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What's Economics Worth?
Valuing Policy Research

EDITED BY PHILIP G. PARDEY AND VINCENT H. SMITH

Published for the International Food Policy Research Institute

The Johns Hopkins University Press
Baltimore and London
To Vernon Ruttan
who ignited and encouraged
our interest in evaluating
economic research and,
through his own inestimable
creativity, pointed the way ahead
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As economists, we invest in economics research partly because we are fascinated by economic behavior and relationships. Moving beyond myopic self-interest, we also genuinely believe that careful and thorough economics research focusing on growth and on the needs of the poor has tremendous potential to benefit the lives of people throughout the global community, often by vastly improving the policymaking process and policy decisions. That we, as economists, believe strongly that economics research is worth a great deal is regrettably viewed by research funders and administrators as a less-than-compelling justification for future economics research funding. Public and private sources of research funds exhibit similar skepticism about the claims of many other science, social science, and technology disciplines on their purse. However, many of those other disciplines have appealed to a third-party arbitrator—the economics profession—to demonstrate what their research is worth. Economists are less fortunate. We have to appeal to ourselves.

Further, the outputs of economics research are rarely as tangible as those of the science and technology disciplines with which economics often competes for funding. Economists provide analysis and information, especially in the context of policy processes and debates, not better medical procedures. Nevertheless, the contributions of economics research to the daily quality of life of individuals and communities across the world can be substantial. Information that improves macroeconomics policymaking preempts catastrophic recessions. Research that establishes mechanisms for the efficient creation and functioning of markets enables societies to experience long-run economic growth and dramatic improvements in per capita incomes. Timely economic analysis in setting and implementing policies frequently forestalls socially wasteful resource allocations and reveals the impacts of proposed policy changes on different social groups (for example, low- and high-income households, producers and consumers, different ethnic groups, and rural and urban sectors).

Furthermore, like the medical science profession, economists occasionally produce flawed research or communicate misleading solutions. Thus, the value of economic research can at times be negative, especially when paradigms are
fixed, research is ideology-driven, or the market for research quality or policy is malfunctioning. It may be partly for these reasons that documenting the value of economics research has generally been a task that economists have been reluctant to take on, especially compared with their willingness to investigate the value of science and technology research. The editors and authors of this book take an important step along the methodological and practical paths of valuing economic research, with a specific focus on its utility in the policy process. The work is timely. All research is increasingly subject to scrutiny in terms of its impact on and value to society. Economists can no longer afford to shy away from hard-nosed and credible economic assessments of their own work.

The International Food Policy Research Institute (IFPRI) recognized this fact of research life early on, but also realized that the issues were endemic to the economics research profession. Between 1996 and 2001, several conferences were held and papers commissioned that drew upon the expertise of economists and policy practitioners from a broad range of academic, government, and international institutions and an array of social science disciplines. The studies in this volume represent a distillation of the outcomes of those efforts. The authors are widely respected economists, and the results of their contributions are insightful and provocative. Their research has helped to shape IFPRI's own approach to assessments of the impacts of its research programs, and it is hoped that the concepts identified in this volume will be tested and developed further in low-income country contexts. By raising many questions that economists have not yet addressed, this volume has the potential to contribute significantly to the economics of evaluating economic, social science, and policy research.

To optimize the value of economics, economic research may need to be further linked to complementary research in such fields as political science and communications in order to strengthen the impact of economics.

This book also highlights the importance of economics research and economists in the policy process. It is essential reading for professional economists and graduate and undergraduate students interested in the value of what they are studying. The density of economists (per million members of the population) may not often correlate positively with long-run economic growth episodes (as in China), but the North-South gap in terms of trained economists—and agricultural economists in particular—is clearly a constraint on good policy research and policy implementation.

Joachim Von Braun
Director General, IFPRI
Acknowledgments

We thank the authors of the contributions to this book. In many cases, the studies included in this volume involved the development of original work and concepts in an area of economics that has received little previous attention. Several of the studies were presented at three symposiums—two sponsored by the International Food Policy Research Institute (IFPRI), in Washington, D.C., in 1996 and in Scheveningen, the Netherlands, in 2001, and the third at the American Agricultural Economics Association annual meetings in Toronto in 1997. Commentators and participants in those workshops provided invaluable insights, and we owe them all a considerable debt of gratitude. The symposiums received generous sponsorship from the Dutch government, with additional core funding provided by IFPRI. This project could not have gone forward without their support. Additional resources were provided by the American Agricultural Economics Association (AAEA); the University of Minnesota, St. Paul; Montana State University, Bozeman; and, through the auspices of then AAEA President Walter Armbruster, the Farm Foundation, Oakbrook, Illinois.

Two individuals deserve particular recognition. Robert van der Berg, formally the director of Policy and Operations Evaluations at the Ministry of Foreign Affairs, the Netherlands, was the first person to urge IFPRI to address the important research issues that underlie assessments of the value of economics research. He has been a constant source of encouragement and insight. Per Pinstrup-Andersen, formally director general of IFPRI, was a tolerant, patient, and cheerful inspiration to both of us. His belief in this project and commitment to quality economics research was a true driving force for excellence in this work. The present director general, Joachim Von Braun, has been equally supportive, and we are most thankful for his encouragement in the completion of this work.

Many other people deserve recognition. Julian Alston, as usual, has been exceptional in his willingness to read draft manuscripts, provide positive critiques, and chide us into clarity of exposition. Nicole Ballenger, Peter Barry, Bruce Beattie, Curt Farrar, Bruce Gardner, George Norton, Sherwin Rosen, Jim Ryan, Kerry Smith, Wendy Stock, Dan Sumner, Brian Wright, and David
Zilberman have all been wonderful colleagues, willing to share broad perspectives that went well beyond any individual contributions. We also owe a real intellectual debt to two anonymous referees. Several people contributed directly to the development and production of the manuscript. Laura Smith established and analyzed an extensive bibliographic data set for this project. We are indebted to her for this effort. Sam Bixler and Luke Martin provided able research assistance and data management expertise, and Louise Letnes tracked down data and references from obscure sources. Misty Herman from the AAEA office was great. Mary Jane Banks shepherded the manuscript through multiple drafts with wit, style, and true professionalism. Finally, it is common to be especially appreciative of the patience, support, and forbearance of one’s spouse and children. We certainly cannot excuse ourselves from this responsibility and will be even deeper in household debt for several years as a result of this project.
PART I

Context
1 Overview

PHILIP G. PARDEY AND VINCENT H. SMITH

Economics matters, and—as a result—economics research has become an indispensable component of policymaking processes in all developed and most developing economies. Indeed, societies that pay little or no attention to the implications of careful economic analysis generally condemn themselves to substantial social and economic instability and, frequently, to relative or absolute poverty by failing to make realistic decisions. Economists can provide their societies with genuine economic welfare through their research and also by educating the citizenry to grasp the insights of economic understanding. It is perhaps surprising, however, that the importance of economics research and education in the policy process is not very well documented, despite the fact that economics research has been an expanding sector of overall science and social science research since at least the 1940s.

In reality, close links exist between most economics research programs and issues of direct interest to public policymakers, interest groups, and private corporations. These issues pervade all aspects of social choice, ranging from very broad concerns about the general gains from trade, world poverty alleviation, and macroeconomic management policies to less global issues such as the efficient provision of health services in rural communities and the optimal mix of internal and external financing for a specific private corporation.

Economics and economics research are valuable to many decisionmakers, as evidenced by their willingness to pay for it. In the United States, for example, thousands of economists are employed on a full-time basis by federal and state agencies, and many more economists in universities, research institutes, and consulting firms provide policy-related analysis through research grants and contracts. Economists are also found throughout the corridors of the agencies and departments of the governments of European Union member countries and within the European Commission; in countries such as Australia, New Zealand, Russia, and India; and increasingly in China.1 As the chapters presented in this

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1. The essays in Coats (1981) are among the limited prior literature that attempts a comparative, cross-country assessment of the role of economists in government.
volume show, however, economists' participation in formulating policies in many other parts of the developing world is still woefully limited.

The relevance of economic knowledge to everyday life is reflected by the fact that, in the first decade of the twenty-first century, many undergraduates take at least one economics course in their baccalaureate programs, and hardly anyone questions the need for economics education. By 2003, for example, 36 state governments in the United States had established minimum standards for economic literacy at the high school level, supported in many cases by state-based Councils for Economic Education.\(^2\)

It is widely believed that the contribution of economics research and economists to economic welfare has been substantial. Evaluating just what that contribution has been and assessing the roles of economists in the policy process, either in quantitative or qualitative terms, are exercises that have largely been evaded, in part because they are perceived to be difficult.\(^3\) The exercise of assessing the benefit of economics research has also been avoided because an economic evaluation of economics research has perforce to be carried out by economists, individuals inevitably viewed as having a vested interest in the outcome of the evaluation.

Nevertheless, there are compelling reasons to address these issues. Describing what economists do and assessing what economics research is worth in the policy process is valuable simply because it enables noneconomists and students of economics to understand the potential benefits of economics to the policy process. In addition, the economics profession itself needs effective and credible methods for assessing the value of economics research if public support for economics research is to be maintained. Public decisionmakers who determine both the size of total public research funding and its allocation across disciplines increasingly require evaluations of the benefits of research across all those disciplines.

**The Impact of Economics Research**

Economists have developed many of the techniques used to quantitatively and qualitatively assess the contributions of other areas of research, but, to date, they

\(^2\) The U.S. National Council on Economic Education, founded in 1949, is a nationwide network of state councils and over 225 university-based centers for economic education. The council focuses on improving the economic understanding of students in grades K through 12 by providing effective curriculum and professional development for their teachers. Each year, the council trains over 120,000 U.S. teachers, who in turn affect over 7 million students. The council also provides teacher training in market economics in 20 developing-country economies, reaching 1.5 million students annually (see www.ncee.net/).

\(^3\) An exception is Colander and Coats (1989), which includes 18 essays exploring links among the ideas of professional economists and the popularized and policy-forming forms of these ideas. Stigler (1982), as usual, provides insightful views on this and related topics.
have not been nearly so diligent in developing credible methodologies for examining their own contributions.\(^4\) In fact, estimating the economic benefits of research and development investments in science and technology has itself become a major area of research for economists.\(^5\) Numerous studies have attempted to estimate rates of return on investment in areas such as agricultural science as well as the absolute size of the economic welfare gains and their distribution among producers, consumers, and others. A wide variety of techniques have been used and, at least at first blush, many of those techniques appear as though they could be transferred wholly, or with minor modifications, to the business of assessing the value of economics research.

Appearances may be deceptive, however. As the authors of several of the chapters in this book note (for example, see Harberger and Timmer), often an economist's major contribution is to prevent public or private decisionmakers from committing gross follies by being in a position to offer timely advice rather than simply to create new products, processes, or information that improve productivity.\(^6\) Indeed, if the only contribution of economics research were to reduce the incidence of bad decisionmaking at any level, over time, economics research would have significant effects on productivity and output. In addition, such effects would eventually show up in more efficient uses of resources within the economy as a whole.

Economic realities evolve, requiring economics research to analyze, understand, and inform these new realities. As in other fields of research, economics research produces several types of output: new products, new processes, and new or improved information. Economists do develop new products and new processes, and these typically include applied innovations such as computer

\(^4\) Some analysts have suggested that simply demonstrating that economics and other social science research is used by or feeds into the policy process is sufficient to indicate impact. This may be important in some contexts and it is often a helpful intermediate step in determining the contribution of economics research to policy change, but from an economic welfare perspective it is only a partial contribution to the assessment of the value of economics research and the distribution of the benefits from that research.

\(^5\) See, for example, Alston et al. (2000), who surveyed nearly 300 studies of the economic returns to agricultural R&D, beginning with Schultz's seminal 1953 study.

\(^6\) In the early 1960s, the Joint Chiefs of Staff strongly supported the development of a supersonic transport (SST), partly on the grounds that it should be created because it could be created. Walter Heller, chairman of the Council of Economic Advisers at the time, joined a group of prominent economists including Kenneth Arrow, Paul Samuelson, Milton Friedman, Wassily Leontief, and Arthur Okun—many of whom served on the council—to argue that any reasonable net present value calculation of the returns to the multimillion dollar investment would result in a large negative number. For once, perhaps remarkably, the economists carried the day in a highly charged political debate (Horwich 1982; Heppenheimer 1999). Ex post, the European experience with the French and British Concorde strongly validated the council's original appraisal of a similar project. Kenneth Arrow has noted that even if nothing else he has done as an economist has been of social value, his opposition to the SST project during his service on the Council of Economic Advisers more than paid for the scholarships that funded his education (Arrow 1996).
programs for the development of economically efficient feed rations, pricing airline seats, estimating credit default probabilities, and improved investment appraisal techniques. However, the economics researcher’s major contributions most often consist of new or improved information that enhances public and private decisionmaking. Again, these contributions typically lead to improved use of limited resources that expand an economy’s production possibility frontier. Thus, as is the case with information innovations in other fields, the science and art of economics has considerable economic value. Further, beyond these outputs, economics has contributed very broadly to the quality of decisionmaking in everyday lives by enabling many people to develop what Paul Heyne described as an economic way of thinking (1999).

The potential contributions to economic welfare from economics research are illustrated by the production possibility diagrams presented in Figure 1.1. In Figure 1.1a, $TT^1$ represents the initial production possibility frontier in an economy that produces two broad types of output, $A$ and $B$ (say, goods and services). The two community indifference curves, $U_0$ and $U_1$, are associated respectively with lower and higher levels of economic welfare. The economy may initially be at a point like $C$ in Figure 1.1a, on the economy’s production possibility frontier but not at the product mix that maximizes economic welfare.

Over the short term, economics research (perhaps especially policy-oriented research of the type that facilitates improved decisionmaking, described in this volume by Norton and Alwang, Ryan, Gardner, and Zilberman and Heiman), has the potential to shift the economy from a product mix like $C$ to a product mix like $D$, improving welfare by enhancing economic efficiency. Over the longer term, improved economic policies often also enable societies

**FIGURE 1.1** Production possibilities
to expand their production possibilities, as illustrated in Figure 1.1b. Shifts in a society’s production possibility frontier result either from an increase in the resources the society possesses or from the innovation and adoption of improved technologies.

Economics research, like any other R&D investments, can improve a society’s technology by providing improved processes and products, and it can also effectively increase a society’s resource base by expanding intellectual and human capital. Policy-oriented economics research may also provide opportunities for longer-term investments that increase both the technology and the resources available to society by increasing the efficiency of resource use in the short term (for example, through the design and implementation of improved property rights in emerging democracies) (de Soto 2000). Some have also argued that one important contribution of policy-oriented economics research, especially in a development context, is to create capacity for further research and thereby increase efficiency in decisionmaking (a point emphasized by Timmer and hinted at in Ryan’s analysis of economic policy research in Vietnam).

Harberger’s and Timmer’s chapters both point out that sometimes the most important job for an economist is to prevent bad choices from being made. This function is illustrated in Figure 1.1a. If a society makes a choice, without the benefit of economics research, that would move the economy from $D$ to $F$, say, by choosing to make a wasteful investment in supersonic aircraft, then the research that prevents the poor policy from being implemented is potentially of considerable economic value.

**Economics and Policy**

Due to the close links between economic information and the policy process, economics research that directly informs that process is of particular interest to public policymakers. Thus, whereas several chapters in this volume identify the general nature of the benefits of economic and agricultural economics research (Harberger; Krugman; Smith and Freebairn) and discuss potential methodologies for estimating the benefits of that research, three chapters provide case studies of specific policy-oriented research programs (Norton and Alwang; Ryan; Zilberman and Heiman).

In principle, therefore, the outputs of economics research have impacts on productivity and economic welfare that are qualitatively similar to the outputs derived from research programs in the basic and applied sciences and engineering. Thus, the tools developed by economists to evaluate R&D-related welfare improvements should be applicable to their own R&D efforts, notwithstanding the inherent “fox in the henhouse problem” that comes with the territory.7

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7. At a 1997 conference at IFPRI, Sherwin Rosen expressed serious concerns about the ability of any discipline to assert its own value to society with any credibility.
Economics also has a poisoned-well problem that is particularly important. Whereas effective economics research can do much good, poor economics research can do considerable harm. For example, Anne Krueger identifies a specific area in which a good deal of the economics profession initially made major errors that resulted in the implementation of ill-advised, welfare-reducing policies. Her “case study” of poor economics involves the now-notorious recommendations by otherwise distinguished economists in the 1950s and 1960s that developing countries should pursue industrialization through import substitution policies that relied heavily on import restrictions.8

Rightly, therefore, any evaluation of economics research cannot simply focus on the upside of the research and ignore its downside. Krueger’s insights raise questions about the validity of assessing the economic welfare effects of economics research by adopting a cherry-picking approach. This approach has been justified by the following argument. In economics, as in other areas, many (if not most) research projects are “dry wells” that produce little if any economic benefit. However, some are “gushers” that are inordinately successful. Ex ante, it is not at all easy to sort out which projects will be gushers and which will be dry wells. Ex post, comparing the benefits from those gushers to total outlays on all research provides a lower-bound estimate of the value of the overall research program.

With respect to economics research, this version of the cherry-picking approach is fatally flawed if the poisoned-well problem discussed by Krueger is at all significant, because some of the projects that are ignored in the cherry-picking approach may have large negative benefits.9 A second problem with cherry picking is that it tells us nothing about the likely shape of the marginal product curve for returns to research investments in an area.

### The Objectives of This Book

This book focuses on three sets of issues. The first set involves questions about what economists do, how much of it they do, and how what they do has changed since the 1930s. The second set of issues involves economists’ perceptions of what they do and how they do it. The third set concerns whether or not economists can develop credible methodologies to assess the economic value to society of what they do and, if they can, what those methodologies would be.

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8. It is highly likely that advocacy and adoption of the Prebisch import substitution hypothesis caused the production possibilities frontier (PPF) to move inwards in several developing countries (that is, the policy caused shifts from a PPF like $\overline{V^1}$ to a PPF like $\overline{T^1}$ in Figure 1.1b).

9. Cherry-picking approaches are much loved by directors of research institutes, regardless of discipline; however, the poisoned-well problem bedevils not only economics research but also other research areas such as plant science or entomology. Incorrectly assessing the efficacy of biological pest controls, for example, can have catastrophic consequences for crop output on individual farms and even across the agricultural sector.
In the economics literature, a relatively large number of contributions have been devoted to assessments of the virtues, vices, and vicissitudes of economics research and economists. These essays mainly appear in the form of presidential addresses and invited papers by distinguished practitioners. Several are insightful and present unique and fascinating perspectives about what economists do and the contributions of economists and economics research. Three such contributions appear in this anthology (the essays by Harberger, Krueger, and Lindner). Generally, however, this literature has not provided any systematic insights about or formal methods for estimating the economic benefits of the economics industry.

In Part I, Chapter 2 examines the general economics research industry and the subsector of agricultural economics. Historical data are presented on trends in employment, professional associations and association memberships, and the awarding of graduate and postgraduate degrees in economics and agricultural economics. The changing composition of economics research is examined through the prisms of the allocation of space in leading journals to alternative areas of research and the evolution and organization of economics associations.

Part II also examines the role and value of economists and applied economics research in the policy process. In Chapter 3, Paul Krugman provides a broad overview of the major contributions of economics to our understanding of economic systems and policy choices. Chapter 4, Arnold Harberger's 1993 Richard T. Ely lecture, provides a careful accounting of the role and functions of the economics practitioner in the policy environment. He identifies both the key issues that economists face in such settings and the real-world constraints that limit the scope and likely effectiveness of their advice. Peter Timmer, in Chapter 6, echoes Harberger's theme, providing a more personal account of his experience as an advisor to the government of Indonesia on rice and food policy. In Chapter 5, Vincent Smith and John Freebairn examine the nature of economics research outputs and review alternative empirical approaches to estimating the benefits of this research.

The three chapters in Part III by Lindner, Krueger, and Gardner explicitly consider the value of economic information in the policy process. Bob Lindner, in Chapter 7, describes and critiques the applicability of the Bayesian approach to assessing the potential value of information from alternative proposed agricultural economics research projects. The emphasis in Lindner's study is on ex ante assessments of the potential gains from research, and he draws heavily on Hirschleifer and Riley's analysis of the economic value of new information. Gardner also proposes a Bayesian framework for assessing the benefits of agricultural economics research, again relying on Hirschleifer and Riley's framework of analysis. He provides an ex post assessment of the effects of agricultural economics research on key policy debates, including trade policy reform under the Uruguay Round of the General Agreement of Tariffs and Trade (GATT) negotiations. The study by Krueger (her 1997 American Economic Association
presidential address) presented in Chapter 8 examines the history and economic policy consequences of the import substitution debate. It provides an important example of the potential for economics research to uncover poisoned wells and, at the same time, an example of how academic research and the competition of ideas in the academic marketplace is essential to the development of improved economic analysis.

Part IV contains three case studies describing how the economic welfare effects of specific economics research programs may be estimated. These case studies are drawn from development and agricultural economics settings, reflecting the relevance of economic policy analysis to a broad spectrum of contexts. Economic analysis has clearly also been important in many other policy areas, including regulation, finance, and law.

Chapter 10 consists of an award-winning study by Norton and Alwang of agricultural economics research programs in two developing countries. The study shows not only that the returns to a targeted economics research program that affects policy choices may be large, but also that estimating the degree to which those benefits can be attributed to the research program is itself difficult. Ryan’s study of rice trade policies in Vietnam, presented in Chapter 11, also provides large estimates of the returns to a specific agricultural economics research project in which communicating economic analysis findings to policymakers was an integral component of the research program. Both the Norton and Alwang and Ryan case studies utilized interviews of participants in each country’s policy process to assess the relative importance of the research, but show that this is an imperfect mechanism for resolving problems associated with attributing policy change to research. In Chapter 12, Zilberman and Heiman discuss key conceptual and methodological issues associated with measuring the value of economics research and also provide an interesting case study of the management of water rights in California. They also estimate large net benefits from an economics research program.

Taken in isolation, the optimistic findings presented in the three studies in Part IV and Gardner’s analysis in Part III could very well be viewed as examples of cherry picking (in this case by the editors). However, Krueger’s cautionary tale of the import substitution policy fiasco of the 1950s and 1960s and Smith and Freebairn’s emphasis on the poisoned-well problem that confronts economics reminds the reader that valuing the worth of economics is not an easy task. These issues are revisited in Chapter 13, which summarizes this volume and offers a conclusion.

Finally, our objective in presenting the studies here is to provide a platform for progress toward the development of methodological and practical approaches

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10. For their study, Norton and Alwang received in 1997 the IFPRI Social Science Essay prize for essays on the theme of evaluating policy-oriented social science research.
to valuing economics research, with a specific focus on its utility in public and private decisionmaking processes. Thus, this volume has been developed with several different readerships in mind. The intent is that this material be of interest and accessible to practicing economists, including both graduate and advanced undergraduate students of economics; funders and administrators of research programs; as well as to any regular reader of *The Economist*.

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Arrow, K. 1996. Personal communication with authors.


The business of economics is as complex and diverse as the business of any other knowledge-generation and -transfer industry. Economists teach in schools, community colleges, and universities. They carry out basic research in universities, government, private research institutes and think tanks, and a myriad of private businesses. They provide applied research and technology transfer services through consulting activities to private and public entities, extension and outreach programs, and as employees in government agencies and private corporations. In addition, they serve as policy advisors in a wide variety of public policy settings. Many are teachers and researchers; many are (in Harberger's nomenclature [1993]) policy practitioners working in complex political environments in which the feasible sets of alternative recommendations are heavily constrained. Over time, many economists shift their foci on research and professional activities in response to changing opportunities and incentives, and in so doing they change the structure and composition of the profession.

This chapter provides some perspectives on how the industries of economics and specialty disciplines within economics have evolved over the past 80 or so years both in terms of size and composition. Consistent and comprehensive data on the size of the economics profession are difficult to obtain, but data are available on membership in important professional associations such as the American Economic Association (AEA) and the American Agricultural Economics Association (AAEA). These data, together with information on the number of bachelor's and postgraduate degree recipients and the numbers and functions of professional economics and agricultural economics associations, are used to assess the evolution of the industry in both developed and developing countries.

Among the subdisciplines of economics, in the context of this book, agricultural and resource economics are of particular interest because of their historical importance and relevance to economic development issues. Several case studies on the value of economics research presented in this volume deal with agriculture and food (Gardner; Norton and Alwang; Ryan; Timmer) and resources (Zilberman and Heiman; Norton and Alwang), and a major concern is
the efficient allocation of resources to agricultural and food policy research. In addition, relatively extensive time-series data are available on a cross-country basis on the agricultural economics profession, which allows useful comparisons of the links between economics research and agricultural economics research and the relative importance of the economic sectors served by that research. Further, we found that it is possible to draw some comparisons vis-à-vis the evolution of general economics research and research in a subdiscipline through an interesting case study of an area that at one time was perhaps the most important subdiscipline in the field of economics.

