# RESOURCES, TECHNOLOGY, ENVIRONMENT, WHAT IS THE QUESTION ?

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**ABSTRACT**
This paper resulted from thinking-at-the-typewriter in an attempt to accomplish two goals. The first goal was to be useful in the design of a new research project at the International Institute of Applied Systems Analysis (IIASA). The project was to deal with "resources, technology, and the environment in agricultural production". The second goal was to design my own next research project in a way that was efficient, relevant, and in alignment with the project at IIASA. The comments that follow were addressed more to myself than to anyone else. If they sometimes sound critical or impatient, it is because parts of myself need that kind of talk from other parts of myself, not because I was trying to tell someone else what to do. Since then my students and other analysts have found the paper useful and have encouraged me to publish it. I have generalized it somewhat and left in just enough references to "resources, technology, and the environment" and to IIASA to use that research project as an illustration of my general points about problem definition.

**ON THE PROBLEM OF DEFINING A PURPOSE**
*Every scientist and systems analyst should define the purpose of a research project carefully before beginning it.* That is perfectly obvious. Everyone knows it. The purpose of a project shapes everything that follows – defining the purpose is the most important single step in research. Everyone also knows that the very essence of scientific creativity is to have not just a mundane purpose but a powerful, piercing Question, one that defines sharply the area of inquiry, that sets one's mind working in fruitful directions, that leads one to see the world in new and useful ways. Asking the right Question can transform a project from mediocrity to brilliance. Once we have that Question, then we can pursue its answer through the scientific method:

Question → set of possible hypotheses that might answer the Question → elegant, pivotal set of experiments designed to *disprove* false hypotheses → method or methods appropriate to the experiments → data, observations of the world → hypothesis that has not been disproved; tentative answer to the Question

All of us who have been trained in the sciences have been taught this. And we almost never act as if we know it. What usually happens, especially if we are academics without a policy client, is that we have not a Question but a vague subject area we would like to know more about (resources, technology, environment). Our minds immediately start dithering with details like data bases, computer software, budgets, and personnel. Sometimes we define the purpose and boundaries of the project entirely because of available data. Sometimes we leap instantly to a method and shape the project, including its purpose, by what we can optimize, or fit into an input-output matrix, or express in feedback loops. Occasionally we have a hypothesis, or more likely a conviction, an ill-defined hunch. We then design our project to *prove* or demonstrate that, without ever considering other possible hypotheses. Most usually, I suspect, we do all these things at once, carrying around a wonderfully complicated mush in our heads, envisioning first this and then that corner of the model, first this and then that tremendously important thing we will learn from it, first this and then that idea about how to proceed.

In short, *most research projects and models never have a sharply-defined purpose.* They are based on a sort of urge about what would be nice to do, and they develop in a formless way until the time or research funds run out, at which point what has been done is really not much use for anything. In my own first modeling project I never formulated a clear purpose for my model until I was writing the documentation, after two years of work. After learning the lesson the hard way, I now make a practice of asking my students and colleagues at any research seminar or model presentation, "what is the purpose of this model?". Try that sometime, and watch the surprised reaction followed by the uncomfortable, imprecise answer.

Once in a while purposeless drifting is a good idea. If you can do it with an aware, open mind, you can make interesting, unexpected discoveries. But in my experience, the result is most likely to be:

inefficient allocation of effort and resources (if there is no clear purpose against which to measure importance or relevance, there is no way of realizing that one is spending too much time on inconsequential details);

immense megamodels that are impossible to control, understand, validate, or document (a vague purpose gives no reason to leave anything out of a model);

mechanical re-use of old concepts, routines, and programs, that are inappropriate, inharmonious, and distracting (prefabricated assemblies from other models may or may not have been appropriate for their original purposes, but they are rarely ideal for any other purpose).

insufficient and inconclusive model testing (it is simply impossible to design an intelligent test with an unclear model purpose);

choice of research areas, boundaries, and degrees of detail that are either trivial or impossible, while truly grave, urgent, and possible tasks go unattended (this is the greatest tragedy of all).

I would contend that the above characteristics are all-too-descriptive of the field of social systems analysis. I would also assert that most of those problems could be cleared up if we were more careful and conscious about defining our research Questions.

