of the most competent young researchers in this field is Marlon Boarnet, who is right here at the Irvine campus. I've studied his research really carefully. He has looked at a number of studies of economic benefits from highway and transit investments, and his findings are rather disturbing. Those studies showing most unambiguously that highway and transit investments create net economic benefits tend to be methodologically the most flawed. The most thorough and rigorously conducted studies raise the biggest doubts. The studies tend to show that most of the benefits are the result from redistributions of economic benefits that would have occurred elsewhere had the projects not been built, instead of as a result from the creation of net benefits.

In other words, the studies that give the answers that are most desired by the policy makers are the studies that are the weakest when criteria of good research are applied to them. When rigorous research is done, it tends to be unable to sustain the conclusions that the policy makers want most from the research. This inability tends to cause policy makers to prefer to fund cursory, shallow research that gives them results that support their gut reactions, while at the same time, decrying research that other researchers think of as brilliant but that decision makers find indecisive or unhelpful. There may be a great market for schlock, while the work of the true craftsman is left on the shelf. This thought may be very disturbing, but it is an extremely important insight.

By using the notion of a marketplace as a metaphor, we might ask, Are there some principles that we can bring to this marketplace that will help us make it more productive for both researchers and for those who are engaged in public policy making? What should we expect is possible from research, and how can we get research results that are more useful and more valid at the same time and also more timely?

I can think of a few general statements that I would like you to consider as you conduct your workshop dis-

cussions over the next few days. These statements grow out of this notion that if there is a "market" for research, there has to be, at some point, a meeting of minds between the buyer and the seller to address ways in which, I think and hope, these perspectives can be brought closer together.

First, it is better to define research topics that are narrow, bounded, and precise than to define topics that are broad and general. Researchers are unable to respond as effectively to calls for general thrusts in research as they are to specific requests for analyses and evaluations. Products of research are more useful when the funding agencies are more clear and precise in formulating their expectations from research. This task is hard to do, but it is a mistake to place the burden for doing this on the researcher alone.

Second, there is far too little genuine evaluative research being done in the field of transportation planning and policy. We are constantly implementing new concepts or applying older concepts in new contexts. An enormous amount of learning could take place if we did genuine, unbiased evaluations of many more of those applications. Too many transportation innovations are unstudied, and perhaps even worse than that, too many innovations are evaluated in cursory and politically motivated ways so that real lessons are not learned at all. We cannot admit our failures, so we pursue our self-interest by declaring every experiment a success, and we apply weak and self-serving evaluation techniques. Research would be more useful and valuable if we funded truly independent evaluation studies of experiments. The Federal Highway Administration and the Federal Transit Administration could play an enormously valuable role if they insisted on truly independent and truly rigorous evaluations of new transportation projects and services. Examples of these projects and services include land use impacts of capital investments, social or economic effects of construction programs, rail lines, and commuter bus lines.

Third, the development of new technology, devices, and materials are critical parts of a transportation research program, but they must be complemented by research on institutional and organizational issues in transportation and on decision-making processes. In addition, people who are interested in planning and environmental issues must insist that our research programs should be more balanced to include these softer issues as well as the traditional harder topics. I think often of the really exciting work being done on smart vehicle technology, which blends telecommunications and transportation. But as we go deeper and deeper into intelligent transportation systems research, we really are not doing enough on the institutional and organizational aspects of these technologies. How human beings respond, how organizations respond, and how planning

should respond to ITS are issues that are in enormous need of research attention. Engineers think that the technical dimensions of these issues are the interesting parts, planners deny the significance of the whole endeavor, and there is no meeting of the minds in the critical realm of organizational processes that promise true social progress.

Fourth, it is appropriate to set aside at least a portion of our resources for research support for basic research, for speculative and exploratory work, and for researcher-initiated studies. In comparison with other fields, such as medicine, health sciences, physics, and chemistry, the client agencies for research in transportation call for specific project-related products, and creativity is not given a sufficient chance to blossom on the basis of the initiatives of the researchers themselves. The research in transportation is, by comparison with other fields of endeavor, too much driven by crises, current needs, and short-term interests. We suffer from discontinuities and from lack of depth, because we are unwilling to sustain our research programs over a long period of time.

Fifth, research in transportation has been less productive and less useful than it could be, because the funding agencies don't have sufficient "stick-to-it-iveness." Studies are initiated, and before they can be refined, perfected, and fully developed, they are dropped as we pursue other areas that have become more faddish. We discard the older topics before the researchers develop a sufficient understanding to make the results usable. In the 1970s, we had an active and a creative research program in travel demand analysis and forecasting, and we let it languish. Now, some are trying to rebuild that program with the Travel Model Improvement Program and are having an extremely difficult time. The discontinuities in this program have been enormous, and today we are paying dearly for long lapses in our commitment to conduct research in this field.

Sixth, I would like to note that some areas of transportation research have been on the list of topics that

need to be researched for decades. Yet, we have failed to create the marketplace in those areas to actually enable a meeting of the minds between those individuals who need the results and those who might be willing and able to do it.

Once again, the first refocusing conference in Washington identified a need for research in goods movement. Goods movement will be one of the major areas of growth in traffic, an area in which environmental policy will be pushing (diesel engines and particulates). Every major research conference on transportation for the past 25 years has listed better models, better data, better forecasts, and better analysis tools for goods movement as a pressing research need. I would predict that this conference will do the same thing. But why have we not started a major research program in goods movement? Why are there so few projects funded and so little to call on in the way of research results? Every organization thinks that goods movement is an important topic, but not one has the responsibility to invest resources in a program or sees it as a topic for which it has a particular competitive advantage. In other words, no market exists in which there are real suppliers of needed resources for research on goods movement nor are there real bidders who are pressing to do more research in goods movement. Therefore, we remain content to give goods movement research a place on our lists of things to do, but we never get around to doing it.

If you want to make a genuine contribution to research in the public interest over the next 3 or 4 days, try to structure research and strategy programs for creating markets in research. Also, after we leave here, try to get your organization to become involved in creating those markets in which clients and purveyors of research might actually meet. Please commit yourselves to answering questions honestly and to intellectual rigor and honesty in the research that you do in transportation. Don't be satisfied at this conference by just making lists of research needs. We need much more than that.