Of considerable interest is the issue of how the composition of the research that economists and agricultural economists carry out has changed over time. To obtain some insights into this issue, we also present new data on the allocation of space in leading economics and agricultural economics journals over the period 1930 to 2000. These data are used to investigate links between trends in the subject matter of economics research and policy priorities. The scope and changing focus of economics is also reflected, somewhat idiosyncratically, in the subject matter for which Nobel Prizes are awarded and the topics that leading economists choose to address in professional forums. The appendix tables to this chapter present the topics of presidential addresses of the AEA, AAEA, and Royal Economic Society (RES) and of Richard T. Ely lectures, as well as the citations for winners of the Nobel Prize in Economics, illustrating the diversity of economics research over the years.

The Development of the Economics Profession

We first examine the evolution of the economics research industry, beginning with indicators of growth in the industry’s workforce. We then present new information on the development of professional activities using data on the origins and expansion of professional economics associations across the world. Finally, we examine the composition of the economics research workforce by occupation, gender, and ethnicity.

*Indicators of the Economics Profession’s Growth*

The American Economic Association may not be the oldest professional association in the world, although it was founded in 1885 and thus predates the Royal Economic Society, which was established in 1890. It is the world’s largest economics association, with a membership of about 20,000 economists in 2000.\(^1\) While inevitably U.S.-centric, the AEA is clearly a professional society with a large international membership, if only because of the preeminence of its

\(^{1}\) For purposes of comparison, in 2000, the Royal Economic Society had just over 3,300 members and the European Economic Association had about 1,700 members.
journal, the *American Economic Review*, and because membership dues have historically been and remain relatively low. Thus, membership in the AEA could be viewed as a proxy (albeit imperfect and with some important caveats discussed below with respect to developing countries) for the development of the global economics research industry.

The AEA's beginnings may have been modest (in 1886, the association had 182 full members and subscribers to its journal) but, as illustrated in Figure 2.1, by 1893, its membership had increased to almost 600. Thereafter, expansion was more gradual: by 1902, the AEA had only increased to 860 members and by 1909, to 1,205 members. Two years later, however, membership had almost doubled to 2,190. Over the next decade, the association's membership plateaued at between 2,091 and 2,466 individuals before a further period of expansion took place in the 1920s, with membership expanding to 2,916 in 1925.

Between 1925 and 1938, membership remained stagnant at, or slightly below, its 1925 peak. Only with the onset of World War II in Europe in 1939, and the recovery of the U.S. economy from the effects of the Great Depression, did further growth in the AEA and, by implication, the economics profession begin. Once initiated, however, expansion was rapid. By 1945, for example, AEA membership had increased to 4,154, and by 1950, to 6,936—about a 68 percent expansion over a period of 5 years. Growth continued to be rapid throughout the 1950s and 1960s as employment opportunities for economists expanded in higher education (in concert with the explosion in student enrollments associated with, initially, the GI Bill and then rising per capita incomes), government, private research institutions, and private corporations. Thus, by 1969, total AEA membership had risen to over 19,000.

The 1970s, however, were years of stagnation for the AEA in a decade of relatively unsettled and weak macroeconomic performance, characterized by two major recessions in the United States and several other developed economies, and very low rates of measured economic growth in all developed economies. In the 1980s and early 1990s, generally a period of economic growth, some further expansion in AEA membership occurred until 1992, when AEA membership reached a peak of 21,273. The association then declined in size, and in

---

2. Founding members paid annual dues of $3. Fees remained unchanged for 25 years, and by 1970 had only increased to $10, jumping to $20 the following year (Siegfried 1998, 213). In 1890, a dual fee structure for individual ($3) and institutional ($4) members was introduced. Annual fees in 2003 were $62 for members earning less than $41,000 per year, $74 for those earning between $41,000 and $54,000, and $87 for those earning more than $54,000. Based on the membership structure, the weighted average dues were $79. If the growth in membership fees since 1890 had matched the rate of increase of average salaries paid to professors working in U.S. public universities, dues for 2003 would have been almost double ($152).

3. This and related figures in the text refer to individual members only. In the membership data available for 1886, there is no distinction between individual and institutional members.
FIGURE 2.1 Annual membership of the American Economic Association


Notes: Data were interpolated for 1979, 1981–83, 1985–87, and 1989–91. Membership in 1896 may include some institutional members because data were not broken down by membership category.

2000, membership stood at about 20,000. Between 2000 and 2002, membership again increased to 21,615.

Members of the AEA initially were drawn almost exclusively from the United States, but over the years the membership base became steadily more geographically diverse (Table 2.1). U.S. members fell from 94.4 percent of the total in 1933 to 71.5 percent in 2002. At first, the preponderance of non-U.S. members was from developed countries, but by 2002, around one-fifth of non-U.S. members were from developing countries and transition economies.

New Economics Degree Recipients and the Gender Gap in the Economics Profession

The total number of practicing economists is far larger than the membership of the AEA. Further evidence of trends in the size of the economics profession in the United States may be gleaned from data on the number of individuals
TABLE 2.1 Geographic distribution of American Economic Association memberships (percent)

<table>
<thead>
<tr>
<th>Year</th>
<th>United States</th>
<th>Developing countries</th>
<th>Developed countries</th>
<th>Other</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Individual members</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1933</td>
<td>94.4</td>
<td>0.5</td>
<td>5.1</td>
<td>0</td>
<td>5.6</td>
</tr>
<tr>
<td>1943</td>
<td>97.4</td>
<td>0.5</td>
<td>2.1</td>
<td>0</td>
<td>2.6</td>
</tr>
<tr>
<td>1953</td>
<td>94.1</td>
<td>1.2</td>
<td>4.7</td>
<td>0</td>
<td>5.9</td>
</tr>
<tr>
<td>1963</td>
<td>91.9</td>
<td>1.8</td>
<td>6.3</td>
<td>0</td>
<td>8.1</td>
</tr>
<tr>
<td>1966</td>
<td>89.4</td>
<td>2.2</td>
<td>8.4</td>
<td>0</td>
<td>10.6</td>
</tr>
<tr>
<td>1969</td>
<td>87.7</td>
<td>2.3</td>
<td>10.0</td>
<td>0</td>
<td>12.3</td>
</tr>
<tr>
<td>1974</td>
<td>81.2</td>
<td>3.9</td>
<td>13.9</td>
<td>0</td>
<td>18.8</td>
</tr>
<tr>
<td>1989</td>
<td>81.8</td>
<td>0.9</td>
<td>10.9</td>
<td>6.4a</td>
<td>18.2</td>
</tr>
<tr>
<td>1997b</td>
<td>79.0</td>
<td>na</td>
<td>na</td>
<td>0</td>
<td>21.0</td>
</tr>
<tr>
<td>2002</td>
<td>71.5</td>
<td>4.0</td>
<td>23.5</td>
<td>1.0c</td>
<td>28.5</td>
</tr>
<tr>
<td>Institutional membersd</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1933</td>
<td>70.6</td>
<td>1.6</td>
<td>27.9</td>
<td>0</td>
<td>29.4</td>
</tr>
<tr>
<td>1943</td>
<td>88.8</td>
<td>3.0</td>
<td>8.2</td>
<td>0</td>
<td>11.2</td>
</tr>
<tr>
<td>1953</td>
<td>61.4</td>
<td>4.4</td>
<td>34.2</td>
<td>0</td>
<td>38.6</td>
</tr>
<tr>
<td>1963</td>
<td>56.6</td>
<td>6.1</td>
<td>37.3</td>
<td>0</td>
<td>43.4</td>
</tr>
<tr>
<td>1966</td>
<td>48.1</td>
<td>6.4</td>
<td>45.5</td>
<td>0</td>
<td>51.9</td>
</tr>
<tr>
<td>1969</td>
<td>49.1</td>
<td>4.8</td>
<td>46.1</td>
<td>0</td>
<td>50.9</td>
</tr>
</tbody>
</table>

SOURCES: The 1933–69 membership data are from the American Economic Review (1970, 596); 1974 U.S. memberships are from the AER (1974, 565); 1974 non-U.S. members were hand tallied by the authors. 1997 data are from Siegfried (1998). 2002 data were hand tallied by the authors using country-by-country compilations of American Economic Association (AEA) members from AEA's online directory (AEA, no date).

NOTES: na, Not available.

a Includes members identifying a Middle Eastern, Eastern European, Former Soviet Union, or unspecified non-U.S. residence.
b Information on the country of residence of non-U.S. members was not given for 1997.
c Includes members identifying a Middle Eastern, Eastern European, or Former Soviet Union residence. For the years 1933–69, members from Eastern Europe, the Middle East, and Asia (including China, India, and Japan) were aggregated in the developed country total.
d Data on the regional distribution of institutional members after 1969 are unavailable.

awarded bachelor's and postgraduate (master's and doctoral) degrees in economics in the United States. These data, which are presented by gender for the period 1966 to 1998 in Figures 2.2a–d, are flow data rather than stock data; that is, they show the number of new economics degrees awarded, not the stock of people with degrees in economics.

In the late 1960s, in concert with a period of rapid growth in AEA membership, the total number of all degrees awarded annually in economics in the
FIGURE 2.2 Number of economics degrees awarded in the United States by sex of recipient for all degree levels

a. All degrees

Number of degrees

b. Bachelor's degrees

Number of degrees
FIGURE 2.2 Continued

c. M.S. degrees

Number of degrees

- Male
- Female

---

d. Ph.D. degrees

Number of degrees

- Male
- Female

---

United States expanded rapidly from 14,802 in 1967 to 21,688 in 1970. Between 1971 and 1976, this number declined to 19,632, but then it steadily increased over the next 15 years to 29,050 in 1990. In the early and mid-1990s, enrollment shrank, and the number of degrees awarded declined substantially to 21,668 in 1997, but recovered to 22,242 in 1998.

Most of the expansion in the mid-1970s and 1980s and the decline in the 1990s resulted from changes in the number of bachelor's degrees awarded in economics. Trends in the awards of postgraduate degrees may be better indicators of the number of economics research practitioners entering the job market. The number of recipients of master's and doctoral degrees in economics increased rapidly in the late 1960s and, while master's degree numbers remained constant after 1970, doctoral numbers continued to increase until 1973. Between 1973 and 1990, annual numbers of new master's and doctoral degree recipients remained relatively stable (at about 2,500 and 1,000, respectively), with the former expanding from about 2,500 to about 3,000 during 1991–94, and the latter increasing from about 1,000 to about 1,150 during 1990–98.

In both cases, these increases were driven almost entirely by steady and nearly uninterrupted increases in the number of women receiving master's and doctoral degrees in economics. In 1966, for example, women accounted for less than 10 percent of all master's degree recipients and less than 5 percent of all doctoral recipients; by 1998, women constituted about one-third of all new master's degree recipients and a quarter of all new doctoral recipients.4

To provide some context with respect to this gender disparity, it must be stated that the economics profession is much more male oriented than the social sciences in general, political science, or even engineering and other applied and basic sciences. In 1998, 47 percent of all social science degrees, 48 percent of all political science degrees, and 47 percent of engineering and science degrees were awarded to women (Hill 2001). In contrast, less than one-third of all economics degree recipients are women. Although the proportions of women receiving advanced degrees declines with the level of the degree in all three areas, women consistently constitute a smaller proportion of degree recipients in economics than in the social sciences in general or political science.5

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4. It should be noted that this implies that even today women constitute a relatively small proportion of all doctorates in economics, possibly less than 12 percent. In agricultural economics, for example, only 14 percent of AAEA members are women. This means that institutional goals of gender equity among employed economics Ph.D.s are infeasible for almost all research and educational organizations because there are not enough female Ph.D. recipients available. The problem is particularly acute at the more senior level, since even in the mid-1980s, the proportion of women among total recipients of postgraduate degrees in economics was much lower than in 1998. This dearth of qualified women in the economics profession is almost surely much more serious in most developing countries.

5. McDowell (1982) has examined the economic basis for differences in the proportions of women among academic disciplines.
International Evolution of the Economics Profession

An indicator of the evolution of a knowledge-generation and -transfer industry is the establishment and growth of professional societies linked to the industry. Figures 2.3a–c present new data on the global evolution and composition of professional economics associations. In 2000, 354 professional economics societies were identified as active in the Economic Associations and Societies Data Base (EASDB) constructed by the authors (IFPRI 2001). Information on the origins of each society is available for 277 of these organizations. These 277 observations were used to construct cumulative measures of the numbers of professional economics associations active in each year over the period 1777 to 2000. These cumulative data are presented in Figure 2.3a. They understate the actual number of professional associations active in each year for two reasons. First, data on the origins of 77 societies in our database (approximately 22 percent of the associations) could not be obtained. Second, some societies were established and then later merged or otherwise became defunct.

Associations of professional economists have been extant since at least 1777 (one year after the publication of Adam Smith’s Wealth of Nations), when the Real Sociedad Económica de Amigos del País de Tenerife (Royal Economic Society of Friends of Tenerife) was established in Spain. By 1900, at least

6. The Economic Associations and Societies Data Base (EASDB) developed for this study consists of information gathered on a total of 372 economic associations. The primary source of information on extant economic associations is the list of economic associations and societies compiled by Christian Zimmermann (2001). To construct data on each society’s date of origin, membership, geographic location, and disciplinary foci, the web site of each association was accessed and, where possible, the following information was recorded: year of foundation, function of the society, country in which the association is registered, the number of members at foundation, and the current (year 2000 or latest year available) number of members. In addition, the data set was augmented with information from the web site maintained by the Scholarly Societies Project (www.scholarly-societies.org) and the 47th edition of The World of Learning, published by Europa Publications in 1997. This publication, which consists of an inventory of academic institutions, learned societies, and international, cultural, scientific, and educational organizations worldwide, facilitated the identification of some existing associations that did not have web sites and were therefore omitted from Zimmerman’s listing of economic associations. For many societies, the data sought were not available through the organization’s web site or other published sources. Where this was the case, the society’s officers (president, secretary, and so on) were contacted and asked to provide the missing information. Comprehensive data on societies’ origins were obtained for 277 of the 354 organizations extant in 2000.

7. This society still exists, and in 1997 it had 490 members. Its focus is eclectic, and includes moral, material, cultural, and economics interests (The World of Learning 1997). The Nationale Nederlandsche Huishoudelijke Maatschappij (National Netherlands Economic Society) was also founded in 1777 as the Oeconomische Tak van de Hollandsche Maatschappij der Wetenschappen (Economic Branch of the Holland Society of Sciences, which itself was founded in 1752). In 1797, it became an independent society called the Nationale Nederlandsche Huishoudelijke Maatschappij (National Netherlands Economic Society), now called the Nederlandsche Maatschappij voor Nijverheid en Handel (Netherlands Society for Industry and Trade). We excluded it from our database, judging that, rather than being a scholarly society, it was more a civil society organization promoting discussion and consensus building and stimulating public and private economic initiatives in the Netherlands.
FIGURE 2.3 Professional economic associations by regions of the world and fields

a. Cumulative distribution of economic associations

<table>
<thead>
<tr>
<th>Year</th>
<th>Number of associations</th>
</tr>
</thead>
<tbody>
<tr>
<td>1777</td>
<td>00</td>
</tr>
<tr>
<td>1885</td>
<td>00</td>
</tr>
<tr>
<td>1909</td>
<td>00</td>
</tr>
<tr>
<td>1927</td>
<td>00</td>
</tr>
<tr>
<td>1940</td>
<td>00</td>
</tr>
<tr>
<td>1949</td>
<td>00</td>
</tr>
<tr>
<td>1958</td>
<td>00</td>
</tr>
<tr>
<td>1967</td>
<td>00</td>
</tr>
<tr>
<td>1975</td>
<td>00</td>
</tr>
<tr>
<td>1983</td>
<td>00</td>
</tr>
<tr>
<td>1991</td>
<td>00</td>
</tr>
<tr>
<td>1999</td>
<td>00</td>
</tr>
</tbody>
</table>

b. Cumulative distribution of economic associations by region

Number of associations

- United States
- Europe
- Other developed countries
- Developing countries

(continued)
c. Cumulative distribution of economic associations by fields

Number of associations

![Graph showing cumulative distribution of economic associations by fields.]

Source: Information derived from the Economic Associations and Societies Data Base (EASDB), compiled by the authors.

Notes: Associations for which the year of foundation was unknown or unavailable were excluded.

a Developing countries includes less-developed countries and transition economies.
b Developed countries includes international associations.
c Specialty excludes econometrics and agricultural, resources, and environmental economics (AREC) and includes the following fields of economics: mathematical economics, development economics, economic education, economic history, economic and game theory, financial economics, risk and insurance forecasting, business cycles, monetary economics, health economics, industrial economics, regulation, international economics, labor and demographic economics, law and economics, public economics, political economy, public policy, regional and urban economics, real estate, and welfare economics.

Between 1900 and 1940, when the number of professional associations across the world expanded from 15 to 42, the growth of professional economic associations was steady but relatively gradual. As illustrated in Figure 2.3b, in 1930, 85 percent of all economic associations were located in developed economies, with two-thirds of the total located in Europe and North America. New associations were created either as general economic societies to serve economists in different countries and regions or to serve economics practition-
ers in subdisciplines. In 1940, agricultural economics, with seven associations, and econometrics, with three associations, accounted for about half of the specialty societies.

Between 1940 and 1960, the number of professional economic associations more than doubled, to nearly 90 societies. Although many of the new associations were general economic associations serving new geographic (country or regional) entities, more addressed specialized areas or subdisciplines within economics such as agriculture, development, education, and political economy. This process of segmenting professional economic associations by regions and subdisciplines was probably in large part a consequence of three factors: (1) the expanded scope of the work of economists in international, national, and regional governments, and private-sector activities, coupled with potential gains from specialization within subdisciplines; (2) the sheer increase in the size of the profession (AEA membership, for example, also expanded markedly between 1940 and 1960 from 3,148 to 10,837 members), which both expanded the demand for professional publication outlets and may have created diseconomies of size with respect to some associations; and (3) rising per capita real incomes and per capita real research funds, which increased the effective demand for membership and participation in professional associations.

This process of expansion and geographic and subdiscipline specialization continued throughout the last 40 years of the twentieth century. By 2000, a total of 354 professional economic associations were functioning in over 60 countries. More than two-thirds of the associations (243) were subdisciplinary in orientation while less than one-third (111) were general economic societies. Over time, as the profession of economics developed a wider array of subdisciplines and subdisciplinary professional associations, the relative importance of agricultural, resource, and environmental economic professional associations declined. However, as the data presented in Figure 2.3c indicate, associations dealing with agricultural, resource, and environmental issues continued to increase, expanding from 12 societies in 1960 to 42 societies in 2000, mainly because of the establishment of new environmental and resource associations, rather than new agricultural economics associations.9

The overwhelming majority of professional economic associations are located in developed countries (246, or 69 percent). Only 55 associations—16 percent of the total, and generally not the largest associations—are located

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8. Egypt, in 1909, and India, in 1918, were the first of today's developing countries in the African and Asian continents to establish economic associations.

9. These totals exclude nine agricultural and resource economics associations whose date of foundation could not be determined.
in developing and transition economies (with economic associations in only 6 of the world's 64 low-income countries).\textsuperscript{10} The remaining 52 associations (15 percent) are explicitly international associations, only some of which have relatively large proportions of members from developing countries. This supports the view that, as is the case for many other knowledge-generation and -transfer industries, populations in poor countries are much more inadequately served by economists than are populations in rich countries. This shortfall may be particularly crucial if, as Harberger and others have argued, the need for well-trained economics policy practitioners in such settings is particularly great. The data presented in Figure 2.3a also indicate that the professional society gap between developed and developing countries has not diminished since the 1930s. Although the number of professional economic associations in the developed and developing countries has been growing at about the same rate, the absolute gap has been expanding.

\textit{Where Economists Work: Evidence from the United States}

Economists practice their professions in many different settings, and carry out many different functions. Since 1985, every four years the AEA has surveyed its membership located in the United States and published quadrennial data on the employment of members by sector. These data are presented in Table 2.2.

In 1985, among survey respondents, about 67 percent of AEA's 20,076 members held academic positions. Of the remaining 33 percent, approximately 10 percent worked in federal and state government agencies, about 14 percent worked in the commercial private sector (industry, finance, and consulting), and about 6 percent worked for international agencies, nonprofit corporations, and research institutes. Little changed between 1985 and 1997: by 1997, there had been a slight reduction in the proportion of survey respondents working in academic positions (to 64 percent), a small increase in the private commercial sector membership and in those working for international agencies and nonprofit corporations, and no change in the share working for government, which was 10 percent in both years.

The evidence from the AEA survey is limited in two dimensions. First, coverage of the profession is incomplete because the survey only covers AEA members, most of whom have postgraduate degrees and many of whom are active in advanced research. Second, the AEA survey response rate substantially declined between 1985 and 1997 from about 98 percent to 76 percent, and there is no clear information about self-selectivity bias with respect to occupation among nonrespondents.

\textsuperscript{10} The 64 low-income countries were classified as such by the World Bank (2001). They had per capita incomes in 2000 of less than $460 (in 1995 U.S. dollars).
TABLE 2.2 American Economic Association members by sector of employment (percent)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Academic</td>
<td>67</td>
<td>68</td>
<td>63</td>
<td>64</td>
<td>67</td>
</tr>
<tr>
<td>Nonacademic</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Federal government</td>
<td>8</td>
<td>7</td>
<td>8</td>
<td>8</td>
<td>7</td>
</tr>
<tr>
<td>State/local government</td>
<td>2</td>
<td>2</td>
<td>2</td>
<td>2</td>
<td>1</td>
</tr>
<tr>
<td>Business/industry</td>
<td>5</td>
<td>4</td>
<td>4</td>
<td>4</td>
<td>3</td>
</tr>
<tr>
<td>Banking/finance</td>
<td>4</td>
<td>4</td>
<td>4</td>
<td>4</td>
<td>3</td>
</tr>
<tr>
<td>Consulting</td>
<td>5</td>
<td>5</td>
<td>7</td>
<td>7</td>
<td>8</td>
</tr>
<tr>
<td>International agency or organizationa</td>
<td>2</td>
<td>2</td>
<td>3</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Nonprofit organization</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Research institution</td>
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<td>3</td>
<td>4</td>
<td>4</td>
<td>4</td>
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<tr>
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<td>2</td>
</tr>
<tr>
<td>Other</td>
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<td>2</td>
<td>1</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Nonacademic total</td>
<td>33</td>
<td>32</td>
<td>37</td>
<td>36</td>
<td>36</td>
</tr>
<tr>
<td>Total members</td>
<td>100</td>
<td>100</td>
<td>100</td>
<td>100</td>
<td>100</td>
</tr>
</tbody>
</table>

Sources: The 1985 data are from the *American Economic Review* (1985, 631–47); 1989 data are from the AER (1989, 613–647); 1993 data are from the AER (1993, 635); 1997 data are from the AER (1997, 674); 2003 data are from the American Economic Association web site www.vanderbilt.edu/AEA/TblEmploy.htm, last accessed September 4, 2003.

Notes: The 1985 and 1989 data for nonacademic members by sector of employment were hand tallied by the authors; academic members were implicitly calculated by subtracting the number of nonacademic members from the total 1985 and 1989 American Economic Association members reported.

*b* Nonprofit organization other than educational and research.

Less detailed but more representative data for the economics profession as a whole is provided in the Scientists and Engineers Statistical Data System maintained by the U.S. National Science Foundation Science Resources Studies Division. These data are reported for economists, political scientists, all social scientists, and all scientists and engineers for 1993 and 1997, including all persons working in the United States ever to receive a bachelor’s degree or higher in a science or engineering field, as well as those occupied in a science or engineering field without such degrees. The data show that, in 1993, only 15.1 percent of all economists were employed in educational institutions. In contrast, 53.4 percent were employed in business and industry and 31.5 percent in government. In 1997, the proportion of economists employed in education had declined to 9.5 percent and the proportion employed in government had declined to 27.5 percent, while the proportion employed in business and industry had increased to 63 percent.
In other social sciences, the distribution of the workforce across sectors is quite different (Table 2.3). In 1997, for example, 42 percent of political scientists were employed in government, 16.5 percent in education, and 42 percent in business and industry. In 1997, among all social scientists, 45 percent were employed in education, 43 percent in business and industry, and only 12 percent in government. In fact, in terms of their distribution across private and public sectors, economists are more like scientists and engineers, 70 percent of whom are employed in the private sector and 30 percent in education and government.

The Ethnicity of Economists

The AEA survey of members also provides information about the distribution of that part of the economics profession by gender and ethnic background. Among respondents to the survey’s gender and ethnicity questions in 1985, 14 percent of AEA members were female and 5.5 percent were black, Hispanic, or of other nonwhite ethnicity. In 1997, the proportion of AEA members who were women had increased very modestly to 14.3 percent, while the proportion who were black, Hispanic, or of other nonwhite ethnicity had increased to 13 percent.