Why, when we absolutely know better, do we go on launching analyses with amorphous statements of purpose such as "exploring the interactions among A, B, and C" or "understanding the behaviour of system X" or "constructing an all-purpose planning tool for Ministry M"? Why do we so readily try to model *systems*, which have an infinite number of elements and no clear boundaries, and why do we resist modeling *problems* that could possibly be limited and understood and solved? I have several possible explanations for why I, at least, do that:

I really do want to know everything about everything; I would like to be helpful and to make the world work better, and I am too impatient to go one step at a time;

computer modeling is so difficult and complicated that it doesn't seem worth doing it for any small, temporary purpose; if I'm going to bother to make a model, I want it to be useful for a long time and for many different purposes;

I am isolated in a world of theory and idealism; I don't see enough of the policy world to get a sense of urgency or relative importance;

I am isolated in a world of theory and idealism, supported by public funds, spoiled, lazy, and unaccountable for my results; I have no pressure to be clear or relevant;

Thinking, being creative, perceiving the essence within the myriad details is hard work, work that calls on more than just rationality, work that I have not been trained to do. Dithering with details, sifting data, writing equations is easy and fun; I have been trained to do it very well;

I want to show the world I'm doing something, not just staring into space. Doing something means assembling complicated mathematical structures, running the computer, and writing reports, not searching for Questions or producing simple, elegant answers.

## HOW CAN ONE FIND THE PERFECT QUESTION?

Since I have trouble myself with defining clear, brilliant research questions, I am not going to be able to set forth the definitive rules for how to do it. I suspect that since the selection of a fruitful Question involves creativity and inspiration, there are no definitive rules. Inspiration does not come from following rules.

But I am sure that I and others in my field can do better than we have done in choosing and clarifying model purposes. So I have assembled some ideas and suggestions that might help. Some follow directly from the reasons previously listed for why I have not defined my Questions carefully in the past. Other ideas come from things that have worked for me or for other people in other modeling projects. Most of these suggestions are quite frankly unproven. They sound like good ideas to me, and I will try them in my next project. The purpose of these suggestions is not to prescribe what one *should* do, but to nudge one's mind loose a little bit, to elicit further suggestions, and to make the crucial step of problem definition become more serious, deliberate, and systematic instead of haphazard and unexamined. So here are some things to try:

### Collect Good and Bad Examples

Once I asked my students to do an impact study on a large pulp mill that was to be built in the river valley where I live. Some of them went into the study with the Question, "exactly what will this mill do to this valley?" This question was far too inclusive; it assumed wrongly that exact and detailed prediction was possible; and its time horizon was too vague. The students got bogged down very quickly by the enormity of the task.

Others went at it with the Question, "how can I prove that the impact of this mill will be devastating and that it should not be built?" This was not a question at all, but an answer. It blinded them to many truly beneficial aspects of the mill, and it gave them an attitude that alienated them from many sources of information (such as the mill company).

The fruitful Question in this case turned out to be, "what could we suggest to both the mill company and the people of the valley that would increase the beneficial impacts of the mill and decrease its adverse impacts?" That question kept our attention on both good and bad impacts, interested both the advocates and the opponents of the mill, and shifted the focus from exact prediction, which is impossible, to constructive policy suggestions. (The result was that the things that needed to be done were so costly that both the valley and the mill company decided to abandon the project).

Other examples from my experience:

| **The Question** | **resulted in** |
|---|---|
| How does the US economy work? | An enormous model that has not yet yielded a significant published result in over 60 man-years of effort. Only 3 of a planned 18 sectors of the model have been linked, and they already produce behaviour that is not understood by the modelers. |
| Why do Americans throw so much garbage on the roadsides and how can they be made to stop? | A requirement for impossible data such as the average number of soda cans drunk per passenger-mile. The model was abandoned before completion, and a new model was begun around a new question – what determines the flow of resources from mines through useful products to garbage dumps, and what policies would increase the utility and |