Summary

Since its origins (mainly in the late 1800s), the economics research profession has expanded substantially, although with extended (decade-long) periods of stagnation in the 1930s, 1970s, and in the 1990s. Particularly since the 1940s, the profession has tended to become more fragmented as more subdiscipline affiliations have been created. This process of specialization was closely correlated with a period of rapid expansion in the aggregate size of the profession and in the scope of economics research between 1940 and 1970, although clearly with some lags. From a global perspective, economics research is concentrated in developed countries and probably closely aligned with the geographic division of the world’s economic activity. This means that, as with other knowledge-based professions, the world’s poor are poorly served by economists (notwithstanding the roles of several important international economic research organizations and associated spillovers from rich countries to poor countries).

The Agricultural Economics Research Industry

Arguably, the economics of agriculture was the first substantive subdiscipline to develop in the field of economics. The largely agrarian nature of economic activity and the importance of harvests for national economic welfare throughout Europe and the New World prior to the 1800s inevitably focused attention on agricultural commodity markets. The nineteenth-century transition of

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11. For example, in the United States in 1865, over 60 percent of the GDP was produced by agriculture.
<table>
<thead>
<tr>
<th>Field</th>
<th>1993</th>
<th>1997</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Business and industry</td>
<td>Educational institution</td>
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<td>40.6</td>
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</tr>
<tr>
<td>All sciences</td>
<td>67.8</td>
<td>17.8</td>
</tr>
</tbody>
</table>

**Source:** National Science Foundation (2001).
western Europe and North America to more urbanized and industrialized economies also raised crucial questions about the changing economic nature of agriculture and rural economies. In the twentieth century, critical questions were asked about the continued relative decline of agriculture in developed economies (often described as “The Farm Problem”) and the role of agriculture in the economic development of poor countries. More recently (certainly since the mid-1960s), concerns about the short-term and long-term impacts of agriculture on national and global environments have framed important debates about the economics of the food and fiber sector. This section therefore examines the growth and structure of the agricultural economics research industry.

Indicators of the Agricultural Economics Profession’s Growth

The American Agricultural Economics Association (AAEA) is somewhat congruent to the American Economic Association in that it is the largest association of professional agricultural economists, with a substantial international component to its membership. Thus, while it is an imperfect proxy for the worldwide growth of the agricultural economics profession, membership in the AAEA does provide an indicator of global as well as U.S. trends.

The AAEA is the oldest agricultural economics association. In 1910, one of its precursors, the American Farm Management Association, was established by agronomists. In 1917, members of the AEA (including Richard T. Ely) formed the American Association of Agricultural Economists. Two years later, in 1919, these two organizations merged to become the American Farm Economics Association, which rebranded itself as the American Agricultural Economics Association in 1968.

Data on membership in the AAEA are presented in Figure 2.4. It should be noted that, over the past 80 years, trends in AAEA membership have been closely linked to trends in AEA membership (the simple Pearson correlation coefficient between AAEA membership and AEA membership is 0.96), although there have been some subtle but important differences.

Upon its inception in 1919, the AAEA had a membership of 561 individuals. Membership peaked in 1929 at 974, but as the Great Depression took hold, membership in the AAEA declined (as did membership in the AEA) and

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12. Data on the distribution of membership in the AAEA by country of origin are available for the period 1968–2000 (AAEA 2001). In 1968, 20 percent (816 individuals) of AAEA members were from outside the United States. By 2000, the number of AAEA members resident in other countries had declined to 646, which was still about 20 percent of a declining total membership base.


14. The 1919 membership was not categorized according to individual and institutional members. In 1958, the first year that the membership was so categorized, there were only seven industry (or, in the parlance of that time, “sustaining”) members.
FIGURE 2.4 American Agricultural Economics Association and American Economic Association memberships

![Graph showing memberships of AAEA and AEA](image)

**SOURCE:** For the American Economic Association, see Figure 2.1. American Agricultural Economics Association membership data were obtained from the AAEA business office in Ames, Iowa.

**NOTE:** Coefficient of correlation of AAEA and AEA: 0.96.

did not recover to its 1929 peak until the end of World War II. Beginning immediately after World War II, agricultural economics experienced a 20-year period of substantial growth. Membership in the AAEA increased by almost 400 percent from 845 in 1945 to 4,180 in 1969.\(^1\)\(^5\) During the decade that followed, however, AAEA membership reached a somewhat erratic plateau (to some degree paralleling developments in AEA membership over the same period). The first 7 years of the 1980s, however, constituted a further period of expansion and AAEA membership increased from 3,910 individuals in 1980 to 4,904 individuals in 1987.

Then, between 1987 and 1997, there was a precipitous decline in AAEA membership from 4,904 to 3,431 individual members, in part because of a substantial cohort-retirement effect in U.S. land grant universities and a simultaneous

\(^{15}\) Over this 20-year period, the proportional expansion in AAEA membership was larger than the proportional expansion in AEA membership. This was also a period in which international economic development issues became important in public policy debates and received more attention in leading economics and agricultural economics journals.
cutback in U.S. government investments in agricultural economics research. As the data on AEA membership and the awarding of new economics degrees indicate, at least in the United States, the economics research industry was also contracting during the early and mid-1990s. However, the proportional decline appears to have been more substantial in agricultural economics.

This recent proportional decline in the size of the agricultural economics research profession relative to the economics research profession is really part of a long-run trend dating back to the late 1920s. Data on the ratio of AAEA members to AEA members are presented in Figure 2.5 for the period 1919 to 2000. Between 1919 and 1929, this ratio increased substantially from 0.25 to 0.35.16 Beginning in 1930, however, the ratio began to fall, and by 1945, it was approximately 0.2. The ratio of AAEA members to AEA members then increased steadily over the next 15 years to a peak of 0.3 in 1961, at least in part in response to increased funding for both international economic development research and research on domestic agricultural economics issues. Thereafter, this ratio decreased (with a brief interregnum in the mid-1970s), and by 2000 it had fallen to 0.17.

This decline in the relative importance of the agricultural economics research industry as a component of economics is linked to the decline in the importance of the agricultural sector in the overall economy. Figure 2.5 also shows the evolution of the share of GDP accounted for by the primary agricultural sector. As is well known, agriculture’s share of gross domestic product (GDP) has declined substantially. The correlation coefficient between the ratio of AAEA to AEA members and agriculture’s share of U.S. GDP is 0.49. This suggests that, although this link has been important, it is not the sole determinant of the relative demand for agricultural economics research. For one thing, agriculture’s role in developing economies, concerns about food security, and the developing role of downstream food processing have played important roles in determining the demand for agricultural economics research. In addition, as we will discuss below, over time, agricultural economists have adjusted their portfolio of research to include new issues such as food safety, resource management, and the environment.

Cross-Country Comparisons of Agricultural Economics Associations

Agricultural, resource, and environmental economists enjoy considerable overlap, both with respect to research issues and memberships in subdisciplinary associations. Data on the evolution of agricultural economics associations and resource and environmental economics associations are presented in Figures 2.6a,b.

16. If there were perfect overlap in membership in the AAEA and the AEA (in the sense that all AAEA members were also AEA members), this would imply that, in 1929, 35 percent of all AEA members were active in the AAEA; that is, agricultural economics research was a very important component of all economics research.
FIGURE 2.5 Ratios of American Agricultural Economics Association and American Economic Association members and agricultural gross domestic product to gross domestic product

SOURCES: For the American Economic Association, see Figure 2.1. For American Agricultural Economics Association membership data see Figure 2.4. GDP and AgGDP data for 1883–1959 are based on farm gross product, gross national product, and deflators from the U.S. Department of Commerce (1975); 1883–1925 deflators were obtained through a personal communication from the International Monetary Fund. GDP, AgGDP, and GDP deflators for 1960–98 are from the World Bank (1998).

NOTES: AgGDP, agricultural gross domestic product; GDP, gross domestic product. Coefficient of correlation of the ratios of AAEA to AEA members and AgGDP to GDP: 0.49.

Some time-series data on memberships in five leading agricultural economics associations are presented in Figure 2.7. In addition to the AAEA, these associations include the Australian Agricultural and Resource Economics Society (AARES), the British Agricultural Economics Society (AES), the Agricultural Economics Society of Japan (AES J), and the International Association of Agricultural Economists (IAAE). Memberships in the British and Australian societies have followed trends similar to AAEA memberships. The British AES was established in 1926 and the Australian AARES in 1957. In both cases, from inception to the mid-1960s, memberships increased and thereafter declined.

17. A cultural observation is that Americans and multinational groups merely associate while others form societies.
FIGURE 2.6 Distribution of agricultural, resource, and environmental economic associations

a. Distribution by time period

Number of associations

b. Distribution by region

Number of associations

SOURCE: Economic Associations and Societies Data Base, compiled by the authors.
FIGURE 2.7 Membership of selected agricultural economics associations

<table>
<thead>
<tr>
<th>Year</th>
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<th>IAAE</th>
<th>AESJ</th>
<th>AARES</th>
<th>AES</th>
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SOURCE: American Agricultural Economics Association (AAEA) membership data were obtained from the AAEA business office in Ames, Iowa. International Association of Agricultural Economists (IAAE) membership data were obtained from the Farm Foundation. Agricultural Economics Society of Japan (AESJ) membership data were obtained through a personal communication (Junichi Ito, June 2001). Australian Agricultural and Resource Economics Society (AARES) membership data for 1957, 1961, and 1966–80 are from AARES (1980); 1985–95 data are from the business manager's report to the 1996 annual general meeting (Arch 1996); 1997–2000 data are from AARES (2001). Agricultural Economics Society (AES) membership data are from Giles (2001).

NOTES: IAAE data were reported on a triennial basis beginning in 1951; annual data were therefore interpolated. AES data were reported every four or five years after 1928; annual data were therefore interpolated. AARES data were interpolated for 1958–60, 1962–65, 1981–84, and 1996.

The timing of substantial shocks to memberships in these societies was different, but, as Alston, Pardey, and Smith (1999) note, funding for agricultural R&D stagnated in both countries in the 1980s and the 1990s. The more recent history of agricultural economics in Japan is somewhat different. Between 1984 (the first year for which AESJ membership data are available) and 2000, membership in the AESJ increased by 18 percent from 1,249 to 1,476 individuals. In contrast, membership in the International Association of Agricultural Economists mimicked trends in AES and AARES membership, with little or no change over the past three decades.

One useful indicator of both research output and trends in industry size is the annual number of doctoral degrees awarded. Data on the number of agricultural economics and economics doctoral degrees granted in the United States are presented on a decennial basis in Figure 2.8. In 1920, three doctoral degrees were awarded in agricultural economics, accounting for 7.5 percent of the
FIGURE 2.8 Ph.D. dissertations in agricultural economics and economics in the United States

Number of degrees awarded

40 doctoral degrees awarded in economics. The number of agricultural economics doctoral degrees increased only very gradually until 1950, when 18 degrees were awarded (compared with 397 doctoral degrees awarded in economics). However, roughly in concert with the expansion of AAEA membership, the annual number of doctoral degrees awarded in agricultural economics grew rapidly over the next two decades, and, in 1970, a total of 216 degrees were awarded. U.S. doctoral programs in agricultural economics continued to expand after 1970 until the late 1980s, and 310 degrees were awarded in 1990. In the 1990s, enrollments in agricultural economics doctoral programs declined, and in 1999 only 260 individuals received doctoral degrees in the subdiscipline.

Whether this implies that agricultural economics research training has become less important than economics research training is another matter. In fact, the ratio of agricultural economics doctorates to economics doctorates has fluctuated somewhat over the entire 80-year period, generally increasing between
1950 and 1980 (from 4.5 percent to 18.2 percent) and thereafter declining steadily over the next two decades to 12.7 percent in 1999.

Some insights about the international distribution of graduate student agricultural economics research are provided in Table 2.4. Among a group of 14 countries (all of which are located in Europe and North America, with the exception of South Africa), doctoral-granting institutions in the United States accounted for 84 percent of all agricultural economics doctoral degrees (310 out of 367 degrees) in 1990, and 92 percent in 1999. Although many U.S. agricultural economics degrees were awarded to citizens of other countries, these data highlight the preeminence of U.S. graduate programs in this field.

The Changing Composition of Economics Research

Economists provide education and research on a wide array of issues in a wide variety of settings. How they allocate their resources across different areas of research and education is also important. Table 2.5 and Figure 2.9 present data (on a decennial basis) that describe the changing distribution of journal space among the ten major Journal of Economic Literature (JEL) categories over the period 1930 to 2000 in (arguably) the three major economics journals published continuously over this period. The three journals are the American Economic Review (AER), the Journal of Political Economy (JPE), and the Economic Journal (EJ). Two of the journals (AER and JPE) are published in America, and the third (EJ) is published in Britain. Over this period, all three journals published articles across a broad spectrum of economics research issues that were viewed by editors and referees as making substantial contributions to the discipline of economics. The ten JEL categories for articles published in the three journals are shown in Table 2.5. First, changes in the distribution of journal space for each of these categories are described. Then, some comparisons across subject areas are made.

The size of the economics profession has substantially expanded since the 1930s. Memberships in professional economics organizations increased dramatically, as did the number of professional journals dealing with economics. Moreover, the size of many journals, crudely measured by the annual number of pages published, also increased. For example, the total number of pages published annually in the three journals represented in Table 2.5 more than tripled from 1,518 pages in 1930 to 4,721 pages in 2000. Clearly, the total amount of research effort invested in almost all categories of economics research

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18. A more ideal measure of journal size may be word count, as many journals have changed their formats and type sizes over the years. Other changes, including the increased use of mathematics and graphics, have also occurred that may have more subtle effects that bias interpretations based solely on measurement of the sheer volume of publications.
TABLE 2.4 Doctoral dissertations in agricultural economics, selected countries

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**SOURCE:** Hand tallied by authors from the database “Dissertation Abstracts Online.” This database is available for subscribers of OCLC (Online Computer Library Center) at www.oclc.org/home/. Dissertation Abstracts Online includes over 1.6 million master's theses and doctoral dissertations for selected countries from 1861 to the present.

**NOTES:** A blank entry indicates that data were not available. To construct this table, Dissertation Abstracts Online was queried by year and the abstract and institutional details obtained thereby were used to compute totals by year and country.
## TABLE 2.5 Distribution of journal space in the *American Economic Review*, the *Journal of Political Economy*, and the *Economic Journal* by subject category (percent of total pages)

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<td>All general economics; theory; history; systems</td>
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<td>30.9</td>
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<td>17.3</td>
<td>18.3</td>
</tr>
<tr>
<td>9</td>
<td>Welfare programs; consumer economics; urban and regional economics</td>
<td>0.9</td>
<td>0.9</td>
<td>1.0</td>
<td>10.3</td>
<td>8.9</td>
<td>8.2</td>
<td>6.6</td>
<td>13.0</td>
</tr>
<tr>
<td></td>
<td><strong>Total number of pages</strong></td>
<td>1,518</td>
<td>1,777</td>
<td>2,174</td>
<td>2,602</td>
<td>2,992</td>
<td>2,994</td>
<td>4,057</td>
<td>4,721</td>
</tr>
</tbody>
</table>

**Source:** Authors' hand tallies are based on all the issues of the *American Economic Review*, the *Journal of Political Economy*, and the *Economic Journal* published in each of the decennial years.
FIGURE 2.9 Proportion of economics journal space by subject category

a. All general economics, theory, history, systems
Percent

f. Administration, business finance, marketing, accounting
Percent

g. Industrial organization, technological change, industry studies
Percent

b. Economic growth, development, planning, fluctuations
Percent

c. Economic statistics
Percent

h. Agriculture, natural resources
Percent

d. Monetary and fiscal theory and institutions
Percent

i. Manpower, labor, population
Percent

e. International economics
Percent

j. Welfare programs, consumer economics, urban and regional economics
Percent

SOURCE: Authors' compilation from all the issues of the AER, JPE, and EJ published in each of the decennial years.
increased substantially over the past 70 years, and has had important implications for the diversity of economics research.

The main focus in this section is the changing proportion of journal space allotted to different categories and the broader economic policy and economic theory developments linked to these changes. The proportion of journal space allocated to category 0 (general economics, theory, history, systems) has generally decreased (Figure 2.9). It was much larger prior to and immediately after the post–World War II period than subsequent to the 1960s. In 1930 and 1940, for example, the category accounted for about 31 percent of total journal space. In the 1960s, however, category 0 accounted for about 5 percent of journal space. Category 0's share recovered somewhat to 19.1 percent in 1970, but thereafter declined, falling to 14.7 percent in 2000. The relative decline since 1950 in the proportion of space allocated to theory, history, and systems in the three major journals is largely explained by the more rapid growth of applied research in other areas.

To some degree (as with other categories), the proportion of space allocated to general theory may have declined because of changes in labeling practices over time that shifted papers with theoretical innovations to subdiscipline categories such as labor and natural resources. In addition, the introduction of journals explicitly devoted to theoretical innovations such as the *Journal of Economic Theory* and the *Journal of Mathematical Economics* provided alternative outlets for this type of research. However, the resources allocated to economic history have probably not increased at the same rate as resources designated for other subdisciplines within economics. This was also the case with economics systems research, at least until the late 1980s and 1990s, when the demise of the Soviet system and the development of transition economies provided some stimulus for work in this area.

The share of journal space allocated to category 1 (economic growth, development, planning, fluctuations) changed over the period, initially increasing from 4.5 percent in 1930 to 22.5 percent in 1960 and, thereafter, declining to 7.3 percent in 2000. The expansion between 1940 and 1960 probably reflects the economics profession's interest in economic growth and economic development during this period. For example, Robert Solow's Nobel Prize–winning work on economic growth and technical change was published in 1957, as was Zvi Griliches's seminal study of hybrid corn; Joan Robinson's work on golden age growth was published in 1962. In addition, funding for research on economic development issues began to flow from post–World War II multinational institutions such as the World Bank and from national agencies such as the U.S. Agency for International Development (USAID). The share of journal space allocated to category 1 declined after 1960, perhaps in part because of a reduced growth rate of funding for development economics research (especially in the late 1970s and early 1980s). The modest increase in journal space allocated to economic growth and development in 1990 (11.9 percent) as compared with 1980 (9.3 percent)
may have been associated with a surge in theoretical and computational studies of economic growth and the role of real business cycles in determining economic fluctuations. In addition, the apparent decline in long-run economic growth rates in the 1970s also engendered research programs addressing both the determinants of economic growth and the measurement of economic growth.

Journal space allocated to category 2, economic statistics, has also fluctuated over the 70-year period. About 7 percent of journal space was allocated to economic statistics in 1930 and 1940, with somewhat less space in 1950. By 1960, however, economic statistics accounted for over 13 percent of journal space. This increase is not surprising. As improved data sets became available and the accessibility of computational power increased during the 1950s, the innovation and adoption of quantitative, econometric, and other statistical methods became more extensive and important. However, by 1970, the proportion of space allotted to category 2 had decreased to 8.5 percent and continued to decline to about 4.1 percent in 2000. The decrease in space allocated to economic statistics after 1960 does not imply that innovations in this area became less frequent or less important. Rather, it may well reflect an expansion since 1960 in the number of specialist and high quality journals devoted to the field (including the *Journal of Econometrics* and the *Journal of Business and Economic Statistics*), the more frequent incorporation of econometric and other quantitative innovations in articles dealing with applied areas in other categories, and the expansion of economics into new areas such as consumer economics and the economics of the family.

The proportions of total pages in the three journals allocated to category 3 (monetary and fiscal theory and institutions) in 1930 (16.4 percent) and 1940 (also 16.4 percent) were much higher than in 1950 (8.7 percent). The relatively high proportion of space allocated in 1940 may reflect the increased interest in the potential role of government in the macroeconomy that followed the onset of the Great Depression and the publication of Keynes's *General Theory of Employment, Interest and Money* in 1936. Between 1950 and 1970, the share of total space allocated to category 3 steadily increased. This was at least a partial consequence of the expansion in the proportion of teaching and research effort allocated to macroeconomic issues and the development and expansion of economics research programs at both public institutions (such as the Board of Governors of the Federal Reserve System and individual federal reserve banks) and private research organizations (such as the Brookings Institution).20

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19. In this context, it is interesting to note that between 1930 and 1995, the *Review of Economics and Statistics* expanded from 195 pages to 780 pages, a larger proportionate increase than the increase in the total number of pages in the *AER, JPE, and EJ*.

20. Although the Brookings Institution was founded in 1916, it was in the 1950s, under the leadership of Robert Calkins, that the institution was reorganized around three program areas, including the Economic Studies program. The *Brookings Papers* were first published in 1970.
By 1980, category 3’s share of total journal space had increased by about half to 18.3 percent. At about this time, important innovations were taking place in macroeconomic analysis, including the development of Sargent and Wallace’s work on the rational expectations hypothesis (1976), the related development of the Lucas critique of more traditional macroeconomic policy prescriptions (1976), and a substantial expansion of work on monetary theory, the demand for money, and the policy relevance of alternative monetary aggregates. In addition, during the 1970s, many countries experienced substantial periods of stagflation (the joint occurrence of increasing unemployment and increasing inflation rates), relatively high rates of inflation, and substantial increases in natural rates of unemployment. All of these phenomena stimulated research into the behavior of the macroeconomy. Although the proportion of space allocated to macroeconomic policy issues declined somewhat in 1990 to 14.9 percent, in 2000 it had again increased to 18.1 percent. It seems clear that, since 1970, the economics profession has, as a proportion, paid considerably more attention to macroeconomic issues than was the case between 1950 and 1970.

The proportion of total journal space allocated to category 4, international economics, was relatively stable between 1930 and 1950, ranging only from 7.8 percent in 1930 and 1940 to 8.4 percent in 1950. However, by 1960, the share of total journal space allocated to international economics had increased to 12.2 percent. As was the case with economic development, this expansion came in part as an outgrowth of the increased demand for research on international economic issues. Policymakers had became more engaged in international trade and finance issues in the post–World War II era, and institutions such as the General Agreement on Tariffs and Trade (GATT) and the International Monetary Fund (IMF) were created and their remits steadily expanded.\footnote{The IMF was founded in 1946 and the first GATT was signed in 1947.} After 1980, the share of space allocated to international economics declined and, by 2000, category 4 accounted for 9 percent of the pages published in the three journals. This proportional decline does not necessarily mean that fewer resources were allocated to international economics research, but rather that other areas of research may have expanded more rapidly and that work in international economics may have become more applied, shifting publications to other outlets.

Category 5 (administration, business finance, marketing, accounting) enjoyed a checkered history with respect to the space allocated to these fields in the three leading economics journals. Prior to 1960, relatively little space in these journals (no more than 2.3 percent) was allotted to these topics. Beginning in 1960, however, category 5 accounted for at least 3 percent of total journal pages and, in 1990, this category’s share of journal space was 9.3 percent. A considerable part of the expansion since 1960 can certainly be attributed to developments in financial economics, perhaps especially in the 1960s and 1970s.
as the work of economists such as Black and Scholes (1973) and Merton (1973) on options and other derivatives came into vogue. The volatility in the share of space allocated to this category (which was 3.5 percent in 1960, 7.2 percent in 1970, 3.4 percent in 1980, 9.5 percent in 1990, and only 3.1 percent in 2000) may be due in part to the development and increased prestige of alternative outlets for such research.22

Between 1930 and 1950, category 6 (industrial organization, technical change, industry studies) occupied relatively large proportions of total journal space, ranging from about 17 percent in 1930 and 1940 to 12.4 percent in 1950. Much of the research on industrial organization in the 1930s and 1940s was institutional in nature. By 1960, the proportion of total space allocated to category 6 had declined to 7.1 percent, and was quite similar in 1970. By 1980, however, category 6’s share of journal space had increased to 13.2 percent and in both 1990 (9.4 percent) and 2000 (10.8 percent) was substantially higher than in 1960 and 1970. Part of the relatively recent expansion in research in this category can be attributed to innovations in the analysis of imperfect competition (including the development of empirical and game theoretic and computational models of oligopoly). Part may be associated with increased interest in the economics of antitrust and other forms of industry regulation that derived from the work of economists such as Buchanan and Tullock (1962) in the 1960s and 1970s on government failure as opposed to market failure.

In both 1930 and 1940, category 7 (agriculture, natural resources) accounted for 5.4 percent of total space in the three leading economics journals. This was the period of the Great Depression, when major proposals for new agricultural policies were being widely discussed and then implemented in both the United States and the United Kingdom, and when agriculture accounted for over 20 percent of U.S. GDP. Between 1940 and 1980, the share of total journal space allocated to agriculture and resources declined fairly consistently until, in 1980, the category accounted for only 0.6 percent of journal space. In 1990 and 2000, category 7’s share of journal space in the three leading journals increased to 1.7 percent.

The share of total journal space allocated to category 8 (manpower, labor, population) was about 9.5 percent in both 1930 and 1940. Category 8’s share of total journal space declined to about 7 percent in 1950, increased to 11.1 percent in 1960, possibly because of increased research on the determinants of unionization, but then further declined to 6 percent in 1970. After 1970, however, the share of total space allocated to studies in manpower, labor, and pop-

22. For example, the Journal of Finance has only relatively recently (in the last two decades) become a top-ranked journal for economists. The Journal of Financial Economics, which was ranked an average of third among all economics journals in five widely quoted citations indexes in 2001, was introduced only in 1974 (www.jfe.rochester.edu/ssci.htm).
ulation increased substantially, rising to 14.2 percent in 1980, 17.3 percent in 1990, and 18.3 percent in 2000. Beginning in the 1970s, rich panel data sets on household participation in the workforce became available for the analysis of a wide range of issues, leading in part to innovations in econometric methodologies tied to labor economics issues. In addition, during the last 30 years of the twentieth century, important economic questions involving differences in education, employment, and wages across age, gender, and race became more prominent political issues that consequently attracted increased economics research resources.

Category 9 (welfare programs, consumer economics, urban and regional economics) received almost no attention prior to the 1950s. Fewer than 1 percent of the total pages available in the three leading economics journals were allocated to these topics in 1930, 1940, and 1950. By 1960, the picture had changed dramatically—category 9 accounted for 10.3 percent of total journal space in that year. Subsequently, between 1960 and 2000, welfare programs, consumer economics, and urban and regional economics accounted for at least 6.6 percent of total journal space (in 1990), and in 2000 it accounted for 13 percent of total journal space. These research areas were clearly not widely investigated prior to the 1950s, at least in part because there was relatively little government policy in these areas. In the 1950s and 1960s, the confluence of important policy debates and actions, increasing affluence, and rapid urbanization increased research funding in these areas. Moreover, in the 1960s and 1970s, Becker (1976) developed and disseminated models of household production, and in the process virtually created the subdiscipline of the economics of the family. The substantial increase in journal pages allocated to category 9 in the 1990s is partly linked to the expanded interest in welfare reform and the implementation of new social security and unemployment compensation programs in the United States and other developed economies.

Summary

Overall, the data show that two of the ten JEL categories—category 8 (manpower, labor, population), and category 9 (welfare programs, consumer economics urban and regional economics)—have substantially and fairly consistently increased their share of total pages published in the three leading economics journals since 1950 and both have much larger shares than they enjoyed in 1930 and 1940. In contrast, the proportions of total journal space allocated to category 7 (agriculture, natural resources), category 0 (general economic, theory, history, systems), and category 2 (economic statistics) declined relative to the 1940s, although the patterns of adjustment have differed within each of these categories. Other categories have been more variable. The share of total journal pages allocated to category 1 (economic growth, development, planning, fluctuations) increased between 1930 and 1960, but declined thereafter. In contrast, the share of total journal pages allocated to category 6 (industrial organization,
technological change, industry studies) waned between 1930 and 1960 and then waxed between 1960 and 2000, and category 3 (monetary and fiscal theory and institutions) followed a similar path.

While the data presented in Table 2.5 and Figure 2.9 are subject to important caveats, the following three observations may at least be reasonably accorded the status of working hypotheses. First, as in many other fields, research output in the leading economics journals is quite closely linked to policy debates and related research funding. Second, economics is no less subject to the impact of major analytical advances than other disciplines. Innovations in economic theory, empirical methods, and measurement (new and more accurate data sets) have had important effects, as changes over time in the proportion of space allocated to macroeconomics and labor economics suggest. Third, the development of competing outlets for research also affects the volume of publications in the three leading economics journals (as suggested by the behavior of category 6: administration, business finance, marketing, accounting). The final observation may be more of a truism than a working hypothesis. Whatever else can be said of economists, as a profession they are not static, neither with respect to the issues they address nor the ways in which they address them.

The Changing Composition of Agricultural Economics Research

Agricultural economics research has been no less dynamic than economics research. Decennial data on the allocation of space for different areas of agricultural economics research in the *American Journal of Agricultural Economics (AJAE)* for the 70-year period 1930 to 2000 are presented in Table 2.6. The *AJAE* has unambiguously been and remains the leading journal in the world specializing in agricultural economics, and the research published in the journal is drawn from a wide area of research topics, reflecting areas of emphasis and innovation within the subdiscipline. Table 2.6 and Figure 2.10 provide data on 14 aggregates of *JEL* two- and three-digit categories mainly within *JEL* category 7: agriculture and natural resources.

In some cases, these categories are “stand-alone” *JEL* two- or three-digit categories (for example, agricultural demand and supply analysis and economic statistics). In others, they are aggregates of two or more two- or three-digit subcategories within *JEL* category 7. As with the three major economics journals, we first describe changes in the distribution of journal space within the *AJAE* for each of these categories. Then, we make some comparisons across subject areas.

The data presented in Table 2.6 and Figure 2.10 show that the proportion of total space in the *American Journal of Agricultural Economics* (called the *Journal of Farm Economics* prior to 1968) allocated to the category of general, growth, and macroeconomics has often been rather substantial, ranging from 7.8 percent in 1950 to 28.2 percent in 1970. The relative importance of this
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</tr>
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<td>1</td>
<td>General, growth, and macroeconomics</td>
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<tr>
<td>2</td>
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<td>7.3</td>
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<td>8.4</td>
<td>16.4</td>
<td>16.7</td>
<td>17.6</td>
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</tr>
<tr>
<td>3</td>
<td>International economics</td>
<td>9.8</td>
<td>1.4</td>
<td>5.6</td>
<td>2.9</td>
<td>0.5</td>
<td>6.5</td>
<td>3.2</td>
<td>8.5</td>
</tr>
<tr>
<td>4</td>
<td>Industrial organization; technological change; industry studies</td>
<td>2.1</td>
<td>1.6</td>
<td>4.1</td>
<td>12.9</td>
<td>6.6</td>
<td>2.4</td>
<td>5.5</td>
<td>6.8</td>
</tr>
<tr>
<td>5</td>
<td>Agriculture: general and situation and outlook</td>
<td>19.7</td>
<td>7.5</td>
<td>8.4</td>
<td>2.1</td>
<td>5.8</td>
<td>5.3</td>
<td>7.4</td>
<td>4.1</td>
</tr>
<tr>
<td>6</td>
<td>Agricultural demand and supply analysis</td>
<td>7.6</td>
<td>6.6</td>
<td>11.1</td>
<td>12.0</td>
<td>12.9</td>
<td>8.5</td>
<td>11.2</td>
<td>10.3</td>
</tr>
<tr>
<td>7</td>
<td>Agricultural policy</td>
<td>12.7</td>
<td>9.9</td>
<td>12.0</td>
<td>10.5</td>
<td>9.5</td>
<td>11.9</td>
<td>10.1</td>
<td>9.4</td>
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<td>Agricultural finance</td>
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<td>6.5</td>
<td>0.4</td>
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<td>9</td>
<td>Agricultural marketing</td>
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<td>4.8</td>
<td>8.9</td>
</tr>
<tr>
<td>10</td>
<td>Farm management and business administration</td>
<td>7.9</td>
<td>7.6</td>
<td>11.3</td>
<td>5.7</td>
<td>4.7</td>
<td>2.1</td>
<td>3.0</td>
<td>11.9</td>
</tr>
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<td>Land, natural resources, and conservation</td>
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<td>10.5</td>
<td>11.7</td>
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<td>4.0</td>
<td>7.4</td>
<td>5.9</td>
<td>5.0</td>
</tr>
<tr>
<td>12</td>
<td>Economic geography</td>
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<td>0.0</td>
<td>6.4</td>
<td>1.5</td>
<td>0.0</td>
<td>0.1</td>
<td>0.8</td>
<td>0.0</td>
</tr>
<tr>
<td>13</td>
<td>Manpower; labor; population</td>
<td>0.0</td>
<td>13.1</td>
<td>1.9</td>
<td>2.7</td>
<td>4.9</td>
<td>1.8</td>
<td>1.5</td>
<td>1.0</td>
</tr>
<tr>
<td>14</td>
<td>Welfare programs; consumer economics; urban and regional economics</td>
<td>2.3</td>
<td>3.2</td>
<td>2.7</td>
<td>7.3</td>
<td>4.7</td>
<td>5.9</td>
<td>2.7</td>
<td>5.0</td>
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<tr>
<td>Total pages</td>
<td></td>
<td>529</td>
<td>708</td>
<td>1,047</td>
<td>1,351</td>
<td>770</td>
<td>892</td>
<td>1,094</td>
<td>1,207</td>
</tr>
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</table>

Source: Authors' hand tallies based on all the issues of the *American Journal of Agricultural Economics* published in each of the decennial years.
FIGURE 2.10 *American Journal of Agricultural Economics* proportion of journal space by subject category

a. *AJAE*: general, growth, and macroeconomics

b. Economic statistics

c. International economics

d. Industrial organization, technological change, industry studies

e. Agricultural: general situation and outlook

f. Agricultural demand and supply analysis

g. Agricultural policy

h. Agricultural finance

i. Agricultural marketing

j. Farm management and business administration

(continued)
category is not surprising, as it embraces topics in general economics, including theory, history, economic systems, economic growth, development, planning, and macroeconomic fluctuations. Prior to the 1950s, the proportion of space allocated to these topics was generally lower (ranging from 13 percent in 1930 to 14.3 percent in 1940) than after the 1950s, when the category's share of total space in the *AJAE* was generally about 20 percent (ranging from 17.5 percent in 1960 to 28.2 percent in 1970). The expansion of interest in links between agriculture and economic growth, development, and planning in the 1950s may account for part of the increase that occurred in the decades of the 1950s and 1960s. Agricultural economists also became engaged in macroeconomic issues in the 1980s, as links between agricultural commodity and asset prices and inflationary processes and other macroeconomic indicators became the subjects of research programs.

Within agricultural economics, economic statistics and quantitative methods have been of considerable importance because of the heavily applied focus of the subdiscipline. Over the entire 70-year period, the proportion of the *AJAE* accounted for by the category of economic statistics ranged from a low of 4.1 percent in 1950 to a high of 17.6 percent in 1990. The low proportion of articles on economic statistics in 1950 may be explained by the impact of World War II on the availability of data and a related lag in the development
of statistical methods. The expansion of space given to the economic statistics category in 1960 (8.4 percent) and 1970 (16.4 percent) and the sustained relative importance of the category over the next 30 years is certainly linked to the simultaneous development of computational power, data, and econometric and other quantitative techniques that occurred in the 1950s and 1960s, as well as to the continued innovations in these techniques since then. The decline in the relative importance of this category in 2000 may be related to a shift in focus to simulation models and computational techniques.

The amount of space allocated to international economics has fluctuated in relative importance between 1930 and 2000 (Figure 2.10), but was highest in the first and last years (9.8 percent in 1930 and 8.5 percent in 2000). This is not a complete surprise, because trade issues were of considerable importance in the late 1920s, at a time of national debate over the need for increased domestic protection. These issues again became important in the late 1990s, as policymakers became engaged with trade liberalization issues in relation to the impacts of the 1994 GATT agreement and new GATT and other multilateral trade policy initiatives. Over the entire 70-year period, the proportion of space allocated to international economic issues ranged from 0.5 percent in 1970 to 9.8 percent in 1930, but there is no discernible long-term trend. However, international agricultural economics issues appear to have received more journal space when agricultural trade has been on the minds of policymakers. In 1950, for example, just two years after the first GATT agreement, 5.6 percent of journal space was allocated to trade issues.

The category of industrial organization, technological change, industry studies has also exhibited considerable variation, accounting for only 1.6 percent of the AJAE in 1940, but more than 12.9 percent in 1960, and 6.8 percent in 2000. The work of Theodore Schultz (1953) and others on technological change and agricultural labor markets in the 1950s may in part account for the large proportion of space allocated to this category in 1960. The steady increase in the importance of this category between 1980 (when its share was only 2.4 percent) and 2000 (when its share was 6.8 percent) may also be associated with a growth in the body of work on the economics of technical change and the economic returns to agricultural research.

The proportion of space allocated to the next category, agriculture: general and situation and outlook, has generally been lower since the 1950s. This category accounted for 19.7 percent of journal space in 1930, 7.5 percent in 1940 and 8.4 percent in 1950. Since then, its largest share was 7.4 percent in 1990, and over the period 1960 to 2000 its allotment ranged from 2.1 percent in 1960 to 7.4 percent in 1990. In 2000, the category accounted for only 4.1 percent of total journal space in the AJAE. The decline in the relative importance of situation and outlook material after 1950 is not surprising. Prior to this time, many innovations concerned the development of new data sets and their interpretation. After 1950, as discussed above, innovations in econometric and other
quantitative methods and their application within agricultural, environmental, and resource contexts became a larger focus of agricultural economics research.

The analysis of agricultural markets in the context of demand and supply analysis has been an important component of agricultural economics research throughout the period 1930 to 2000. The share of total journal space allocated to the category of agricultural supply and demand analysis ranged from 6.6 percent in 1940 to 12.9 percent in 1970. Since the 1940s, between 10 and 12 percent of the *AJAE* has been regularly given over to such studies. This is not surprising, as the performance of agricultural commodity markets has been of ongoing concern to policymakers and research resources have been available for work in this area.

In the same way, the share of total journal space in the *AJAE* allocated to the category of agricultural policy has been very stable, ranging from 9.4 percent in 2000 to 12.7 percent in 1930. The relatively large proportion of journal space allocated to this category in 1930 (12.7 percent), 1950 (12 percent), and 1980 (11.9 percent) may be explained in part by the fact that these were each years during which policymakers were engaged in major U.S. agricultural policy debates. However, research resources for agricultural policy analysis have been available on a consistent basis throughout the past 70 years.

The amount of *AJAE* space allocated to the category of agricultural finance fluctuated considerably over the period, ranging from 0.4 percent in both 1950 and 1970 to 6.5 percent in 1940. The impact of policy debates on the amount of research in this area has probably been substantial. In the mid- and late 1930s, key elements of the U.S. Farm Credit System were established. Similarly, the late 1970s and early and mid-1980s were years of relative instability in agricultural asset values, while major changes in the U.S. Farm Credit System were considered and implemented in the late 1980s. In comparison, agricultural finance issues received much less attention in the mid- and late 1960s and the 1990s.

The category of agricultural marketing has also exhibited considerable variation in terms of journal space, ranging from only 1.4 percent in 1970 to 12.5 percent in 1950. Until 1950, agricultural marketing generally accounted for about 10 percent of journal space in the *AJAE*, but by 1960 its share had declined substantially to 3.9 percent and that share further declined to only 1.4 percent in 1970. Since then, agricultural marketing issues have received more attention, and this category's share of *AJAE* journal space increased to 5.9 percent in 1980, remained relatively stable at 4.8 percent in 1990, and then further increased to 8.9 percent in 2000. This expansion may have been associated with the development of futures and options markets for agricultural commodities and the innovations by economists such as Black, Scholes, and Merton in economic analyses of related markets for financial derivatives that could be applied in an agricultural commodity context. In addition, through private institutions such as the Chicago Board of Trade and, more recently, publicly funded research
initiatives on risk management issues, increased research resources may have been provided to this area of agricultural economics over the past 25 years.

The data on journal space allocated to the category of farm management and business administration indicate that, prior to the 1950s, more journal space was allocated to this category than after the 1950s, with the exception of 2000. From 1930 to 1950, between 7.6 percent and 11.3 percent of the AJAE was allocated to this category. In 1960, the category's share of journal space declined to 5.7 percent and by 1980, it had fallen even further to only 2.1 percent. In 2000, however, almost 12 percent of the AJAE was allocated to this category, mainly because of a substantial increase in studies on farm management (possibly associated with research on crop and revenue insurance, an area that received substantial USDA funding in the late 1990s).

Data on land, natural resources, and conservation indicate that while the proportion of the AJAE allocated to this category was very small in 1930 (1.1 percent), these topics were important throughout the 1930s, 1940s, and 1950s, and during this period the category's share of total journal space was consistently in excess of 10 percent. Certainly, land use, natural resource, and conservation issues were important during these periods as, at least periodically, public policy issues focused on concerns such as the Soil Bank program, soil erosion, and related issues. However, even the most casual observer would expect proportionally more attention to have been given to resource and environmental issues by agricultural economists during and after the 1960s, as those issues appeared to receive a much higher political profile. Moreover, in the 1980s and 1990s, several agricultural economics departments deliberately chose to rebrand themselves as departments of agricultural and resource economics, typically to "better reflect" the implications of changes in the composition of their research and teaching activities and to acquire grants and contracts in the areas of resources and the environment. Paradoxically, during this period, the share of the AJAE allocated to these issues actually declined to 4 percent in 1970 and, although it increased to 7.4 percent in 1980, it declined again to 5.9 percent in 1990 and 5 percent in 2000.

The data presented in Table 2.7 on the share of total pages published in the Journal of Environmental Economics and Management (JEEM) by faculty in land grant institution departments of agricultural economics provide at least a partial resolution of this paradox. In 1974, the first year in which JEEM was published, faculty from agricultural economics departments accounted for only 10 percent of JEEM's total journal space. This proportion steadily increased to 16.4 percent in 1980, 23.7 percent in 1990, and 27.5 percent in 2000. This evidence suggests that the creation of journals such as JEEM that are devoted to

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23. For example, the U.S. Environmental Protection Agency was created in 1970, and in the United Kingdom, a separate Department of Environment was not created until later that decade.
TABLE 2.7 Proportion of total pages published in the Journal of Environmental Economics and Management by faculty in agricultural economics departments

<table>
<thead>
<tr>
<th>Year</th>
<th>Pages published by faculty in agricultural economics departments (percent)</th>
<th>Total pages published in JEEM</th>
<th>Proportion of total JEEM pages published by faculty in agricultural economics departments (percent)</th>
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<tbody>
<tr>
<td>1974</td>
<td>33</td>
<td>331</td>
<td>10.0</td>
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<tr>
<td>1980</td>
<td>66</td>
<td>402</td>
<td>16.4</td>
</tr>
<tr>
<td>1990</td>
<td>160</td>
<td>676</td>
<td>23.7</td>
</tr>
<tr>
<td>2000</td>
<td>188</td>
<td>683</td>
<td>27.5</td>
</tr>
</tbody>
</table>

**SOURCE:** Authors' hand tallies based on all the issues of JEEM published in 1974, 1980, 1990, and 2000.

**NOTES:** Agricultural economics departments are defined here in terms of function rather than form, and include joint agricultural economics and economics departments, such as those at Iowa State University and Montana State University; departments labeled "Departments of Agricultural and Resource Economics," such as those at the University of California, Davis, and North Carolina State University; and departments with other labels, such as the Department of Applied Economics at the University of Minnesota, with major agricultural economics teaching and research responsibilities.

environmental and resource issues has siphoned off articles in this category from the AJAE. The implication of the decline in the proportion of the AJAE allocated to this category is therefore akin to the declines in the proportion of space in leading economics journals allocated to finance and economic statistics that took place in the later part of the twentieth century. Researchers are not doing less work in the area; other outlets for such research have developed.

Figure 2.10 indicates that the field of economic geography has generally received scant attention by agricultural economists, except in the late 1940s. In 1950, the share of journal space in the AJAE allocated to this category was 6.4 percent. In all other years, it has been less than 2 percent and often zero. It is conceivable that immediately after World War II relatively little research was done and that, quite simply, space was available for this topic.

Data on the category of manpower, labor, and population indicate that, although the proportion of the AJAE allotted to this category was relatively large in 1940 (13.1 percent), in no other year did this category account for more than 4.9 percent of the journal’s space (1970). More commonly, this category has accounted for only between 1 and 2 percent of the research published in the AJAE.

Prior to the 1950s, relatively little space in the AJAE was allocated to the fields of welfare programs, consumer economics, and urban and regional economics. Not until 1940 did the proportion of the journal accounted for by this category exceed 3 percent. In 1960, however, this category’s share more than doubled to 7.3 percent as policymakers became much more interested in welfare programs and consumer economics. After 1960, this category generally
accounted for about 5 percent of total journal space. Then, in 1990, the share of the journal allocated to this category declined to 2.7 percent, following a decade noted for a lack of effective political interest in programs targeted toward the poor.

Summary

Overall, as is the case with the three major general economics journals, the data show that, between 1930 and 2000, substantial changes occurred in the shares of *AJAE* space allocated to different aggregates of *JEL* categories. Compared with the 1930s and 1940s, the proportion of *AJAE* journal space allocated to three categories (general, growth, and macroeconomics; economic statistics; welfare programs, consumer economics, and urban and regional economics) has increased significantly. In contrast, the proportion of *AJAE* journal space allocated to three other categories (agriculture: general and situation and outlook; farm management and business administration; and land, natural resources, and conservation) has declined. In some cases, these adjustments have resulted from changes in the relative amounts of funding for different areas of research (for example, the declining focus on agriculture: general and situation and outlook). In others, the changes have also occurred in part as the result of innovations in techniques (for example, the increased focus on economic statistics). The share of space given to some categories has been relatively stable (for example, agricultural policy and agricultural demand and supply analysis). In contrast, the share of space allocated to one or two categories has been relatively volatile, as in the case of international economics, where sporadic surges in journal space appear to have been quite closely linked to surges in policy concerns about international agricultural trade relations.

Thus, as with economics, research output in the leading agricultural economics journal appears to be linked to policy debates and related research funding. It also appears to be affected by innovations in economic theory and empirical methods and by the development of competing outlets for research (as is suggested by the share of *AJAE* space allocated to the category of land, natural resources, and conservation).

Conclusion

We have examined the development and growth of the economic and agricultural economics research industries over the past 100 and more years. Time-series and cross-sectional data were compiled on the origins and expansion of professional economics associations and societies, professional association and society membership, the awarding of economics degrees, and the employment of economists in the United States and other countries. The data indicate that growth in both the economic and agricultural economics research industries
is linked to increases in the demand for the services of economists and agricultural economists. Another finding is that the economics profession has tended to become more diffuse, particularly since the 1940s, as more subdiscipline affiliations have been created. This process of segmentation was closely correlated with a period of rapid expansion in the aggregate size of the profession between 1940 and 1970. In global terms, economics and agricultural economics research resources have been much more heavily concentrated in developed countries than in developing countries. Since the 1960s, with respect to economics resources, the proportional gap between developed and developing countries appears to have remained stable and the absolute gap to have increased; with respect to agricultural economics, however, both the proportional gap and the absolute gap appear to have decreased.

We have also examined the changing composition of economics and agricultural economics research output in three leading economics journals (the AER, the JPE, and the EJ) and in the leading agricultural economics journal (the AJAE) by JEL categories over the period 1930 to 2000. As noted above, these data are subject to important caveats (perhaps even including idiosyncratic effects associated with changing editor preferences), but they do indicate that, as in many other research fields, research output in the leading economics and agricultural economics journals is quite closely linked to policy debates and research funding. They also show that economics and agricultural economics are no less subject to the impact of major analytical and technical advances than other disciplines. In addition, the development of competing outlets for research also appeared to affect the composition of output in the leading journals in economics and agricultural economics.