| | |
|---|---|
| | life-time of the resource base? This question produced a simple and useful model. (Incidentally the first question has been answered and implemented into successful policy in some states on the basis of mental models alone). |
| Why is the number of dairy farms in Vermont decreasing? | A clear, validated model of moderate size with useful policy recommendations. The model also suggested general reasons for the loss of small farms in all agricultural sectors. |
| Why is the Sahel nomad culture so vulnerable to drought conditions? Are current aid efforts decreasing the vulnerability? | A clear model that concluded that the current forms of aid *increased* vulnerability. The model was not about to suggest what kind of aid, if any, *could* improve the lot of the nomads. That was not in the original research question and the model boundary was too narrow to permit an answer. |
| How will increasing energy prices affect income distribution in the U.S.? | The modeler actually knew what he wanted the answer to this question to be and built the answer into the model. The model demonstrated nothing except the mental model of the maker. |
| How can the U.S. economy accomplish a smooth transition from non-renewable energy sources to renewable ones over the next 100 years? | An initial moderate-size model that captured the basic problem clearly and graphically and gave the modeler so much understanding that he was hired to develop the U.S. energy plan. The model was then expanded to answer new questions – what will be the effect of a major synfuel program? how much oil will the U.S. import in 1990? – that increased the model complexity by several orders of magnitude and turned it into a black-box accounting device that only its makers trusted. |

This short catalog and other examples we all can think of already indicate some common traps to beware of. Research Questions can be dangerous if they are:

too broad and vague to limit the problem;

too narrow to contain the solution to the problem;

not really questions, but answers (especially dangerous if the answer contains a hidden assumption that is simply wrong);

framed in a way that antagonizes important actors in the system;

likely to lead to unproductive side-tracks;

dependent on data that cannot be found, or assuming predictability of things that cannot be predicted.

On the other hand, I can draw two positive conclusions from these examples. First, Questions phrased around clear policy levers or agreed-upon social goals are likely to be productive ("how could we stabilize business cycles?" or "can fiscal and monetary policies stabilize business cycles?" seem to be more feasible questions than "how does the U.S. economy work?"). And second, a simple Question asked about a specific and bounded region (the disappearance of the Vermont dairy farmer) may lead to clues to more general solutions. It is worthwhile to look at other examples and to muse upon the reasons why some questions lead directly to successful models while others do not.

**Spend Time in the Policy World and Look for the Real Problems**

I am often amazed by how easy it is for me in academia to assume something is a problem in Washington when in fact it isn't. Hanging around the areas where the decisions in the system are made will lead to useful insights about the perceived problems and the way the problems are now being handled. It is useful to watch how different kinds of actors in the systems perceive different problems, all of which may be symptoms of the real Problem.

Although I think listening to policy people and experiencing their world is a good idea, I would like to qualify this recommendation by saying that it should be only one input to deciding the Question. The policy world is as isolated and distorted as is academia (though in different ways), and policy makers are not much better at defining truly fruitful Questions than we are. They tend on the one hand to ask questions entirely too limited by short-term political feasibility (how can we keep the price of corn high until after the Iowa primary?) and on the other to ask for things that are simply impossible to answer (what will the price of O.P.E.C. oil be in 1995?). The questions they ask are often disguised answers ("How can I train more managers?" when the more fruitful question might be "How can I reduce the turnover rate of managers so I don't have to train so many?"). However, some of their simple, blunt unanswerable questions (what outside shocks are likely to mess up our agricultural 5-year plan?) can suggest more feasible Questions (how resilient is our agricultural system to outside shocks? How can we make it more resilient?).

In summary, it's good to be in touch with the places where the problems are felt and are dealt with, but listen for the real questions underneath the voiced questions, the real problems underneath the problems that are recognized.

**Take Time to Think About Your Question**

I have heard modelers recommend allocating one-fourth of the time of the modeling project to problem definition. I know a private consultant who informs prospective customers that it may take months of talking just to define the problem well enough to know whether a model is even appropriate. Despite all the pressure to the contrary, one should not jump into the technicalities too soon, before the problem definition is clear. If you *expect* to use your time that way and convince your clients of the importance of doing so, perhaps you can keep away the pressure to starting modeling.

During the time one is searching for the Question, a lot can be done:

learn about the system. Talk to people, watch them make decisions, listen to their complaints, read the literature. Don't take any of it too seriously, don't grasp for conclusions. This exercise will not only help define the Question, it will also establish contacts and data sources when the actual modeling begins.

have Task Force Meetings. Bring together all sorts of people who are concerned about the system. Ask them what Questions they wish they were working on. Find out what Questions have already been answered and what Questions have bogged people down.

be childlike. This is the perfect time to ask stupid questions. I went around IIASA for a day playing the game of the magic box with the agricultural experts there. "I have here a magic box that can answer any question about agriculture. But you are allowed to ask it only one question. What question would you most like to have answered?" Interesting thoughts emerged from that exercise.

keep a notebook. Fill it with ideas, names and addresses, questions, impressions of what people tell you. Sketch out a lot of different, simple models in it as they occur to you. Every now and then reread the whole thing and see if any Question leaps out at you.