In summary, the evidence on the growth and development of the economic and agricultural economics research industries suggests that, just like other sectors of the economy, economists respond to economic incentives both in the amount and composition of what they do. Moreover, given that barriers to entry into the profession are not insurmountable, they are no less at the mercy of Marshallian forces of supply and demand than professors of English or sanitation workers. By the same token, however, the fact that relatively large quantities of economists are employed at positive market prices provides at least some prima facie evidence that their output has some economic value to someone. What that economic value may be and how it comes about is the subject of the following eleven studies.
<table>
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<td>Gay, Edwin F.</td>
<td>Historical records</td>
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<td>Hammond, Matthew B.</td>
<td>Economic conflict as a regulating force in international affairs</td>
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<td>1932</td>
<td>Bogart, Ernest L.</td>
<td>Pushing back the frontiers</td>
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<td>1933</td>
<td>Barnett, George E.</td>
<td>American trade unionism and social insurance</td>
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<td>1934</td>
<td>Usher, Abbott P.</td>
<td>A liberal theory of constructive statecraft</td>
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<td>1935</td>
<td>Millis, Harry A.</td>
<td>The union in industry: Some observations on the theory of collective bargaining</td>
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<td>1936</td>
<td>Clark, John M.</td>
<td>Past accomplishments and prospects of American economics</td>
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<td>1937</td>
<td>Johnson, Alvin</td>
<td>The economist in a world of transition</td>
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<td>1938</td>
<td>Sprague, Oliver M. V.</td>
<td>Recovery problem in U.S.</td>
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<td>1939</td>
<td>Hansen, Alvin H.</td>
<td>Economic progress and declining population</td>
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<td>1940</td>
<td>Viner, Jacob</td>
<td>Short and long view in economic policy</td>
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<td>1941</td>
<td>Mills, Frederick C.</td>
<td>Economics in a time of change</td>
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<td>Collective bargaining and the common interest</td>
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<td>Lucas, Robert E., Jr.</td>
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**NOTE:** Table includes only those addresses published in the *American Economic Review*. 
### APPENDIX TABLE 2.2 Richard T. Ely lectures of the American Economic Association

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<td>1990</td>
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(continued)
### APPENDIX TABLE 2.2 Continued

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<td>2003</td>
<td>Fischer, Stanley</td>
<td>Globalization and its challenges</td>
</tr>
<tr>
<td>2004</td>
<td>King, Mervyn A.</td>
<td>The institutions of monetary policy</td>
</tr>
</tbody>
</table>

**NOTE:** Table includes only those lectures published in the *American Economic Review*. 
## APPENDIX TABLE 2.3 Presidential addresses of the Royal Economic Society

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Title</th>
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<tbody>
<tr>
<td>1933</td>
<td>Cannan, Edwin</td>
<td>The need for simpler economics</td>
</tr>
<tr>
<td>1934</td>
<td>Cannan, Edwin</td>
<td>The future of gold in relation to demand</td>
</tr>
<tr>
<td>1935</td>
<td>Scott, William R.</td>
<td>The manuscript of an early draft of part of <em>The Wealth of Nations</em></td>
</tr>
<tr>
<td>1936</td>
<td>Scott, William R.</td>
<td>New light on Adam Smith</td>
</tr>
<tr>
<td>1939</td>
<td>Pigou, Arthur C.</td>
<td>Presidential address</td>
</tr>
<tr>
<td>1946</td>
<td>Hawtrey, Ralph G.</td>
<td>The need for faith</td>
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<tr>
<td>1949</td>
<td>Robertson, Dennis H.</td>
<td>On sticking to one's last</td>
</tr>
<tr>
<td>1950</td>
<td>Allen, G. C.</td>
<td>Economic progress, retrospect and prospect</td>
</tr>
<tr>
<td>1953</td>
<td>Brand, Lord</td>
<td>A banker's reflections on some economic trends</td>
</tr>
<tr>
<td>1954</td>
<td>Robinson, E. A. G.</td>
<td>The changing structure of the British economy</td>
</tr>
<tr>
<td>1955</td>
<td>Robbins, Lionel C.</td>
<td>The teaching of economics in schools and universities</td>
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<tr>
<td>1958</td>
<td>Carr-Saunders, Alexander M.</td>
<td>The place of economics and allied subjects in the curriculum</td>
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<tr>
<td>1959</td>
<td>Hall, Robert</td>
<td>Reflections on the practical applications of economics</td>
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<tr>
<td>1962</td>
<td>Hicks, John R.</td>
<td>Liquidity</td>
</tr>
<tr>
<td>1963</td>
<td>Harrod, Roy</td>
<td>Theme in dynamic theory</td>
</tr>
<tr>
<td>1967</td>
<td>Meade, James E.</td>
<td>Population explosion, the standard of living and social conflict</td>
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<tr>
<td>1972</td>
<td>Brown, E. H. Phelps</td>
<td>The underdevelopment of economics</td>
</tr>
<tr>
<td>1974</td>
<td>MacDougal, Donald</td>
<td>In praise of economics</td>
</tr>
<tr>
<td>1976</td>
<td>Kaldor, Nicholas</td>
<td>Inflation and recession in the world economy</td>
</tr>
<tr>
<td>1979</td>
<td>Brown, Arthur J.</td>
<td>Inflation and the British sickness</td>
</tr>
<tr>
<td>1980</td>
<td>Stone, Richard</td>
<td>Political economy, economics and beyond</td>
</tr>
<tr>
<td>1983</td>
<td>Deane, Phyllis</td>
<td>The scope and method of economic science</td>
</tr>
<tr>
<td>1985</td>
<td>Worwick, David</td>
<td>Jobs for all?</td>
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<tr>
<td>1986</td>
<td>Matthews, Rolan C. O.</td>
<td>The economics of institutions and the sources of growth</td>
</tr>
<tr>
<td>1988</td>
<td>Hahn, Frank</td>
<td>On monetary theory</td>
</tr>
<tr>
<td>1996</td>
<td>Atkinson, Tony</td>
<td>Bringing income distribution in from the cold</td>
</tr>
<tr>
<td>1997</td>
<td>Hendry, David F.</td>
<td>The econometrics of macroeconomic forecasting</td>
</tr>
<tr>
<td>2000</td>
<td>Dasgupta, Partha</td>
<td>Valuing objects and evaluating policies in imperfect economies</td>
</tr>
<tr>
<td>2003</td>
<td>Nickell, Stephen</td>
<td>Poverty and worklessness in Britain</td>
</tr>
</tbody>
</table>

**NOTE:** Table includes only those addresses published in the *Economic Journal.*
## APPENDIX TABLE 2.4 Presidential addresses of the American Agricultural Economics Association

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Title</th>
</tr>
</thead>
<tbody>
<tr>
<td>1968</td>
<td>Breimyer, Harold F.</td>
<td>The AAEA and USDA in an associationistic age</td>
</tr>
<tr>
<td>1969</td>
<td>Hathaway, Dale E.</td>
<td>The economics of agricultural economics</td>
</tr>
<tr>
<td>1970</td>
<td>Hillman, Jimmye S.</td>
<td>On returning from our agricultural Babel</td>
</tr>
<tr>
<td>1971</td>
<td>Ruttan, Vernon W.</td>
<td>Technology and the environment</td>
</tr>
<tr>
<td>1972</td>
<td>Castle, Emery N.</td>
<td>Economics and quality of life</td>
</tr>
<tr>
<td>1973</td>
<td>Tefertiller, Kenneth R.</td>
<td>Rural development in an urban age</td>
</tr>
<tr>
<td>1974</td>
<td>Nielson, James</td>
<td>Accountability and innovation: Challenges for agricultural economists</td>
</tr>
<tr>
<td>1975</td>
<td>Bonnen, James T.</td>
<td>Improving information on agriculture and rural life</td>
</tr>
<tr>
<td>1976</td>
<td>Farrell, Kenneth R.</td>
<td>Public policy, the public interest, and agricultural economics</td>
</tr>
<tr>
<td>1978</td>
<td>Stanton, Bernard F.</td>
<td>Perspective on farm size</td>
</tr>
<tr>
<td>1979</td>
<td>King, Richard A.</td>
<td>Choices and consequences</td>
</tr>
<tr>
<td>1980</td>
<td>Tweeten, Luther G.</td>
<td>Macroeconomics in crisis: Agriculture in an underachieving economy</td>
</tr>
<tr>
<td>1981</td>
<td>Schuh, G. Edward</td>
<td>Economics and international relations: A conceptual framework</td>
</tr>
<tr>
<td>1982</td>
<td>Polopolus, Leo C.</td>
<td>Agricultural economics: Beyond the farm gate</td>
</tr>
<tr>
<td>1983</td>
<td>Hart, Neil E.</td>
<td>Agricultural economics: Challenges to the profession</td>
</tr>
<tr>
<td>1984</td>
<td>Baker, C. B.</td>
<td>Agricultural effects of change in financial markets</td>
</tr>
<tr>
<td>1985</td>
<td>Tomek, William G.</td>
<td>Limits on price analysis</td>
</tr>
<tr>
<td>1986</td>
<td>Havlicek, Joseph, Jr.</td>
<td>Megatrends affecting agriculture: Implications for agricultural economics</td>
</tr>
<tr>
<td>1987</td>
<td>Paberg, Daniel I.</td>
<td>Agricultural economics: Finding our future</td>
</tr>
<tr>
<td>1988</td>
<td>Manderscheid, Lester V.</td>
<td>Undergraduate educational opportunities in the face of declining enrollments</td>
</tr>
<tr>
<td>1989</td>
<td>Batie, Sandra S.</td>
<td>Sustainable development: Challenges to the profession of agricultural economics</td>
</tr>
<tr>
<td>1990</td>
<td>Johnston, Warren E.</td>
<td>Structural change and the recognition of diversity</td>
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<tr>
<td>1991</td>
<td>Beattie, Bruce R.</td>
<td>Some almost-ideal remedies for healing land grant universities</td>
</tr>
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<td>1992</td>
<td>Houck, James D.</td>
<td>The comparative advantage of agricultural economists</td>
</tr>
<tr>
<td>1993</td>
<td>Barry, Peter J.</td>
<td>Coordinating research, extension, and outreach in agricultural economics</td>
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<td>1994</td>
<td>Libby, Lawrence W.</td>
<td>Conflict on the commons: Natural resource entitlement, the public interest, and agricultural economics</td>
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(continued)
<table>
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<th>Year</th>
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<th>Title</th>
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<tr>
<td>1995</td>
<td>Eidman, Vernon R.</td>
<td>The continuing search for relevance in agricultural economics</td>
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<td>1996</td>
<td>Christy, Ralph D.</td>
<td>Markets or government? Balancing imperfect and complementary activities</td>
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<td>1997</td>
<td>Armbruster, Walter J.</td>
<td>Challenges for agricultural economists facing the twenty-first century</td>
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<td>1998</td>
<td>Shumway, Richard C.</td>
<td>Values, changing forces, choices, and the profession</td>
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<td>1999</td>
<td>Antle, John M.</td>
<td>The new economics of agriculture</td>
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<td>2000</td>
<td>Gardner, Bruce L.</td>
<td>Economic growth and low incomes in agriculture</td>
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<td>2001</td>
<td>Kinsey, Jean D.</td>
<td>The new food economy: Consumers, farms, pharms, and science</td>
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<td>2002</td>
<td>Offutt, Susan</td>
<td>The future of farm policy analysis: A household perspective</td>
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<tr>
<td>2003</td>
<td>Brandt, Jon A.</td>
<td>AAEA: Adapting to meet member needs</td>
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*NOTE: Table includes only those addresses published in the *American Journal of Agricultural Economics.*
**APPENDIX TABLE 2.5** Nobel Prizes in economics

<table>
<thead>
<tr>
<th>Year</th>
<th>Recipient</th>
<th>Nationality</th>
<th>Institutional affiliation</th>
<th>Subject area</th>
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<tbody>
<tr>
<td>1969</td>
<td>Tinbergen, Jan</td>
<td>Netherlands</td>
<td>Netherlands School of Economics</td>
<td>Developing and applying dynamic models for the analysis of economic processes</td>
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<tr>
<td></td>
<td>Frisch, Ragnar</td>
<td>Norway</td>
<td>Oslo University</td>
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<tr>
<td>1970</td>
<td>Samuelson, Paul A.</td>
<td>United States</td>
<td>Massachusetts Institute of Technology</td>
<td>Developing static and dynamic economic theory contributing to raising the level of analysis in economic science</td>
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<td>1971</td>
<td>Kuznets, Simon</td>
<td>United States, Soviet Union</td>
<td>Harvard University</td>
<td>Founding empirical interpretation of economic growth, which has led to new and deepened insight into the economic and social structure and process of development</td>
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<tr>
<td>1972</td>
<td>Hicks, John R.</td>
<td>United Kingdom</td>
<td>All Souls College</td>
<td>Pioneer contributions to general economic equilibrium theory and welfare theory</td>
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<td></td>
<td>Arrow, Kenneth J.</td>
<td>United States</td>
<td>Harvard University</td>
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<tr>
<td>1973</td>
<td>Leontief, Wassily</td>
<td>United States</td>
<td>Harvard University</td>
<td>Developing the input-output method and its application to important economic problems</td>
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<td>1974</td>
<td>von Hayek, Frederick A.</td>
<td>Austria, United States</td>
<td>Stockholm University</td>
<td>Pioneer work in the theory of money and economic fluctuations, and penetrating analysis of the interdependence of economic, social, and institutional phenomena</td>
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<td></td>
<td>Myrdal, K. Gunnar</td>
<td>Sweden</td>
<td></td>
<td></td>
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<td>1975</td>
<td>Koopmans, Tjalling</td>
<td>United States</td>
<td>Yale University</td>
<td>Contributions to the theory of optimum allocation of resources</td>
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<td>Kantorovich, Leonid</td>
<td>Soviet Union</td>
<td>Academy of Sciences, Moscow</td>
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<tr>
<td>1976</td>
<td>Friedman, Milton</td>
<td>United States</td>
<td>University of Chicago</td>
<td>Achievements in the fields of consumption analysis, monetary history, and theory and demonstration of the complexity of stabilization policy</td>
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(continued)
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<th>Year</th>
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<th>Nationality</th>
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<td>1977</td>
<td>Ohlin, Bertil</td>
<td>Sweden</td>
<td>Stockholm School of Economics</td>
<td>Contribution to the theory of international trade and international capital movements</td>
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<td>Meade, James</td>
<td>United Kingdom</td>
<td>Cambridge University</td>
<td>Pioneering research into the decisionmaking process within economic organizations</td>
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<td>1978</td>
<td>Simon, Herbert A.</td>
<td>United States</td>
<td>Carnegie-Mellon University</td>
<td>Pioneering research into economic development research with particular consideration of the problems of developing countries</td>
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<td>1979</td>
<td>Schultz, Theodore W.</td>
<td>United States</td>
<td>University of Chicago</td>
<td>Creation of econometric models and the application to the analysis of economic fluctuations and economic policies</td>
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<td></td>
<td>Lewis, Arthur</td>
<td>United Kingdom</td>
<td>Princeton University</td>
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<td>1980</td>
<td>Klein, Lawrence R.</td>
<td>United States</td>
<td>University of Pennsylvania</td>
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<td>1981</td>
<td>Tobin, James</td>
<td>United States</td>
<td>Yale University</td>
<td>Analysis of financial markets and their relations to expenditure decisions, employment, production, and prices</td>
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<tr>
<td>1982</td>
<td>Stigler, George J.</td>
<td>United States</td>
<td>University of Chicago</td>
<td>Seminal studies of industrial structures, functioning of markets, and causes and effects of public regulation</td>
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<td>1983</td>
<td>Debreu, Gerard</td>
<td>United States</td>
<td>University of California, Berkeley</td>
<td>Incorporating new analytical methods into economic theory and rigorous reformulation of the theory of general equilibrium</td>
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<tr>
<td>1984</td>
<td>Stone, Richard</td>
<td>United Kingdom</td>
<td>Cambridge University</td>
<td>Fundamental contributions to the development of systems of national accounts and hence greatly improved the basis for empirical economic analysis</td>
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<td>1985</td>
<td>Modigliani, Franco</td>
<td>Italy, United States</td>
<td>Massachusetts Institute of Technology</td>
<td>Pioneering analyses of saving and of financial markets</td>
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<tr>
<td>Year</td>
<td>Name</td>
<td>Country</td>
<td>Institution</td>
<td>Field of Contribution</td>
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<td>------</td>
<td>-------------------</td>
<td>-----------------</td>
<td>-------------------------------------------</td>
<td>---------------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>1986</td>
<td>Buchanan, James</td>
<td>United States</td>
<td>Center for Study of Public Choice</td>
<td>Developing the contractual and constitutional bases for the theory of economic and political decisionmaking</td>
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<tr>
<td>1987</td>
<td>Solow, Robert M.</td>
<td>United States</td>
<td>Massachusetts Institute of Technology</td>
<td>Contributions to the theory of economic growth</td>
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<tr>
<td>1988</td>
<td>Allais, Maurice</td>
<td>France</td>
<td>École Nationale Supérieur des Mines de Paris</td>
<td>Pioneering contributions to the theory of markets and efficient utilization of resources</td>
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<td>1989</td>
<td>Haavelmo, Trygve</td>
<td>Norway</td>
<td>University of Oslo</td>
<td>Clarification of the probability theory foundations of econometrics and analyses of simultaneous economic structures</td>
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<td>1990</td>
<td>Markowitz, Harry</td>
<td>United States</td>
<td>City University of New York</td>
<td>Pioneering work in the theory of financial economics</td>
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<tr>
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<td>Miller, Merton</td>
<td>United States</td>
<td>University of Chicago</td>
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<td>Sharpe, William</td>
<td>United States</td>
<td>Stanford University</td>
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<td>1991</td>
<td>Coase, Ronald H.</td>
<td>United Kingdom,</td>
<td>University of Chicago</td>
<td>Discovery and clarification of the significance of transaction costs and property rights for the institutional structure and functioning of the economy</td>
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<tr>
<td></td>
<td></td>
<td>United States</td>
<td></td>
<td>Extending the domain of microeconomic analysis to a wide range of human behavior and interaction, including nonmarket behavior</td>
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<tr>
<td>1992</td>
<td>Becker, Gary S.</td>
<td>United States</td>
<td>University of Chicago</td>
<td>Renewing research in economic history by applying economic theory and quantitative methods to explain economic and institutional change</td>
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<tr>
<td>1993</td>
<td>Fogel, Robert W.</td>
<td>United States</td>
<td>University of Chicago</td>
<td>Analysis of the equilibrium in noncooperative game theory</td>
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<td></td>
<td>North, Douglass C.</td>
<td>United States</td>
<td>Washington University</td>
<td></td>
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<td>1994</td>
<td>Selten, Reinhard</td>
<td>Germany</td>
<td>Rheinische Friedrich-Wilhelms-Universität</td>
<td></td>
</tr>
<tr>
<td></td>
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<td></td>
<td>University of California, Berkeley</td>
<td></td>
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<td></td>
<td>Harsanyi, John C.</td>
<td>United States</td>
<td>University of California, Berkeley</td>
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<td></td>
<td>Nash, John F.</td>
<td>United States</td>
<td>Princeton University</td>
<td></td>
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<tr>
<td>Year</td>
<td>Recipient</td>
<td>Nationality</td>
<td>Institutional affiliation</td>
<td>Subject area</td>
</tr>
<tr>
<td>------</td>
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<tr>
<td>1995</td>
<td>Lucas, Robert E., Jr.</td>
<td>United States</td>
<td>University of Chicago</td>
<td>Hypothesis of rational expectations, transforming macroeconomic analysis, and deepening understanding of economic policy</td>
</tr>
<tr>
<td>1996</td>
<td>Mirrlees, James A.</td>
<td>United Kingdom</td>
<td>Cambridge University</td>
<td>Fundamental contributions to the economic theory of incentives under asymmetric information</td>
</tr>
<tr>
<td></td>
<td>Vickrey, William</td>
<td>Canada, United States</td>
<td>Columbia University</td>
<td></td>
</tr>
<tr>
<td>1997</td>
<td>Merton, Robert C.</td>
<td>United States</td>
<td>Harvard University</td>
<td>A new method for determining the value of derivatives</td>
</tr>
<tr>
<td></td>
<td>Scholes, Myron S.</td>
<td>United States</td>
<td>Stanford University</td>
<td></td>
</tr>
<tr>
<td>1998</td>
<td>Sen, Amartya</td>
<td>India, United Kingdom</td>
<td>Trinity College</td>
<td>Contributions to welfare economics</td>
</tr>
<tr>
<td>1999</td>
<td>Mundell, Robert A.</td>
<td>United States, Canada</td>
<td>Columbia University</td>
<td>Analysis of monetary and fiscal policy under different exchange rate regimes, and analysis of optimum currency areas</td>
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<tr>
<td>2000</td>
<td>Heckman, James J.</td>
<td>United States</td>
<td>University of Chicago</td>
<td>Developing theory and methods for analyzing selective samples</td>
</tr>
<tr>
<td></td>
<td>McFadden, Daniel L.</td>
<td>United States</td>
<td>University of California, Berkeley</td>
<td>Developing theory and methods for analyzing discrete choice</td>
</tr>
<tr>
<td>2001</td>
<td>Akerlof, George A.</td>
<td>United States</td>
<td>University of California, Berkeley</td>
<td>Analyses of markets with asymmetric information</td>
</tr>
<tr>
<td></td>
<td>Spence, A. Michael</td>
<td>United States</td>
<td>Stanford University</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Stiglitz, Joseph E.</td>
<td>United States</td>
<td>Columbia University</td>
<td></td>
</tr>
<tr>
<td>2002</td>
<td>Kahneman, Daniel</td>
<td>United States, Israel</td>
<td>Princeton University</td>
<td>Integrating insights from psychological research into economics and establishing laboratory experiments as a tool in empirical economic analysis</td>
</tr>
<tr>
<td></td>
<td>Smith, Vernon L.</td>
<td>United States</td>
<td>George Mason University</td>
<td></td>
</tr>
<tr>
<td>2003</td>
<td>Engle, Robert E.</td>
<td>United States</td>
<td>New York University</td>
<td>Methods of analyzing economic time series with common trends (cointegration)</td>
</tr>
<tr>
<td></td>
<td>Granger, Clive W. J.</td>
<td>United Kingdom</td>
<td>University of California, San Diego</td>
<td></td>
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</table>

**Source:** Compiled by the authors from Nobel Foundation (2001).

*Affiliation of the laureate at the time of receiving the award.

*The subject area is the area identified by the committee as the basis for the award.*
References


PART II

Economists and
Applied Economics Research
I have written this chapter as a “think piece”: it raises problems it fails to resolve and it is sketchy even in the way it raises those problems. Nonetheless, I hope that it may be of some help in advancing the discussion of challenges facing economists addressed in this volume. The main thrust of the chapter is that there are special problems involved in assessing the benefits from economics research, problems that do not arise to the same degree in attempts to assess, say, medical or meteorological research. Broadly speaking, these problems arise from four sources. First, economics is generally lacking in the sort of repetitive cases or events—like daily weather forecasts or repeated examples of a disease—that allow analysis to establish a clear track record of improving predictive accuracy, a track record whose benefits can then be quantified. Second, to the extent that policy-oriented economics research makes recommendations that are put into practice, it is usually not possible to establish controls that would provide an objective measure of the value of those recommendations. As a result, we run into the third problem: Assessments of the results of policies based on economic analysis must generally involve some inference that is also based on economic analysis. To skeptics, this can easily seem to involve a more or less circular process of reasoning, in which policies are assumed to have had good effects because they were in accord with the prevailing theory rather than because they were demonstrated in any objective way to be better than alternatives. Finally, policy-oriented research only pays off to the extent that it is actually used to guide policy—and many of the most important results of policy-relevant research in economics are simply disregarded by the political process. (This disregard is, of course, linked to skepticism about the economic analysis that underlies such research.)

This chapter is divided into five sections. I begin with a discussion and defense of economics research in general. Then I offer a rough characterization of the kinds of predictions that research does and does not allow economists to make. The third section turns to the value of predictions, whether simple forecasts or, more important, contingent predictions of the effects of alternative policies. In the fourth section, I argue that assessing the value of such predictions is
complicated by the frequent unwillingness of policymakers to accept either the researchers' recommendations or their assessment of the effects of past policies. The final section of the chapter turns to the problem of valuing frontier research versus the application of existing economic knowledge.

The Nature of Economic Knowledge

What Do Economists Know, and How Do They Know It?

A substantial number of people, no doubt including some researchers in other social sciences, believe that the answer to this question is straightforward: Economists don't know anything, or at least anything that isn't obvious to everyone. In fact, any economist who seriously attempts to debate real-world policy must become accustomed to experiencing constant challenges to any claims he or she may make of professional authority. Unlike, say, a medical researcher in the health-policy field, a policy-oriented economist must expect, on most issues, to confront people who insist that "economics research" is an oxymoron; who do not accept that any results of prior economics research, no matter how thoroughly accepted within the field, may be considered established; and who regard a researcher's professional credentials as a liability rather than an asset.

It is important to be aware of this attitude, because its prevalence greatly complicates the task of assessing the benefits (if any) of economics research. I obviously do not share the common negative assessment of economics research. Economics is clearly not as well developed as the physical sciences; however, I would argue that the success of economics at explaining the phenomena it studies is at least comparable to that of, say, evolutionary biology (a field with which it actually has a great deal in common in terms of style, and often in content). The ability of economists to offer guidance to useful interventions is, perhaps, comparable to that of physicians at the end of the last century—that is, we are able to suggest very valuable prophylactic measures, but relatively few miracle cures.

Why, then, does the discipline of economics come under constant criticism, not just of particular theories and proposals but of its whole basis? Part of the answer has to do with what amounts to a philosophical rejection of the whole idea of social science, particularly the passionless, mathematical form taken by much of economics. (Evolutionary theory faces the same sort of hostility, for the same reason.) Beyond this, of course, is the inherent politicization of economics—after all, when they enter the political arena, even such normally apolitical types as climate researchers suddenly find themselves confronted by critics who reject even overwhelming scientific consensus and attack the motives of those who report that consensus, because they do not like the implied policy conclusions. (It is a given that critics whose motivations are essentially political are precisely the people who are most likely to claim that a certain
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piece of policy-oriented research whose results are inconvenient was politically slanted.)

Still, because assessments of economics research always take place in such a highly critical atmosphere, it is useful to start by summarizing in what sense economic knowledge does in fact exist and can be added to by research.

There are no doubt many different ways to think about the content of economics as practiced, but I would make a three-way division. First, there are certain basic principles that underlie the bulk of economic analysis; these basic principles, and their necessary implications, are part of what economists have to offer. Second, there is a large collection of models—particular thought experiments that apply the basic principles to stylized settings intended to capture some crucial aspect or aspects of a real-world situation. Finally, there is an accumulation of empirical evidence that bears on that collection of models.

**The Principles of Economics**

Most economic analysis involves viewing economic outcomes as the result of a feedback loop: from the environment to individual behavior and back to the environment again. Individuals make decisions based on the environment they face; but the environment they face is itself affected by individual decisions. (Even the most basic concepts, like that of prices determined by supply and demand, take this form: Decisions about how much to consume and to produce are governed by prices, but these prices are themselves the result of these consumer and producer decisions.) Whatever the details of the specific analysis, economists are generally pretty good about making the arrows in that loop—what Coleman (1990) calls the “macro to micro” and “micro to macro” transitions—explicit. That is, “macro” phenomena such as the overall level of wages are (almost) always explained in terms of the interaction of “micro” motivations, with a clear description both of how individual decisions change the environment and of how these decisions themselves are affected by the environment. (Schelling [1978] offered an excellent slogan when he entitled a collection of essays about applications of the economist’s way of thinking *Micromotives and Macrobehavior*.)

The principle—drilled home during an economist’s professional training—that one must be explicit and careful about the transitions from macro to micro and back again is a major part of what economists know (and what those who criticize economists often do not). Indeed, many of the areas where economic analysis conflicts most sharply with the perceptions of noneconomists—and where that difference generates the greatest annoyance on the part of those noneconomists—precisely involve situations in which economists, unlike their critics, understand that there is a feedback loop, and that to predict what will happen, one must take that loop into account.

Consider, for example, the dispute over the effects that rising productivity in developing countries has on advanced nations. To many people, it seems obvious that the result must be higher unemployment and lower incomes in
advanced nations: because Third World workers currently receive very low wages, it seems obvious that if these workers can come anywhere close to matching Western productivity, Western workers will be unable to compete with them. Economists who consider the question immediately notice, however, that there are surely some important feedback issues involved, in particular, that Third World wages cannot be presumed to be a given. That is, Western employers may be attracted to produce in Third World countries by the low wages there, but these wages are low precisely because productivity is low; if productivity rises, so will wages. Indeed, the standard economic argument, an argument confirmed by all available evidence, is that average national wages tend to track average productivity in a country more or less one-for-one, so that the alleged competitive problem will never materialize. It is conceivable that this argument will turn out to be wrong in the future, but the point here is that economists are led to a point of view that is very different from what most people believe, and which, even if it turns out not to be precisely correct, is also almost surely far closer to the truth than the views of noneconomists. This is because the principles of economics tell economists to think through the loop that mutually links individual behavior and the environment in which those individuals function.

One important aspect of the economist’s relative care in thinking through the economy’s feedback loops is an awareness of important accounting identities, identities that are often not at all obvious to noneconomists, as anyone who has engaged in public debate can attest.

A good example is the proposition that the balance of payments on current account is necessarily equal to the difference between domestic savings and domestic investment, which loosely implies that a country cannot simultaneously import capital and run a trade surplus. It turns out that many people, including those who imagine themselves to be well informed about the world economy—such as the authors of the 1994 World Competitiveness Report (put out by the organizers of the celebrated Davos conferences)—are unaware of this identity. Indeed, that now infamous report quite explicitly predicted a future in which low-wage countries would simultaneously experience large capital inflows and run massive trade surpluses.

Another good example involves the possibilities of more rapid U.S. economic growth. Many advocates of the so-called new paradigm (which holds that old speed limits on growth no longer obtain) argue that the Federal Reserve can safely adopt higher growth targets for real gross domestic product (GDP) because the true rate of productivity growth is higher than official statistics indicate. The obvious answer is that measured productivity growth is nothing more than measured GDP growth minus the rate of labor-force growth. Thus, any understatement of productivity growth must involve a precisely equal understatement of GDP growth; one cannot therefore argue that undermeasurement of productivity growth allows a higher target for measured GDP growth. (Or to put it differently, one should not urge Alan Greenspan to adopt, say, a 3.5 per-
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cent growth target; one should instead congratulate him for having already achieved that growth rate, even though the official numbers say that growth has been much less.) Yet it is clear that this identity, too, is not at all obvious to many people who have strong opinions on the issue.

Accounting identities and the recognition of feedback loops are clear areas in which economic principles provide valuable insight. However, economists are often criticized for another often-used principle: the assumption that individuals act rationally in their own self-interest. Nobody really believes that individuals are perfectly rational. Why, then, assume rational behavior? The answer is that, because it is crucial to keep track of the feedback loop from individuals to environment and back again, it is important to be as explicit as possible about what individuals will do in any given environment, and rational behavior, while clearly a fiction, is a tremendously useful working assumption for the purpose of closing the loop.

Economic Models

Contrary to what is widely supposed, very little economic theorizing involves deductions from a set of axioms. The overwhelming bulk of economic theory consists of models, which are best described as thought experiments, asking how things would work in a simplified world that is intended to capture some, but by no means all, aspects of the real world. The conclusions of such models depend crucially on which particular aspects of reality are included and which are left out; the test of a model is therefore not its truth—because models, like maps, inevitably omit some possibly important details—but its usefulness. (For instance, most street maps of San Francisco do not show the city’s topography. This omission is not very important for tourists in automobiles, for whom contour lines would just be confusing, but it makes standard maps all but worthless to bicyclists.)

Is the construction of economic models a cumulative, progressive venture as opposed to a sheer proliferation of stories? Yes, for three main reasons. First, many models have other models nested inside them so that, say, traditional models of international trade make use of the concept of market-clearing, also known as the model of supply and demand, while many more recent models build on models of monopolistic competition. (This nesting is sometimes subtle and implicit. For example, many international trade models simply assume balanced trade; they do so because those who created them had other models of the balance of payments in reserve, which suggested a long-run tendency of trade surpluses or deficits to correct themselves, thus freeing the modelers to ignore the payments balance as a first approximation.)

Second, the construction of models generally involves technical tricks—certain simplifying assumptions that allow the modeler to get at the main point, certain mathematical tools that allow an easier statement of the issues or a solution to the problem posed, and so on—which generalize to other models.
For example, the technical trick of representing a large number of goods as a continuum, which allows the modeler to use calculus on problems that involve discrete changes, was introduced in the 1960s; it has since been used in dozens of analyses on everything from international trade to finance. So each model builds on the technical tricks developed in earlier models.

Finally, while no one model captures reality, the mosaic of models does cover a growing fraction of reality, so economists become able to offer guidance on a gradually widening range of issues.

**Accumulation of Evidence**

Economists, of course, know quite a few facts, not just about the raw data, but about what you learn by adding and subtracting, multiplying and dividing, regressing and cointegrating those data. Both data collection and data analysis are, of course, guided by the menagerie of models.

Unless one has become involved in the public debate over an economic issue on which noneconomists hold strong views, it is hard to get a sense of just how much useful data collection and analysis economists have done. One tends to assume, for example, that anyone trying to work on global economic issues would quickly figure out that productivity and wages have always moved more or less in tandem, both historically and in cross section; that, as a group, newly industrializing economies run trade deficits, not trade surpluses; or that the distribution of income between capital and labor in the United States has been approximately stable over the past 25 years. But when a writer like Greider (1997) tries to create a picture of the world economy without consulting the results of economics research, he fails to get any of these seemingly simple things right. In other words, these observations, which are surely crucial to any attempt to make either predictions or policy analysis in the area of world trade and investment, are the fruits of professional research, not facts obvious to anyone.

For some important policy issues, simply getting the facts straight has been a difficult and important task. A particularly important example in recent years has concerned the growth of income inequality in the United States. This might seem to be a straightforward measurement issue, but until the early 1990s, the picture of income disparities was cluttered by a variety of confusions: assertions that the whole thing was a statistical illusion, or that income mobility was so high that point-in-time comparisons were meaningless; assertions that the growth in inequality was a labor versus capital issue, though it turns out to be mainly labor versus labor; attempts to frame the debate in terms of the alleged shrinkage of an arbitrarily defined middle class; and so on. Some of these confusions were politically motivated (from both the left and right), but many required the collection and analysis of extensive data to resolve.

It is also probably worth pointing out that, in recent years, the focus of economics research has shifted somewhat away from model-building and even away from sophisticated econometrics in the direction of the creation and rel-
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Atively simple analysis of large amounts of data. The three most recent Clark Medals in economics have been given to economists whose professional work is largely in labor economics, a subfield dominated by a heavily empiricist ethos. (A recent award, to Kevin Murphy of the University of Chicago, was in fact largely for work on income inequality.)

In summary, then, economists do in fact know something, and economics research does in fact add to what they know. But how much is that growth in knowledge worth?

Economic Predictions

Some scientists try to justify their work on the grounds of sheer curiosity: we should study the world because of the joy of understanding. Similar feelings are an important part of the motivation of many social scientists. However, the funders of such science are less moved by that kind of idealism; they want evidence of a more concrete payoff. Such concrete payoffs, it may be argued, always take the form of the ability to make predictions. However, these predictions can take two quite different forms.

Simple Predictions

The simplest kind of economic prediction is a simple forecast: next year GDP will grow by 3 percent, or China will become the world’s largest economy by the year 2020. Such forecasts are the analogue in economics of weather forecasts, both the short-term forecasts made using models of global circulation and the longer-term forecasts made using empirical regularities, such as the events that typically follow an El Niño. (Weather forecasters are unable, at this point, to predict an El Niño itself, but can say quite a lot about what typically follows, just as economists cannot predict stock market crashes, but may have some useful things to say about their consequences.)

Such unconditional economic forecasts are notoriously unreliable; indeed, the inability of economists to show a consistent track record of improving unconditional forecasts is a major part of the field’s public relations problem. (Meteorologists, by contrast, are able to demonstrate a clear record of improvement.) There are a number of excuses for the failure of economic forecasters to manage to improve their track record; high on the list would be the fact that the quality of government-collected economic data has generally deteriorated rather than improved in recent years—in clear contrast with the ever-growing volume of current information available to weather forecasters—and the fact that the economy, to a far greater extent than the weather system, is continually evolving, so that old relationships no longer work.

Still, such simple, unconditional predictions are clearly not the strong suit of economics research. And they actually occupy very little of the field’s resources or best talent.
Contingent Forecasts

The main focus of economic prediction is in the form of contingent predictions: if you (where you might be an individual or a corporation, but most commonly are a government) do this, then that will be the result. If you impose a quota on imports of Japanese autos, the price of both foreign and domestic cars will increase; if you deregulate thrift institutions without removing deposit insurance, there will be large-scale risky lending.

It is far more difficult to quantify the success of such contingent forecasts than that of simple predictions. One cannot simply look at mean-squared errors—errors on what, compared to what? That is, such issues are often one-time events, and, anyway, it is not clear what people would have predicted in the absence of economics research.

Still, for what it is worth, there have been a number of notable successes of contingent predictions in economics research. Some conspicuous examples would include the following cases.

- Although simple predictions went seriously astray with the emergence of stagflation in the 1970s, that emergence itself was a triumph of contingent prediction: the Friedman-Phelps “natural rate” hypothesis had predicted, well before the actual event, that the historical relationship between unemployment and inflation would break down in the face of sustained inflationary monetary policies.
- Environmental economists insisted for decades, to no avail, that allowing polluters to trade emission licenses would considerably reduce the cost of meeting environmental targets; when such permit trading was actually introduced in the Bush (senior) Administration, the prediction turned out to be completely correct.
- International macroeconomists widely predicted that the attempt of European countries to maintain fixed exchange rates in the face of German reunification would lead to a recession, and so it did.

The most important use of contingent predictions is, of course, to make decisions: If an economist can give you a better idea of the consequences of alternative courses of action, you are likely to choose a better one than you would have in the absence of economics research. But how much is this extra knowledge worth?

The Problem of Assessment

To understand the problems of putting a value on economic predictions, it may be helpful to compare it with the problem of putting a value on weather predictions. The similarities and the differences are both instructive.
**Simple Predictions**

The bread-and-butter justification for the science of meteorology is, of course, its ability to make simple, unconditional predictions about the weather over the next few days. And the value of research that lets meteorologists make better predictions is, in principle at least, fairly easy to measure. Suppose that improved theory, better measuring techniques, and faster computers allow forecasters to make a more accurate prediction of, say, the paths of hurricanes. Then people in endangered areas are more likely to be warned well in advance, allowing them to take precautionary measures that preserve lives and property; meanwhile, people who are not likely to be in a storm’s path can be saved the expense of unnecessary precautions. Provided one is able to measure these gains, and to put a value on the property and lives saved, one can quantify the benefits of the research that made the better forecast possible.

Even in this simple case, however, there are some complications. The accuracy of the forecasts must be compared to some baseline—in effect, to what the forecasts would have been without the results of the research. This baseline may not be so easy to construct. One method would be for the National Weather Service to continue to run its old models, using old-quality data, simply to figure out what it would have predicted were it not for recent improvements. But this would eventually become expensive, and perhaps even embarrassing. After all, there will surely be particular instances in which forecasters were too smart for their own good—that is, in which, by sheer bad luck, all the improvements in data and theory actually turned out to yield a worse prediction.

Luckily for meteorologists, storms are repetitive events. They need not replay each storm prediction using old and new techniques; they can simply compare the average quality of forecasts over several years in the past with more recent experience, and use those averages to estimate the payoff to improved knowledge.

Admittedly, even in the case of meteorology, this kind of statistical measure of improvement may be problematic. Suppose that, whether because of global warming or natural climatic shifts, the weather pattern begins to change, perhaps also becoming more unstable in the process. Then models that worked well in the past may cease to track as well, both because they were designed for previous conditions and because the weather has become inherently less predictable. This could easily lead to a situation in which the average accuracy of forecasts is declining even though meteorology as a science is clearly advancing.

The problem that economists encounter is immediately obvious: they are always in the situation of meteorologists facing a changing weather pattern. Some of the events economists try to predict, like the fixture growth of newly industrializing economies or the prospects for high-technology industries, are more or less completely novel. That is, one cannot compare the accuracy of current methods used for predicting which Third World nations will become
“tigers” with those used 20 years ago: The phenomenon of “tiger” economies is a new one, so there is no baseline. Other events, while they are in some respects repetitive, still lack the consistency and, for that matter, the frequency of hurricanes and tornadoes (for example, hyperinflations and currency crises).

The nearest economic counterpart we have to forecasting the weather is forecasting the business cycle. Yet even the business cycle changes its character at a pace that meteorologists would find terrifying. The U.S. economy did not exhibit anything resembling modern business cycles until after the Civil War. From that point until the 1930s, cycles got progressively more severe. They then became much less severe—perhaps because of the impact of economic theory on economic policy, another issue of the kind most sciences are spared; but also perhaps because of the emergence of a vastly larger government sector during World War II, which tended to act as an automatic stabilizer. The oil price shocks of the 1970s produced a new kind of recession, never seen before. And the cycle may have changed character yet again because of growing international trade and the shift from a manufacturing to a service economy. Consider how the track record of weather forecasting would look in a world in which global weather patterns changed character every decade or two, and one has a sense of why business cycle forecasting has not been a good advertisement for economic science.

Thus, the measured quality of economic forecasts tends to be dominated by the changing nature of the economy rather than the progress or lack thereof in economic science. Predictions look pretty good during predictable periods, like the long expansions of 1961–69 or 1991–2000; they look bad during periods of instability like the 1970s. This does not mean that economists should not try to improve the quality of simple predictions; indeed, there are good reasons to believe that, since the forecasting debacle of the 1970s (which was largely the result of the emergence of novel shocks to the economy, but whose impact on the prestige of the profession was made far worse by the previous hubris of econometric modelers), economists have underinvested in the business of making simple predictions that can be evaluated against the actual events.

The major payoff from economic analysis, however, lies in its potential usefulness for making contingent predictions.

Contingent Predictions

Most climatologists are convinced that the continuing emission of greenhouse gases has begun to produce significant global warming. If they are right, over time we will begin to see not only generally higher temperatures, but rising sea levels, increased severity of hurricanes and other extreme weather events, and further unpleasant consequences.

Suppose that there were nothing we could do about that prospect. The prediction would still have some economic value. People making long-term investments would be aware that some land would become unsuitable for culti-
vation while other land would become usable; there would be an obvious incentive to reduce long-term development on coastal plains and in areas likely to be subject to superhurricanes, supertornadoes, and other severe weather events.

The main potential value of analyses of global warming, however, is that they offer the possibility of action. Because they offer a contingent prediction—how much warming will occur if greenhouse gas emissions occur at such and such a pace—they may lead to the adoption of policies that limit such emissions, thereby limiting the damage. The value of the prediction, then, is the difference between welfare under the policies followed in view of the prediction and under the policies that would have been followed otherwise.

Most of the payoff from economic knowledge takes this same form. Suppose that an economic analysis predicts that the cost of any given reduction in pollution will be much less if accomplished via the auction of pollution licenses than if accomplished by direct regulation. This may then lead to the adoption of a license scheme; the benefit from the analysis is the reduction in cost achieved by that scheme, perhaps increased if the regulator takes advantage of the lower marginal cost of pollution reduction in order to pursue a more ambitious goal.

I chose this particular example deliberately—for two reasons. First, it is a case in which economists almost surely have some very good advice to offer. Reasonable estimates suggest that a package of pollution and congestion taxes or license auctions could raise real income in the United States by at least $40 billion annually, possibly much more. This is a huge sum compared with, say, the $20 million or so the National Science Foundation spends on economics research. So one might be inclined to say that this issue alone justifies economics as a field. Indeed, we can be almost certain that the annual social payoff from replacing our current command-and-control system of environmental protection with one that makes use of market-based incentives would be far more than enough to justify all the resources spent on economics research since Adam Smith. However, except in some limited cases, we do not in fact use such measures. What does this say about the value of economics research?

Credibility and the Value of Research

A running theme in this chapter has been that policymakers often do not believe in the results of policy-oriented economics research or, at any rate, choose not to implement them. That is, economics research is widely deemed to lack the kind of credibility often (though not always) granted to research in “harder” sciences. This raises two related problems in assessing the value of such research: call them the Cassandra problem and the circularity problem.

The Cassandra Problem

Cassandra, according to myth, was cursed with the ability to make true prophecies that nobody would believe. This, of course, made her gift worse than useless.
The situation in social science is not quite that bad. Still, if Bishop Berkeley had been an economist, he might have asked: If an economist gives good advice, but nobody takes it, was the advice worth anything?

The issue of good advice not taken is a serious one. In economics—and no doubt to some extent in other social sciences—there are a number of areas in which research provides enough guidance to allow us to follow substantially better policies, which would yield a large social payoff. Yet, for a variety of reasons, those who could implement those policies do not accept the results of that research. Market-based environmental policies are the classic example: They are supported by an overwhelming majority of professional economists, and research suggests that they would have a very large payoff. Yet they have never been used on more than a small scale, mostly because crucial groups—including not only industry groups who would prefer not to pay higher taxes or emission fees, but the broader public and, worse yet, many environmentalists—reject the overwhelming professional consensus.

There are a number of other areas in economics where there is more or less a professional consensus that policy changes based on research could yield major benefits, yet these changes have not been implemented. Many involve pricing of scarce resources, such as Western land and water, rush-hour space on highways, or the electronic spectrum. Others involve such issues as the tax system, bank regulation, and trade policy. The subfield of macroeconomics is marked by much less agreement among economists than microeconomics, but even in that case there are wide areas of general professional consensus.

Suppose that the U.S. government were to actually follow all of the advice of economists in all of the areas in which economists are fairly sure that society would benefit. It would be interesting to try to calculate the gains: they could hardly be less than $100 billion annually. If we are allowed to count the benefits that society would obtain if only it listened to us, economics is an awesomely productive venture. On the other hand, if one counts only the actual impact of economic analysis on policy, the benefits are far smaller, although still quite sufficient to justify the very modest actual sums spent on economics research.

How should one assess the benefits of research in this case? It seems very unfair to researchers to declare their work worthless, even if the results are correct and offer major gains to society, simply because politicians refuse to accept their implications. Suppose that meteorologists were able to offer generally accurate forecasts of hurricane tracks, which would potentially save hundreds of lives and billions of dollars, yet politicians consistently refused to order evacuations of threatened areas. It would seem strange to call this a failure of meteorological science. Yet it is also problematic to credit researchers with all the gains that could have been achieved if their advice had been taken, when in fact it wasn’t!
And, anyway, who says that there are huge potential gains from applying the results of economic analysis? Economists, that's who—which raises the problem of circularity.

The Circularity Problem

In the 1980s, atmospheric scientists produced evidence of a relationship between the emission of chlorofluorocarbons (CFCs) and the depletion of the ozone layer, threatening a dangerous increase in global exposure to ultraviolet radiation. The result was the imposition of restrictions on the use of CFCs, which has substantially reduced that threat. This seems to be a clear-cut case of how a contingent prediction raised welfare—maybe even of how it saved the Earth as we know it.

However, there are influential people, mainly conservative political figures, but also supported by a few scientists, who believe that the science behind the CFC scare was all wrong, that the depletion of the ozone layer was a natural event. And there will never be a simple test to show that they are wrong. Assuming that the depletion of ozone does slow, they can always deny that restrictions on CFCs had anything to do with it. From their point of view, then, the research into the ozone cycle produced no benefits at all.

Such denials involving research in the physical sciences are relatively rare, although they seem to be happening more often in recent years. However, in the social sciences, the problem of circularity—that you must accept the validity of social-scientific analysis to be convinced of the benefits of applying it—is pervasive.

Suppose, for example, that economists were to reach a consensus that the United States should shift from a system of income taxes to one based on consumption taxation, and that politicians were in fact to carry out their recommendation. Suppose also, just to make life easier, that there was an actual acceleration in growth as a result, and, over the 20 years that followed the policy change, economic growth was, say, 3.1 percent annually instead of the 2.3 percent of the previous 20 years. Economists would surely argue that this increase vindicated their analysis; indeed, a growth acceleration of that magnitude would amount to a huge payoff to research into fiscal effects on growth. But economic growth varies for all sorts of reasons. How do we know that the acceleration resulted from the policy? In fact, the only way one could try to estimate the gains from the change in tax policy, even after the fact, would be to run alternative policies through a model of the economy. If politicians do not believe your model, or do not believe in economic models in general, they have no reason to accept your estimate of the benefits of the policy.

Put more generally, suppose that economic analysis indicates that policy B is better than policy A, and this advice is actually followed. How do we know what it was worth? We usually do not have a control available (a parallel situation
in which we follow A rather than B so that the difference can be readily observed), so the benefits of the policies followed based on economics research must be estimated indirectly. How? By applying the same sort of analysis that was used to make the policy recommendation in the first place.

Suppose that a critic of economics simply rejects the whole basis of that analysis. Then that critic will not only reject advice based on economics research, but will also reject estimates of the benefits of such research that require some acceptance of the results of previous research. And, as I have already pointed out, there are many such critics. I would argue that, for the most part, based on personal experience, their critique of economics is not well founded—indeed, the prevailing level of simple ignorance among the most vociferous opponents of conventional economics is hard to overstate—but to say so will not convince many of them. (Documenting that ignorance is often a useful exercise in itself, but not exactly one calculated to win friends.)

This is not a counsel of despair. By all means, the payoff to economics research should be estimated as accurately as possible, making use of economic analysis. But one should be aware that there will always be vocal critics who will challenge not only the specific research but indeed the whole framework on which the estimates are based.

The Value of Frontier Research

I close by turning to a final problem that worries anyone who tries to play a role in both policy discussion and economics research. It is very clear to me that there are large potential payoffs from the application of existing economic knowledge. A year or two of economically sensible environmental policy alone would pay for the economics enterprise in perpetuity. Yet such opportunities exist largely because politicians have failed to take advantage of well-established economic ideas: the idea of pollution taxes, for example, goes back to the 1920s, and it is a standard topic in most principles textbooks. Is there, then, any value to extending economic knowledge when we don’t use the knowledge we already have?

Even some economists seem to be skeptical about this; for example, N. Gregory Mankiw suggested to a reporter from the New Yorker (Cassidy 1996) that economists are overfunded. Because Mankiw continues to write textbooks, he presumably believes that past economics research has taught us useful things; so he must therefore be skeptical only about the payoff to further research. However, there are at least four good reasons for believing that there is in fact a significant payoff to further research. Three of them are straightforward; one follows from the very problem of good advice not taken.

Tracking the Economy

One reason why economics research is always needed is that the economy itself is always changing; simply keeping up requires substantial investments not
only in data collection and analysis but in the development of models to make sense of the changes. For example, prior to 1980, income distribution was not a major issue of economics research, chiefly because of the long stability of that distribution. To the extent that the issue was studied, those studies tended to focus primarily on the nonworking poor rather than on the distribution of wages. As it became clear that something dramatic was happening to the income distribution, however, it also became necessary to change the focus of research. Data sources such as the Panel Study on Income Dynamics, once intended mainly for poverty research, had to be co-opted for broader goals; information had to be extracted from the Current Population Survey; and new analytical issues, such as how to assess the relative impacts of international trade and technological change on wages, had to be modeled.

In the same way, while there is a long tradition of both empirical and theoretical research into economic growth, the rise of the newly industrializing economies of East Asia presented economists both with new data and new phenomena. Even though much of the work on these economies uses well-established techniques, like growth accounting, the need to apply these techniques to entirely new observations meant the need for frontier research.

Tracking Policy

Even where the underlying structure of the economy may not have changed very much, policymakers themselves often make frontier research necessary by changing policies in novel ways. More often than not, these policy changes are not carried out in response to economics research or advice; in fact, they often take place in defiance of the results of economic analysis. Nonetheless, economists need to do more than disapprove; they need to assess likely impacts.