**Make Lists and Stare at Them**

In other parts of this paper I have included lists of Results of Unclear Problem Definition, Reasons for Unclear Problem Definition, Types of Faulty Problem Definition, and (yet to come) Active Hypotheses, Characteristics of the Ideal Problem, and Institutional Strengths. These are all good lists to make. Another essential one is Goals for the System under Study. For example, here is a list of the characteristics of the ideal agricultural system (compiled in a short brainstorming session with three scientists at IIASA:

sufficient production to meet social needs;

stability; resilience to outside shocks;

efficiency in use of all input resources;

work that is interesting, regular in stress but varied in content, under the control of the workers, equitably distributed;

production as near as possible to the consumers (to reduce transportation needs, increase freshness and nutritional content, allow easy return of wastes to the land);

variety of products;

physical beauty of the surroundings, peace;

healthy working conditions;

nutritional products;

security; minimal dependence on far-away or foreign systems;

basis of up-to-date scientific knowledge, generation of new knowledge;

openness to creativity, experimentation, evolution;

kindness to animals and to the environment.

Looking at this list, I am struck by how little it seems to resemble modern agricultural systems. That suggests several research Questions.

**Plot Time Series and Stare at Them**

For my modeling paradigm, dynamic simulation, the most consistently useful technique for defining a research question is to collect time series on major variables in the system and to formulate questions around them. Why does energy use per unit product in agriculture increase exponentially? Will it continue to do so? Why do commodity prices oscillate regularly, each with its own characteristic period and amplitude? Can anything be done to reduce the oscillations? Grain yields in the U.S. have just levelled off after several decades of steady increase. Why have they levelled off, and is this a temporary interruption in a trend or a sign of a new trend? We require our students to define every model they make in terms of such a time plot, which we call a *reference mode.*

A few cautionary notes. This way of defining a problem is obviously method-specific. It already assumes dynamic simulation, not optimization or input-output analysis or queuing theory. Also this technique is dependent on things that have already been measured. It may miss some important problems that do not show up in statistical time series. And it is very likely to call attention to symptoms rather than to the heart of the problem. A reference mode can at best be the stimulus for a lot of hard thinking and research.

**Collect Hypotheses, Including Your Own**

If the general area of the investigation is clear, formulation of a specific Question sometimes comes from looking at the major conflicting hypotheses or biases. For example, in the area of agricultural technology, three major hypotheses are currently alive and debated in the U.S.:

technological trends will continue roughly as they have been; more use of genetic manipulation, artificial fertilizers, mechanization, pesticides and other chemicals;

distinctly different new technologies will appear that will change the nature of food production totally; single-cell protein, hydroponics, chemical synthesis of protein, preservation of food by radiation. Soil and water resources will become less important, nuclear energy will replace solar energy as a major input, climate will be irrelevant;

there will be a shift to decentralized, soft technologies based on renewable resources; composting, solar greenhouses, windmills to pump water, natural pest management, companion planting. This will be scientifically based and yields will not decrease.

In the tension created by these different hypotheses some interesting research Questions are lurking.

When we decided to investigate the decline of small farms in America, one of the most fascinating exercises was to collect all the opinions about the causes of that phenomenon. It was also essential that we recognized and admitted our own opinion and worked especially hard to falsify that one, so that the research Question did not inadvertantly become, "how can we prove that our hypothesis about the disappearance of the small farmer is right?" (As it turned out, it was wrong). There may be no more important step at the beginning of a project than the admission of one's own pet hypothesis, to oneself and to others, and the relocation of that hypothesis from center stage to equal consideration with the other plausible hypotheses one can collect.

**Be Clear About All Your Goals and Align Them**

Most people do this subconsciously. It is not likely that you will come up with a research Question that will get no funding or that will cause your boss to get furious or that will demand a modeling technique you do not know how to do. I find it useful, however, to be very explicit about all the institutional and personal goals I am trying to achieve with a modeling project, to make sure that the balance between them and the big Question is correct. Otherwise I am too likely to let small institutional constraints dominate what I will try to do, or alternatively to do something that is just not appropriate to my very real institutional constraints. (A more positive way of saying that is, I will miss doing that which my institutional setting and resources uniquely equip me to do.) A balance is needed here, a perfect alignment, with neither the institution nor the scientific Question in total control.