One good example of this is the drive toward the European Monetary Union (EMU). There is an existing body of economic theory on such unions, the theory of so-called optimal currency areas. Given the real prospect of a union among major economies, however, it became urgent to go from general theoretical propositions to empirical results; indeed, there was a burst of research not only on the European economy but on that of the United States, viewed as a model of a working continent-sized currency union. General issues raised in the theoretical literature—labor mobility, fiscal integration, the size and symmetry of real shocks to regional economies—have become topics for detailed empirical work. For the most part, this work was not encouraging to the case for EMU (most notably, it turns out that the adjustment process to regional shocks in the United States takes place almost entirely through massive labor mobility, on a scale that seems unlikely in Europe for the foreseeable future), which raises the Cassandra problem. But still it was clearly important to carry out this sort of research.

Another example is the prospect for welfare reform in the United States. Here we had a major policy change that, at least on the face of it, appeared to
be a leap into uncharted territory; new research was (and still is) necessary not just to predict as best we can its consequences, but to be ready to track its effects as the policy change proceeds.

**New Areas of Coverage**

Although many—perhaps most—of the useful things economists can say have been known for a long time (it is still astonishing how much can be found in Marshall’s *Principles*), there continue to be novel developments in theory that have major policy applications.

The most compelling example in recent decades has been the emergence of models of behavior involving asymmetric information, possibly also making use of game theory to model individual choices. These models were not originally intended for direct application to specific policy issues, but they have turned out to have extremely important applications nonetheless. Models of moral hazard, for example, have been useful for making sense of (if not, perhaps, actual policy toward) such financial problems as the thrift crisis and the Lloyd’s of London scandal. And asymmetric information/game theoretic auction models have ended up being used quite directly for designing spectrum auctions.

**The Credibility Problem, Again**

I hesitate to mention it, but there is a final payoff to frontier economics research that may be even more important than any of its direct benefits. In realistic terms, economists need to demonstrate their ability to come up with interesting new ideas in order to be listened to when they give advice based on old ideas. In principle, this should not have to be so. Comparative advantage is a deep, but very well established, insight. It is difficult to understand why we cannot simply apply it. But, in practice, an economist who argues on principle, or makes use of old evidence to make the case for a given policy, is at a severe disadvantage when debating people who seem to be on top of the latest trends. If, however, the economist can point to recent studies that either confirm the validity of classic ideas in modern contexts, or demonstrate the continuing ability of economic analysis in his field to generate productive policy suggestions, good old ideas become much easier to sell.

In the area of trade policy, which is the one with which I am most familiar, this linkage has been extremely clear. Most of what economists have to say about trade policy is still the old-time religion: free trade is almost always a good idea, comparative advantage is still the most important principle to grasp. Even much of the policy-oriented current research in trade relies essentially on classical models, which have been well understood for several decades. However, the credibility of such research crucially depends on the ability of knowledgeable economists to counter critics who insist that old rules no longer apply—that technological change, or the emergence of low-wage exporters, or imperfect competition—have obviated the old principles. The best argument turns out to
be the demonstrable truth that the current generation of trade economists has, in reality, extensively researched all of those "new" issues—that this is, in fact, the cohort that brought increasing returns and monopolistic competition into trade theory and made major efforts to quantify those factors. In practice, the so-called new trade theory has not yielded the kind of innovative policy recommendations one might have hoped for. The potential payoffs just do not turn out to be large enough. But because international economists have been there and done that, it is much easier for them to argue credibly in favor of the well-established, traditional guidelines for trade policy.

This is a fairly direct linkage. However, in view of the general propensity to downgrade economic analysis, any economist engaged in public debate becomes aware that there are powerful external benefits from any and all successful economics research. That is to say, any clear win by any economist, no matter how far removed from the particular subject on which I am trying to make an impact, helps my case by raising the general opinion of economists and economics. Conversely, a fallow period in economics research, one in which recent successes are few and far between and so cannot serve as advertisements for the field to the public at large, is one in which even the most well-established economic truths are likely to be rejected by influential people. That is why, for example, microeconomists who trashed macroeconomics during its difficult days in the 1970s and 1980s made a big mistake: The relevant public does not make such fine distinctions, and the diminished credibility of macroeconomics has hurt the profession—and its ability to convince policymakers to act sensibly in all areas—as a whole. (I would in fact argue that the early successes of Keynesian macroeconomics played a role in the development of economics that was parallel to that of the Manhattan Project in the development of high-energy physics. It was the “killer app” that temporarily convinced the public that this was an enterprise worth supporting. Economists who rejected that receding legacy may not have realized it, but they were undermining the foundation of their own influence.)

Some Suggestions

This chapter has been a series of concerns and complaints. However, let me try to end with some positive suggestions. The first is that it might be useful to compile a list of policy recommendations that currently have the backing of a large majority of economists. I am unaware of such a systematic listing; my guess is that many people, and perhaps even economists, would be surprised at the size of the list. (And they would also be surprised at how many things economists agree on that the broader public thinks differently about.)

Having compiled such a list, it would also be useful to produce at least some order-of-magnitude estimates of the benefits from adopting the standard economic recommendations. Such an assessment is, of course, subject to the
circularity problem. Nonetheless, I would wager that the size of the potential gains would surprise most people, even most economists. The composition of the gains might also be surprising: they are likely to be large in some areas that currently attract little research, and small in many popular but overworked fields.

It might then be useful to compile a second list: one of policy areas that are currently controversial among economists, but where some clearly defined tests could resolve these issues, and where those resolutions might lead to valuable policy changes. Finally, it would be interesting to have an accounting of the resources actually spent on economics research. My strong suspicion, as already mentioned, is that these resources are dwarfed by the size of the potential gains, but it would be interesting to know if my hunch is wrong.

One might still conclude that the marginal benefits of research are small, either because the estimates of potential gains from applying economic analysis are not credible, or because we already know everything we need to know. But that sort of judgment would be a lot more credible if we had a sense of the resources and stakes involved.

References

4 The Relevance of Economists

ARNOLD HARBERGER

This chapter is about something that has meant a great deal to me throughout my professional life: the view of economics as a profession that has practitioners. The analogy I like best is with medicine. Law and accounting do not fit because they are not inferential; in that sense they have no scientific component. Physics, chemistry, and biology are at the opposite extreme; they are much more science than prescription. None of these fits as nicely with economics as medicine—a profession with one foot planted in medical science, the other in what we know as the practice of medicine.

What I present in this chapter may have some degree of relevance to those who pursue the profession of economics in the world of business and finance, but it is not specifically aimed to reflect their views. My aim is to represent those whom I call the "policy practitioners": those who work in the central banks and in the ministries and agencies of governments (my experience is somewhat more with developing than with developed countries), as well as those who serve as economists in the principal international organizations and in the foreign-aid agencies of this and other countries.

My links to this amalgam of types are all the stronger because so many of them—particularly those with whom I have worked most closely—are my former students, as well as close personal friends of long standing. I note this because our relationships have encouraged them to be frank with me about these matters, giving me a certain confidence in what I write here.

Returning to the medical analogy, the people I try to represent here are the general practitioners of policy economics. They are the ones who struggle in the field to harness the knowledge and insights of economic science in order to help improve the economic organization of their countries and the economic lives of their peoples. Some of them are fortunate because their efforts bring

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palpable results; they may see the reforms that they have worked for implemented; they may see government practices rationalized and improved; they may even see enough of these changes such that there is a measurable impact on the level of living of their countries. But most have to endure the frustration of waking up every morning to go out and fight battles they rarely expect to win. Where do these people find their satisfaction? It is typically in the knowledge and conviction that, for their presence and their struggle, things would be much worse. They may be able to scale down by a quarter the overall size of a project or program whose economic benefits are half or two-thirds of its cost. They may be able to stop one counterproductive policy move out of every three. They may be able to modify the wording of a bad economic law or regulation so as to blunt, if not eliminate, its worst aspects. This is the life of the typical policy economist; small wonder that after some years many end up disillusioned and drift off to less frustrating occupations and pursuits.

In addition to describing the life of the policy practitioner, I hope to evoke some visions of what those of us on the academic side of the profession might be able to do about it. This requires a sense of where we are now falling short. Happily, this part of the road has been well prepared by the work of the American Economic Association's own Commission on Graduate Education in Economics (the Krueger Commission), which was composed of 12 distinguished members of our profession, chaired by Anne O. Krueger. Their diagnosis of the problem is reflected in a few extracts from a background paper written by W. Lee Hansen, the executive director of the Krueger Commission, reporting on extensive surveys of faculty, graduate students, recent Ph.D.s, and nonacademic employers.

1. “Both faculty and recent Ph.D. respondents believed mathematical and statistical tools were overemphasized in programs from which they hired new Ph.D.s, and they held even stronger views about the profession as a whole.” (Hansen 1991, 1075)

2. “[O]nly 14 percent [of faculty members] could say that, by the time students completed their comprehensive examinations, most or all of them were good at applying theory to the real world.” (1077)

3. “[A]bout 80 percent [of faculty respondents] call for less theory and technique and more attention to applications policy. The most frequently suggested changes include: more emphasis on the links between theory and real-world connections and applications; less emphasis on technique and more on the substance of economics; and greater emphasis on writing, acquiring research skills, and doing research.” (1067)

What follows is in much the same vein, but I want to take a different approach in selling the product. In a way, I may be taking the role of a Pied Piper, trying to tantalize and tempt you to follow me down a road that I find endlessly
lively and fascinating. I want to try to convince you of important things that should be in our graduate curriculum, and not worry so much about what gets squeezed out. I have always felt that economics—as a profession, a science, and a discipline—is a very big tent indeed. I think it is big enough for all of us to fit comfortably under. In this profession, we need no concept of exile. These thoughts motivate my positive approach.

To continue, here is an excerpt from the Krueger Commission report:

Core courses [should be] taught . . . with a view to balancing breadth and depth, with sufficient attention to applications and real-world linkages to encourage students to start applying the concepts themselves. The core should be regarded as the basic unit in which those things common to all economists should be taught. . . . Field courses should attempt to include more empirical applications, using empirical findings and economic puzzles to spur students. (Krueger et al. 1991, 1052)

I agree 100 percent. I noted there should be no exiles from economics, and I meant it. But the first year of graduate work is quite another matter. What goes into the first year should all be demonstrably useful to those who will go out and practice our profession as well as to those who will be research scientists and teachers. It should impart the kind of simple and robust theoretical framework that economists will be able to use for the rest of their lives, as well as how to use it. If at the same time it imparts a belief, a faith, in the power of economics to help us see the world more clearly, thus greatly enriching our insights and understanding, so much the better.

At least the first year and the applied courses should be suffused with what one might call the spirit of economics. Much of this is captured in the old idea of "thinking like an economist." The weakening of this aspect of our training, in favor of more formal and more strictly technical material, is what worried the Krueger Commission, what worried the majority of Hansen's respondents, and what worries me.

What Do Policy Practitioners Actually Do?

There are a great many questions in economics that I have found much easier to answer than this one. So before I attempt a response, let me reveal that my underlying purpose is to start you thinking, to start us as a profession thinking about how close our lectures, our courses, our papers, books, and journals come to serving the needs of our practitioners. My tentative conclusion is that the medical profession does a far better job than we do. The classes that the medical practitioners have taken in their M.D. programs serve them better in their practice than the classes that the economics practitioners have taken in their Ph.D. programs. And, likewise, the textbooks, treatises, and journal articles written by medical scientists display more understanding of the situation of
the practitioners and end up being more relevant to the specific decisions they have to take than do our corresponding writings.

I think there is a great deal we can do to rectify this situation. In fact, it is not even intrinsically very hard to do. We simply need to be thinking a considerable fraction of the time about how what we are doing helps to serve the needs of the thousands of practitioners out there in the trenches. Those who contribute to our professional literature should be asking themselves such questions, as should the editors who help select books and articles for publication, as should our teachers designing courses and our committees designing curricula.

I think it is quite clear that we used to do better than we are now doing in these dimensions. Practitioners at the major international and foreign-aid agencies complain that recruiting is much harder than it used to be. Candidates for jobs are less in tune with policy analysis, and are less able to answer relatively straightforward questions about it, than they were 15 or 20 years ago. Fewer and fewer of these practitioners maintain their subscriptions to the major journals (even those publications of the American Economic Association that are among the least vulnerable to the practitioners' complaints), mainly, they say, because they find in them so little that is relevant to what they do.

Here is a sort of thumbnail sketch of what life is like for an economic policy practitioner. My hope is that this rapid-fire sequence of vignettes (sort of like a slide show) will help you see that the life of the economic policy practitioner is very demanding and requires sharp eyes, subtle perceptions, and artfully molded prescriptions, possibly as much as the life of the medical practitioner. The "slide show" should also help give you a sense of the distance there is between the focus, emphasis, and tone of the academic side of our profession and the focus, emphasis, and tone of life in the trenches.

General

Probably the best answer to the question of what policy practitioners do is "Lots of things—but always in a hurry and almost always at the request of others." This defines the lot of probably 90 percent of all policy practitioners. They strive to be ready to respond quickly, and most frequently to questions that are not framed in the way that seems most natural to them. They have to be able to think on their feet, and to know simple tools and how to use them well.

One thing that nearly all practitioners have to do is communicate seriously with nonprofessionals. Even when the immediate boss is an economist, those who really need to be convinced (the president, the cabinet ministers, the undersecretaries and other top administrators; the sponsors of new laws in the legislatures; and the legislative committees that draft these laws) are mostly noneconomists. So the art of communicating in general, and particularly the art of communicating with noneconomists, becomes in and of itself an important professional tool for the economist.
In the dialogue between economics practitioners and others, sometimes all the others are remote and difficult to convince. Sometimes one finds a few friendly and understanding allies. On rare occasions, the whole environment is friendly, listening seriously to the economists' concerns and vying to implement their suggestions. Whichever the case may be, one can hope that there will be the chance for fruitful dialogue on precisely the nature of any job an economist might be asked to do.

I wonder if there is anybody who has served as a consultant with specific terms of reference and has not more than once wished that the drafting of those terms of reference had been held off to the end (that is, done ex post instead of ex ante). Those who write terms of reference often do not have sufficient expertise, or they do not adequately foresee the twists and turns of dealing with the problem, or they do not really work out a full decision tree (sometimes a given result at an early step renders inconsequential much of the rest of the terms of reference).

Thus, there should ideally be a dialogue between economics practitioners and those for whom they work. Much of this dialogue will concern the design of the task to be done; some will concern next steps to pursue once some results are in; some may deal with convincing noneconomists of the limits of what we as a profession can and cannot do.

It is very easy, starting from here, to get to where the economist gives the noneconomists a seminar on comparative advantage and the virtues of free trade, on why consumption taxes are better than income taxes, and on why there should be very high peak-time tolls on urban routes. Unfortunately, such general sermonizing, even when every word is soundly based and economically valid, will typically end up achieving nothing, maybe even costing the practitioner his job. So, we have to talk of dialogue on a much more subtle level, where the economist shows sufficient sensitivity to other considerations to keep a fund of goodwill going in his favor, yet shows sufficient spine to achieve concrete improvements in his own terms of reference at one end of the tunnel and in the actual legislative or regulatory outcome at the other end.

Applied Welfare Economics

A big difference between the textbooks and the real world lies in the nature and perceptions of the constraints under which economic policy operates. Our students these days are more than facile in handling optimization problems, and it does not really matter to them whether it is optimization with only one constraint (first best) or with more than one constraint (second best).

The real world is different in that policy is usually subject to many constraints, most of them very hard to specify in clear analytical terms. Keeping a highway budget under a given limit or deciding which tariff rates to reduce in order to achieve a target average rate agreed upon with the General Agreement
on Tariffs and Trade are tasks that are relatively easy to handle using standard economic tools. Far more common, however, are problems like designing a program so as “not to offend” a whole range of different prejudices of different powerful sectors, while at the same time “placating” a series of key interest groups and maybe downright “pleasing” one target group (for example, the lumber industry) for which the legislation is mainly being designed. Also common is the fact that one must avoid, in every law or regulation being drafted, anything that appears to contradict a recent (or even not so recent) declaration of the president or of any powerful, important cabinet member. I hesitate even to note the pressures that occur near elections, when professionals are asked to try to justify all sorts of projects and other rewards thrown to so-called swing states or other electorates where the expected voting margin is close.

The above description defines a subtle shift in the nature of the problem. From a problem of maximizing the subject to one or more well-defined constraints, the problem becomes more like that of a hunter seeking an elusive prey, or a detective trying to solve a very difficult case. The abiding question, as the search goes on is, Will we find a solution that is not shot down for one reason or another?

When not involved in the search for “solutions” that may prove impossible to find, practitioners are often confronted with specific alternatives that have been confected by others, and where the principal problem is just one of choosing A, B, or C. Here, standard applied welfare economics comes into play, but we should realize that it is very likely that all of the alternatives in the set (that is, A, B, and C) fall far short of any optimum that an economist would define. Hence the art of using applied welfare economics to choose among distinctly suboptimal alternatives should be part of the basic tool kit of large numbers of policy practitioners.

Yet another task, or at least challenge, that falls to practitioners is to take solutions that appear to be emerging from complex decision processes and somehow “improve” them. I do not count it as an improvement in the context of this chapter if someone finds new ways to get more votes out of Florida or to please the lumber lobby. I do not count these because they are not natural and intrinsic parts of exercising the profession of economics. They may reflect that one is a good Democrat or Republican, or liberal or conservative, but not a good economist. Most importantly, nothing in economic science, with all its illustrious history and traditions, can tell us how to avoid contradicting the president or how to cater to the whims of a particular senator or pressure group. What our science does teach us—and what, in my opinion, it is our professional duty to reflect—is how to measure benefits and costs using the yardstick of economic efficiency. This is the economists’ genuine professional domain. If we are silent about the efficiency costs or benefits of policies, who else is going to represent them? Therefore, when I refer to “improving” solutions that seem headed down the path toward acceptance and implementation, I mean finding ways of passing
the other tests, but with greater net economic benefits or lower net economic cost (using the efficiency yardstick). If efficiency is the yardstick, the real task still includes dealing with the whole range of ill-perceived, inchoate constraints that characterize the decision process at any given moment. Perceiving and dealing with these constraints and still finding ways to improve overall economic efficiency—this is true art for the policy practitioner.¹ If there is a phrase to characterize the hurdles at this stage, it is “Yes, but will it fly?”

A related but, I think, separate task for policy practitioners is to serve as midwives as new laws and regulations are born. The period of gestation and birth is the time when they can have greatest influence on the results, because less has already been “written in stone.” Changing something that is already there means almost inevitably that its author will one way or another lose face or reputation. Convincing that person before the law is drafted or during the process of its drafting is far more benign. It is wonderful when countries have large numbers of good policy professionals at work in all the nooks and crannies of the public sector. These people silently and selflessly succeed in deleting an economically terrible paragraph, in cutting a distortion from 40 percent to 25 percent, or in changing the coverage of a tax law (or even the definition of a taxed commodity) so as to reduce significantly the efficiency cost of that tax.

Finally, but extremely importantly, a country with a cadre of trained project analysts, working in each entity or agency where projects are generated, can save vast sums through intelligent modifications of project designs at the moment they are most malleable—that is, at the drawing-board stage. An even greater task for such cadres, of course, is to save even more by nipping truly bad projects in the bud—before they ever get started.

Projections

In all of economics there is probably no task that falls to a greater number of practitioners than that of making projections. In just about every bank and business and in just about every department and agency of government, projections have a continuing role. Any activity that has a budget needs projections at least for the next budget year, and often on a multiyear basis. Any entity that undertakes investment projects needs to plan the profile of costs and benefits of each of them—at least if it is subject to decent budgetary procedures or if it has to apply for outside financing. Finally, any entity that does middle- and long-term planning will typically make projections of its likely path of development, perhaps under alternative sets of assumptions.

¹ In a work that should be read by more contemporary economists, John Neville Keynes made a trifold distinction between positive economics, normative economics, and the “art” of economics (see Keynes 1891, 55–85).
If projections are made on a perfunctory basis and are used little or not at all in serious decisionmaking, they can be done in almost any way, because they hardly matter. However, where they are taken seriously, the panorama is very different. Here, projections are like works of art, or audits by accountants: the responsible parties have to sign their names and live with the result. In these cases, economists cannot lay the blame for a bad outcome on "ordinary least squares" or on "instrumental-variables" methods. It is their task to look at all the evidence they think is relevant, to make the best inferences they possibly can, to consult the best sources for relevant information and judgment, and then to put their results down on paper. There is nothing wrong with making projections based on specified, often alternative, contingencies or by using Monte Carlo methods; indeed, the most responsible projections artists tend to work in this way. The important thing is to convey to the readers and users of the projections that every effort has been made (within the available time and resources) to take into account all of the principal factors and forces at work.

Almost as a corollary to the foregoing, the use of time-series regressions as the main device for making projections is highly suspect from the start. I am quite familiar with this because I had two major experiences with projections work. The first was to make (in 1951–52) the whole set of demand projections for the year 1975 that were used by the so-called Paley Commission (U.S. President’s Materials Policy Commission, 1952 [see especially Vol. 2, *The Outlook for Key Commodities*]). The second was when I worked in the Planning Department (later the Planning Ministry) of Panama during the entire period 1963–77, each year being responsible for that department’s projections of public-sector income and outlay.

The key element in the Paley Commission projections of 1975 materials demand was the recognition that 1950, which was perforce our base period, was nonetheless a very unusual year. We observed and predicted that in residential construction and in purchases of new cars, refrigerators, and similar durable goods (but not television sets) the 1950 levels represented peaks that would take some 20 years to re-attain. We were still, in 1950, making up for the deficiencies of stocks of those durable goods that stemmed from one decade dominated by the Great Depression and another dominated by World War II. Therefore,

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2. When the Korean War broke out in 1950, the prices of most raw materials skyrocketed. This aroused concern not just for the immediate situation, but also for the longer run. If the relatively modest demand pressure of the immediate war situation could cause such a dramatic rise in materials prices, what did we have in store when secular economic growth brought the economy to double or triple in size? To help answer this question, President Truman appointed the President’s Materials Policy Commission, headed by William S. Paley. This commission, in its seven-volume report entitled *Resources for Freedom*, correctly diagnosed the 1951–52 situation as stemming from the short-run inelasticity of supply of most raw materials. It also correctly reassured the public that long-run supplies were quite elastic and that materials shortages would not constitute a serious impediment to economic growth over the next quarter century.
most of our work consisted of finding ways to make reasonable projections of
equilibrium 1975 stock demand for housing, cars, and the key durable goods,
and of the normal growth of that demand under our general macroeconomic as­
sumptions. The flow-demand for new construction, new cars, new refrigerators,
and so forth then was built up of normal replacement demand plus normal growth
of these equilibrium stocks. We have looked back at the decade of the 1970s in
light of our projections, and I think I can fairly say we have no serious regrets;
but I shudder to think of how ashamed we would be had we relied on standard
regressions based on flow-demand over the 1920s, 1930s, and 1940s.

Continuing with the 1975 materials projections, we looked for each
material—from iron, steel, coal, petroleum, and aluminum through copper,
lead, and zinc, down to antimony, bismuth, cobalt, manganese, and titanium—at
its actual pattern of end uses. Input-output tables did absolutely no good: even
the largest table available at that time (and I believe even the largest one avail­
able today) lumped copper, lead, and zinc together into a category called “other
nonferrous metals” (separate from aluminum). So we worked with industry data
that gave the producers’ best estimates of where each mineral product actually
went. Working with these available end-use classifications, we then linked each
end-use to one or a combination of our macro variables (gasoline linked to
the stock of cars and trucks; steel use in the auto industry to the new produc­
tion of cars and trucks; household uses linked to the number of households and
per-household incomes, incorporating assumed or estimated income elastici­
ties; and so on). Where technological substitutions were in process (as in the
replacement of copper by aluminum in electrical transmission lines or in the re­
placement of tin and lead by plastics in collapsible tubes), they were in general
projected to continue. Where input-output coefficients were expected by in­
dustry experts to change (as in the thermal efficiency of electrical generating
plants), such changes were incorporated into our projections. The results were
anything but elegant, but readers could trace our precise methodology, could see
exactly what bases we touched and how we touched them, and in the end could
judge for themselves how close we had come to capturing the essential reality
we were trying to deal with.

With respect to projections of tax revenues, the direct use of time-series
regressions is almost out of the question. Rarely does a tax code stay put for
more than two or three years, yet to do a decent time-series regression one wants
at least 15 or 20 years of time-series observations, and hopefully more. How do
we proceed? The worst and least professional way is simply to forget about all
the changes (in the code itself, in the methods of enforcement, and in the level
of compliance). The best and most professional way is to work from the most
recent “base period” that is deemed reliable and to incorporate current law, ad­
ministrative practice, and compliance directly within the procedure. If they are
expected to change, they should be projected as changing; if they are expected
to stay constant, they should be so projected; if one is worried about potential
changes in these elements, they might be projected as remaining constant, but with explicit caveats. Then, one works with the identity that the receipts from a tax are equal to the tax base times the tax rate times the yield ratio (1 minus the fractions lost through failures of administration and compliance) to get the final projections.