I make lists of all my goals and constraints, and I stare at them until some sort of accommodation emerges. For example: Characteristics of the Ideal *Research Problem* for IIASA:

- promises to solve a problem recognized by some or all of 17 IIASA member nations (in fact, preferably, it should make them all sit up and take notice and enthusiastically want to know the answer);
- builds on and enhances the scientific network and the methodological knowledge gained from previous IIASA research;
- is feasible within a 3-year time frame and with IIASA resources;
- interacts positively with other IIASA programs and areas;
- requires understanding of complex interconnections; lends itself to computer modeling;
- is not likely to be done elsewhere in the world; utilizes the special institutional strengths of IIASA which are:
  1) access to information and scientists from both East and West;
  2) ease in assembling cross-disciplinary teams;
  3) reputation for non-partisan analysis;
  4) dedication to pure research, no teaching or other distractions;
  5) excellent support services (computer, library, etc.);
  6) ability to assemble international Task Forces;
  7) European location.

Characteristics of the Ideal Method for the IIASA resources–technology–environment problem:

- is appropriate and adequate to the (clearly defined) problem (I have to add my strong bias here that for any problem about resources, technology, and the environment with a time horizon of over 20 years, the method should not be basically static or linear);
- is familiar to and appreciated by the Task leader;
- is accessible to all major disciplines involved in the problem;
- is transparent to policymakers;
- is usable on the IIASA computer system.

**Follow Your Heart**

Ultimately the best Question is the one you really want to answer for yourself, the one lurking in the back of your head that you have never had time to work on, the one you can devote yourself to fully and joyfully. Not everyone has the opportunity to choose a research topic freely; when you do have that opportunity, you certainly ought to use it. Sometimes it takes me quite a long time to look inside and to loosen up all my self-imposed and society-imposed repressions so I can discover what Question is truly dearest to my heart. But when I find it, I usually find that it is dear to other hearts too.

One thing about problem definition is clear to me; when you do not have the right Question, you know you don't, and when you do have it, you know you do. The most useful piece of advice I can think of is:

> When you don't yet have a Question, don't pretend you do.
>
> Don't start modeling.
>
> When you do have a Question, don't pretend you don't. Announce it loudly and clearly, so others can support you in finding the answer.

## APPENDIX 1: LIST OF POSSIBLE QUESTIONS FOR A PROJECT ON RESOURCES, TECHNOLOGY, ENVIRONMENT, AND AGRICULTURE

Is it possible that short-term, economically-motivated decisions might set off long-term, irreversible, deleterious ecological or technological processes?

What new technologies are in the works, and when might they become available?

If new technology X appears, how quickly will it be adopted?
What effects will it have on production?
What effects will it have on other technologies?
What effects will it have on the environment?

What systematic factors in a society and an agricultural economy cause new technologies to appear? Could one look at economic pressures, or shifting social value priorities, or "empty niches" and guess what new technologies will be brought forth?

Which of the imaginable new technologies are most likely to lead to the achievement of production/employment/environmental goals?

How might exogenous influences like oil price or government trade policies affect technological choices of farmers?
Are current agricultural systems ecologically and economically sustainable?
If not, how could they become so?

How can a region make a transition from technologies based on petroleum to technologies based on more abundant resources without sacrificing production or other goals?

How can the renewable resource *base as a whole* (forests, soils, waters) be managed to meet both short- and long-term goals?

How can the agricultural system be designed to be more resilient to outside shocks?

How great a swing toward organic technologies could be realized in a region before production is adversely affected (if it is adversely affected)? By how much could applications of chemical fertilizers and pesticides be reduced? Is there enough recyclable organic matter to maintain soil fertility with no other inputs?

Up to what point is increased self-sufficiency an improvement, counting all costs and benefits including environmental ones?

What is the likely effect of technology X on patterns of income distribution?
On working conditions? On employment?

How many people at what standard of living could be supported on a given resource base?

How can we account economically for the use of common environmental resources that are degraded in the process of agricultural production?