The fallacy of using time-series regressions for projecting public finance variables is nowhere clearer than in the case of government expenditures. Here, many things change, but some are quite stable. Therefore, I do not complain about regressions explaining primary-school operating costs or the salaries of policemen and firemen. But when it comes to government expenditures as a whole, nothing can replace the hands-on approach. There was a time when the upward trend of government outlays was strong in nearly every country; in this period, time-series regressions would have yielded quite a good fit; and they also would have predicted well for a while. However, imagine how these regression-based projections would have failed in the middle and late 1980s, when major retrenchments of government spending took place in many parts of the world. During the early 1990s (see the International Monetary Fund [IMF]'s IFS Yearbook 1992, 147), central-government expenditures as a percentage of gross domestic product (GDP) reached levels less than 60 percent of their earlier peaks in Argentina, Peru, and Chile; less than 80 percent in Mexico, Uruguay, and Panama; less than 50 percent in Nicaragua; and less than 70 percent in Venezuela. None of these changes could have been captured by standard time-series analysis; all of them would have been simply flagged as part of an ordinary hands-on budget projection.

One of the big problems with most projections, and with many other aspects of the professional practice of economics, lies in the fact that we cannot pick and choose what we would like to project, or what we could readily project using a particular method. No, if we are projecting tax revenues, we have to end up projecting all tax revenues; if we are projecting imports, we have to end up projecting all imports; if we are projecting the demand for aluminum, we have to end up projecting the demand for all uses of aluminum. Inevitably, some parts of the job are easy, some are difficult but manageable, and some just lead one to throw up one's hands. Yet these last gaps must be filled in before the project is complete—before the job is done. Here more than anywhere else, the making of projections turns from science to art. Here more than anywhere else, one's natural instincts as an economist are tested. Here more than anywhere else is where the art of making projections stands most in need of help from economic science.

Diagnostics

The analogy between economics and medicine goes quite deep; one of its most interesting facets is the importance of diagnostics in both professions. Just as in medicine a doctor has to know that a heart rate can run from, say, 60 beats per minute all the way up to perhaps 90 and still be in the normal range, so too
in economics there is nothing particularly pathological about a rate of inflation of 5 percent or even of 10 or 12 percent per year. And just as in medicine a temperature of 104°F (41.1°C) should make any practitioner sit up and take notice, so too in economics real interest rates of 2 or 3 percent per month (as several Latin American countries experienced during significant periods (the 1970s and 1980s, for instance) are distinct causes for alarm. It is definitely part of the business of the economics practitioner to have a clear sense of when the signal is green (for more or less okay) or amber (for caution) or red (for danger). Once clients feel they can rely on a practitioner in this way, the best answer of all may be “Nothing seems to be wrong; don’t worry; just take an aspirin and get some rest.” I believe we all would agree that it is one of the highest expressions of the medical practitioner’s art to be able to give us such advice accurately and with confidence. Obviously, this type of diagnosis has its counterpart in economics. The key requirement here is that the diagnostician have a very good idea of what is “normal.”

Another requirement of good diagnosis is that the practitioner should be able to make subtle distinctions between situations that to the untrained eye may look almost the same. This need for sharp and perceptive observation can easily be illustrated with examples taken from the field that I refer to as real-exchange-rate analysis. To set the stage, I will define the real exchange rate of Mexico as “the real price of the real dollar.” The real exchange rate thus defined equilibrates the market for foreign currency. It tends to fall when there is a big jump in the real supply of foreign exchange, and to rise with a big increase in real demand. The big oil booms of 1974 and 1979 tended to produce a big drop in the real exchange rate (which we identify as Dutch Disease) for the major oil-exporting countries. But they did not do so for Iraq. Why not? And could a good diagnostician have predicted this anomalous result? The reason was that Iraq was maintaining a supertight licensing control over imports, which kept the effective demand down to the level of foreign exchange that was available to the licensing authorities. When the oil boom came, the licensing authority had more to distribute and did so, thus generating a jump in demand to match the jump in supply, and obviating the need for an equilibrating adjustment in the real exchange rate.

Another example: In the period leading up to the debt crisis of the early 1980s, the borrowing countries were awash with foreign exchange, and their real exchange rates reached historic lows. One diagnostic lesson was to recognize that these low real exchange rates were in most cases an equilibrium phenomenon, resulting from the abundance of foreign exchange rather than from some aberration of monetary policy. But another lesson to be learned was a danger signal. In most cases, a quick analysis of the rate of borrowing would show that it was unsustainable in the long run, that if it continued, it would produce ratios of debt/GDP and of debt/exports that exceeded the viable maxima. The debt-crisis countries should have recognized the unsustainability of their rates of
borrowing, and with it the likelihood that the equilibrium real exchange rate would soon take a big upward leap.

For the most part, these prospects were not recognized until they were not prospects, but reality. Country after country—Mexico, Venezuela, Peru, Brazil, Argentina, Chile, and Uruguay, among others—suffered the consequences. In the process of adjustment, the real exchange rate reached a peak of more than three times its pre-crisis level in Argentina, more than twice in Chile and Uruguay, and around 1.5 times in the rest of the debt-crisis countries. A similar yet quite different case was that of El Salvador in the late 1980s. Here, too, there was a flood of dollars on the local marketplace, leading to a low real exchange rate and to complaints from producers of tradable goods. However, here the story was not one of an imminent shift in that status; rather, the likelihood was that things would stay much the same for some years to come. Why? Because the sources of the flood of dollars (remittances from emigrants plus government-to-government transfers, principally from the United States) were not about to be closed down or drastically cut. Hence, export interests were cautioned to recognize the low real exchange rate as reflecting the truth of the situation now and for some time to come. Policymakers were well advised to recognize that a devaluation of the nominal exchange rate would soon be matched by inflation unless something happened to change the underlying forces of supply and demand that determined the existing equilibrium level of the real exchange rate.

I think it is fair to state that real-exchange-rate analysis came into its own in the 1970s and 1980s, in response to a dramatic worldwide increase in real-exchange-rate volatility. Another important analytical advance was the development of the concept of effective protection. Here, we learned that nominal tariff rates can be catastrophically misleading. The same 30 percent tariff on men's shirts can entail an effective protection rate of 30 percent if no imported inputs are used (say, in cotton shirts), or an effective protection rate of 60 percent if imported inputs account for half the world prices of the final product (say, in wool shirts), or an effective protection rate of 120 percent if imported inputs account for three-fourths of the world price of the final product (say, in silk shirts). These examples are special because they assume that imported inputs enter the country duty-free. The rate of effective protection would change with each change in the level of tariffs on imported inputs and also with each change in the world prices of these inputs or of the final product.

One of the big lessons that was learned from this analysis is that one can never really "plan" a set of different rates of effective protection for different goods, because they depend on too many factors that are always changing. On the positive side, however, we learned that a uniform tariff covering all imported goods—final products as well as inputs—has the unique property of providing equal effective protection to all import-substituting activities (potential as well
as actual). This result was an important factor setting in motion the recent trend in which many developing countries have moved their tariff structures sharply in the direction of equalization. Effective protection tariff rates were shown to range all over the map, from negative to several hundred percent. This was the diagnosis, but there was no sound rationale for such variation. The prescription was to greatly reduce this variation by squeezing all nominal tariffs within a narrow band (say, 10 to 20 percent, as in Mexico) or to actually achieve uniform effective protection by way of an almost completely uniform tariff (for example, the 11 percent rate that was in effect in Chile).

Another advance that greatly helped in the diagnosis of macroeconomic problems was the monetary approach to the balance of payments. The starting point here was to recognize that when people have undesired real cash balances they tend to spend them, and when real cash balances are too low, people modify their spending patterns in order to augment them. The next step was to note that when people were working their real balances down or up, an important part of the affected spending was on tradable goods. Under a number of different exchange-rate systems—a traditional fixed rate, a crawling peg, or a tablita (which pre-programs the time path of the nominal exchange rate)—these changes in spending on tradable goods would end up being reflected in the balance of payments. Recognizing this simple sequence has enabled economists to diagnose much more clearly the macroeconomic situation in countries where it is operative.

What Makes Practitioners Feel Isolated?

If there is any quick answer to this question, it is the sense that the academic branch of the profession, in the classrooms and in our journals, reflects its own sense of priorities and its own hierarchy of values, with little empathy for those of the practitioners. It would be wrong to try to put one's finger on a single source of the problem. Some practitioners complain of too much mathematics, some of too much emphasis on methodology. I have used the word "super-technicism" to describe one source of the perceived distance between the two camps. But all these are manifestations of the problem, not the problem itself. I can see all these elements persisting in the profession, without creating any serious problem for the practitioners.

As I interpret the situation, all the issues here could be easily resolved, just by giving "equal billing" (or even "closer-to-equal billing") to the concerns of the practitioners in the various hierarchies that we have set up in the economics profession. How can we better instill in our students (and other members of our guild) a good sense of what is "normal" behavior in all the main dimensions of economic life? How can we better recognize outliers, and most particularly those outliers that bode trouble? How can we send our graduates out into the world better prepared to take on tasks that with high probability will fall to them, such
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as the making of economic projections? My answer is, by paying more attention to these matters in our classrooms, journals, books, and conferences. But to pay more attention, the academic profession somehow needs to be sensitized to a new set of challenges, a new set of legitimate claims on its attention.

Here is a simple example of how our thinking is slanted. When one has a complaint, it is usually best to admit one's own delinquencies. Therefore, I present a brief sketch about the economics of exhaustible resources. If one asks a sample of graduate students or of recent Ph.D.s what they understand about the economics of exhaustible resources, nine out of ten will probably recite the theorem of the principle of extraction: the real price per unit of the stock of resources in the ground must be expected to rise at the real interest rate; any lesser rate of price increase would make immediate extraction the best policy. And I must admit that my own students would have responded just the same. Throughout many years of teaching price theory, I taught this vision of exhaustible resource because it was so neat, so clean. (And besides, it generated such nice exam questions!)

However, let us now stop and think about it. We surely have to assign to mining assets a level of risk as great or greater than, say, the average risk of New York stocks. This means that mineral assets should rise in value at a real rate of at least 7 percent per annum or so (the long-term yield of New York stocks). Seven percent doubles every decade, multiplies by 32 in 50 years, and multiplies by about a thousand in a century. Now, the challenge is to find the minerals whose implicit price-in-the-ground behaves like that. I cannot say I have made an exhaustive search, but certainly a fairly good one, covering most well-known mineral names, and I have yet to find the first mineral whose price-in-the-ground appears to have risen secularly at the real interest rate. (Price-in-the-ground is the price of the extracted ore minus marginal costs of extraction, or price of the processed mineral minus the relevant costs of extraction and processing.)

Does this mean we should stop teaching the beautiful, clean, neat, simple theorem that we all learned? I think not. Rather, we should teach it as a door-opener, to be followed by a demonstration of its massive failure to explain the facts of the last century or so, to be followed in turn by an explanation of some of the more plausible reasons for this massive failure. The theorem itself implies that, if prices are not expected to rise at the required rate, there is no point in holding inventories of minerals in the ground. Under those circumstances, one should extract the minerals rather than hold them, the speed of extraction being governed more by considerations of extraction costs than, as the theorem states, by the benefits of holding per se. This is what we tend to observe. All this makes more sense when one builds in the facts that the available amounts of exhaustible resources are not known, that mineral discoveries are constantly being made, and that technological substitutions often trigger dramatic drops in mineral prices.
What Can the Profession Do?

The vignettes presented at the beginning of this chapter are supposed to give readers a sense of what it is like to be a policy practitioner—of what sorts of abilities that role calls for, and to what kinds of demands practitioners are typically asked to respond. The exhaustible-resources example is supposed to provide a sense of how the academic side of the profession at times distances itself unwisely and unnecessarily from the world of the practitioners. In the process, I hope that readers are more sympathetic to the practitioner's lot and at the same time more aware of the high order of the professional skills that are required. From both angles, I hope readers feel now more than before that those of us on the academic side of the profession should pay more attention to the needs of the practitioners and should try to serve them better.

What follows is another "slide show," another set of vignettes, this one designed so that readers can weigh the merits of each successive thought or suggestion, and to stimulate thoughts related to developing ways of providing greater academic support and improved scientific foundations for the things practitioners do.

General

Very high on the list of ways to serve the practitioners is the simple effort to focus more on results and less on methodology. At least when we are dealing with something relatively new, there should actually be a premium on looking at it from various angles, using "all the evidence at hand," rather than placing all the weight on one particular approach or method. When we are not looking at something new, our task should be to state quite clearly wherein and how our results are superior to those that went before.

From a methodological point of view, we should try to implant the principle of minimalism. This principle can be summarized as the economists' version of the old saw, "You don't use an elephant gun to kill a mouse." Our search should be for methodological simplicity, not methodological overkill. Wherever a straightforward application of supply and demand will do the job, there should be no need to go to anything more complicated. Note that this point does not contradict the first, which deals not with methods as such, but with the range of evidence that we examine.

As a corollary to the principle of minimalism whenever we go to a more complicated methodology, we should be able to show how and why the extra complication is needed. I suggest this may be harder to accomplish in reality, because very often the colleagues who best manage an advanced technique are not the same ones who have the aptitude to get the most juice out of the lemon using supply and demand or some other simple approach. So, in reality the scenario might work like this: A introduces a more complicated methodology that appears (on analysis) to be needed to accomplish the objective at hand,
and B responds in a later journal article or comment that A missed a particular trick or twist, and shows that A's task could still be performed in a very simple framework. This scenario leads to a plea to journal editors and referees to modify their standards and priorities in order to give full status to this type of interchange.

Applied Welfare Economics

As one who has written and taught in the field of applied welfare economics for more than four decades, and on the subject of cost-benefit analysis of investment projects for more than three decades, I feel quite safe in stating that, in the United States, we do not have either the amount or the kind of training that is needed for our profession to make its appropriate contribution to our society's decisionmaking processes. Our society needs thousands of well-trained project analysts with good backgrounds in analysis, with the ability to move quickly to sound quantitative judgments and to devise intelligent scenarios that are tailor-made for each problem at hand. Where we need thousands, I doubt that we even have hundreds. The result is that there is a low level of professionalism in practice, with much work of highly variable quality passing as if it meets the most serious of standards. We need to take cost-benefit analysis more seriously, emphasizing those areas where current technique is adequate to the task. Starting from this base, we have to build large cadres of knowledgeable professionals who can see to it that our work in the field meets adequate standards. At the same time, we need a significant volume of academic work to extend the frontiers of cost-benefit analysis to new areas.

In our teaching, we should see to it that the typical graduate student emerges with a working knowledge of basic applied welfare economics, with emphasis on the measurement of deviations from optima, rather than on the elegant derivation and specification of the optima themselves. This does not mean giving up a lot, since if one can measure the costs and benefits of any situation, it is easy enough to analyze whether a given step is a net plus or a net minus. If you apply this type of analysis, in most cases you are carried to a definition of the optimum as that point where no marginal step in any direction will increase net benefit. The important thing here is to focus attention on nonoptimal—indeed, on far-from-optimal—situations, because that is what the student will encounter in the real world.

When we do modern political-economy analysis, we should avoid any suggestion that it has the patina of a welfare optimum. My colleague George Stigler often pointed out that for 200 years we economists have won the debates on free trade in the lecture halls, but the protectionists have won the battles in our legislatures. The inquietude reflected in this observation led to much fruitful research on why some industries succeeded in winning high tariffs while others did not; why some gained beneficent regulatory treatment and others did not, and so on. At the core of this political-economy analysis is an
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exposition of the interests of those in government (the interests of the regulators, the interests of the legislators, and so on), and how these interests mesh with those of the industries being regulated. All of this is a great aid to our knowledge and understanding, and as such is to be applauded, but I reiterate that we must avoid any suggestion that the outcome of such interaction of interests is beneficial. Having a knowledge of the mechanics of how the U.S. sugar industry has been able to secure protective legislation in its behalf for many decades is beneficial for our economists and citizens, but the best use of that information is to help us manage a better struggle to tear down those protective walls, not to justify their continued existence. In fact, modern political-economy analysis is neither an outgrowth of, nor a substitute for, standard applied welfare economics. The latter remains an essential component of the tool kit of economics practitioners.

Projections

The art of making projections is rich and varied, but it certainly is not well represented in the economics literature. It fits rather nicely in a Bayesian framework, however, because one can think of changing one's "priors" with respect to certain older information, or with respect to the results of a regression, as new information comes in. As I think of projections methodology, I return to the old and very fundamental economics vision that sunk costs are sunk, that bygones are bygones, that economic activity is forward-looking. This is what leads one not to look on any mechanical, repetitive procedures as giving the final answer.

Looking for ancillary evidence is a big part of the art of making projections. Comparing flows with stocks of durable goods can provide good clues as to whether we are dealing with a situation of full equilibrium, or whether our present production rates reflect a transition from one static equilibrium to another. Likewise, analysis of the fundamentals of a commodity market can help reveal when the actual price is unsustainably high or low. In such cases, we often perceive the process of overshooting: learning to recognize overshooting when one sees it is also an important part of the art of making projections. (Please note that recognizing an overshoot implies only that one observes, say, that prices are above the long-run equilibrium price. It does not in the least imply that one knows the time path by which that equilibrium will be reached.)

This point fits somewhat uncomfortably under the heading of projections, but I feel it is important enough that it should be included somewhere. In recent years, our profession has become enamored of dynamics, as never before. The question is, what sort of goals do we set for ourselves as we explore this fascinating but potentially treacherous terrain? I have a humble analogy that I present in all seriousness. If I am in my office at 5:00 P.M., it is almost a certainty that I will be home by 8:00 P.M. What is uncertain is exactly when I will leave the office, when I will arrive home, how long I will take on the way, what route I will follow, and what intermediate stops I will make. Of all the propositions that
I have heard in economics concerning dynamics, the most profound by far is Marshall's distinction between the long run and the short run, and in particular his vision of the short-run situation being one of equilibrium and disequilibrium at the same time—an equilibrium with the arrows of change built in. My question is, can we not emulate Marshallian analysis in our empirical work on dynamics? That is, can we define a short-run situation by the indicated direction or directions of change of one or more key variables and test our hypotheses by checking how often the movements are in that direction? This sort of "qualitative dynamics" would capture the reality that many of us perceive far better than a quantitative dynamics that tries to predict not only the entire path of adjustment, but also the speed with which it is traversed.

Diagnostics

The first step in teaching people how to diagnose different situations is to teach them how to observe, in the same way, perhaps, as a hunter would try to teach his children how to scan the horizon and what to look out for. I know of no true substitute for hands-on practice in this area, but it is surely better undertaken with the help of an experienced guide (or at least a good guidebook) than to try to go it alone. Therefore, in our literature and in our classes, we should become more articulate about how to observe and how to diagnose.

For those interested in the economics of different countries, I know of no better starting points than reading the IMF's *International Financial Statistics (IFS)* and the World Bank's *World Development Report*. Sheer immersion in the data presented in these publications will set novices on the right track, with even better results if they have someone to guide them. I am fascinated by the thought of a course in macroeconomics that would use the *IFS Yearbook* as its principal text. This yearbook gives annual data for some 150 member countries, covering, where feasible, the past 30 years on a vast array of subjects: monetary and banking institutions and magnitudes; national income and product accounts; and trade and the balance of payments, prices, wages, interest rates, and exchange rates. From such data, one can derive first-hand notions of what is normal and what is not—of when the light is green, amber, or red—and can also get a good sense of the nature and source of the trouble, when trouble is present.

The world of diagnostics has its own vocabulary, in which syndromes and scenarios play a key role. These have no formal definition for economic applications, but I prefer to think of the syndrome as the simultaneous presence of a combination of symptoms, which one could read from the data of (or up to) a moment in time. I also prefer to think of the scenario as a sort of motion picture, in which the forces present in a given situation are released in such a way as to tell a quite natural story. Titles of some of the papers I have written include "The Inflation Syndrome" and "Some Debt Crisis Scenarios from Latin America," and one can also envisage scenarios for trade liberalization, or for specific
types of fiscal reform. For devaluation crises, one can identify both a syndrome, signifying the need for major real-exchange-rate adjustment, and one or more scenarios, showing how the story might play out once a major (or perhaps minor) devaluation is undertaken.

The label "diagnostics" suggests a lot of what might be called a practitioner's worldview of economics. Once applied to economics, it automatically classifies it as a fundamentally observational discipline, in which learning about and dealing with the world is the name of the game. Add to this the awesome truth that the real world is far too complex to be encompassed in any single model or vision, and you get another practitioner's adage: in the face of the world's complexity, we have to divide it up into components that are simple enough for us to comprehend—simple paradigms to deal with complex reality. These have always been a part of the teaching of economics, though they may have faded into the background in recent years, as the tone of our discourse has turned increasingly technical. Syndromes and scenarios might be a way to restore those paradigms to a more central role as part of a conscious effort to reinvigorate economics as an "observational discipline."

My focus in pleading for simple paradigms to deal with complex reality does not mean that we should embrace a crudely partial-equilibrium approach. Far from it: he who deals with reality can never forget the wholeness of our complex world. But just as the orthopedist can look mainly at knees and ankles while keeping in mind their relationships to the rest of the human body, so too economics practitioners learn to explore minute details in the sector they are studying, while embedding that sector in a wider economy that is sketched with much broader strokes. The practitioner's way of thinking is probably more deeply rooted in the focused general-equilibrium analyses used in macroeconomics and in international trade than it is in the Walras-Pareto type of vision that treats equally all of the many branches or sectors of the economy.

As one delves into the world of syndromes and scenarios, particularly for purposes of policy analysis, whole new vistas emerge. How do governments get themselves into trouble with inflation? Or with the real exchange rate? Or with social security? To a great extent, most of the evidence suggests that policymakers face temptations and moral dilemmas much like those we all have to confront daily. One of my favorite devices for studying developing-country inflations is a variable I call \( \beta \), which expresses the increment of banking-system credit to the government as a fraction of each year's GDP. This variable goes beyond mere money creation to what is in many cases one of its main underlying causes. I have set up diagnostic categories of stable countries, chronic-inflation countries, and acute-inflation countries on the basis of their actual inflation experience. For each category, I set out the frequency distribution of \( \beta \) for the relevant countries and years. In three successive nonoverlapping data sets, one covering the years 1950–75 (Harberger 1981), the second covering the years 1976–84 (Harberger 1988), and the third covering the years 1985–90
(unpublished), the frequency distribution of $\beta$ for the acute-inflation countries lies to the right of that of the chronic-inflation countries, which in turn lies to the right of that of the stable countries. So, old Calvinists like myself can say that sinners reap their just reward. However, we cannot be pompous or dogmatic about it because, unfortunately for the black-and-white vision of the world, the distributions overlap a great deal. Typically, the third quartile of $\beta$ for the stable countries lies above the first quartile for the chronic-inflation countries, and the third quartile for the chronic-inflation countries lies above the first quartile for the acute inflation countries. This is similar to the medical evidence on smoking, drinking, and diet. Their influence on our life expectancy has been clearly proved, yet some three-pack-a-day smokers live to be 90, as do some pint-a-day drinkers and some well-known gourmards.

Faced with this sort of evidence, governments often fall prey to the temptation to take the easy road and drift into an inflation syndrome, hoping they can get away with just a little more borrowing from the banking system, just one more time. In this world of varying shades of gray, it also becomes harder for advocates of prudent policy to hold the line against their more permissive colleagues. This is why qualities of character such as fortitude and determination tend to be of key importance in distinguishing the great economic ministers and central bankers from their run-of-the-mill counterparts.

**Conclusion**

I hope that in this chapter I have gone a step beyond just pleading for the profession to pay more attention to the professional practice of economics in our teaching, in our research, and in our literature. That extra step has been to show that doing so can be interesting, even fascinating. In short, it is fun to face the challenges and puzzles that abound in the world of the practitioner.

The vision I hope readers will carry away from this discussion sees economics as fundamentally an observational discipline. An important part of our instructional task is to teach students how to be sharp and perceptive observers, to recognize and respond to telltale clues, and to have command of tools that are simple and versatile, so they can react on the spot, help avert disasters by timely action, and in general fix things quickly when they go wrong.

A related vision is of the learning process of practitioners. My perception is that the best practitioners have a certain worldview in their heads. All sorts of crazy things can happen—like hyperinflations and huge recessions and wrenching debt or exchange-rate crises. All of these, plus many more rare and apparently anomalous events, can occur and still leave seasoned practitioners unruffled, because their worldview already contains sensible explanations for them. Every now and then, though, something happens that does not fit the prior image—something that shakes our Bayesian faith in what we used to think. In my view, this is how most practitioners build their expertise, how entities like
the IMF and the World Bank learn as institutions, and how we ought to teach our students to learn.

I hope I have successfully evoked these visions. If we move to link the academic profession more closely to the world of the practitioner, we will enjoy it, our students will profit from it, the discipline of economics will be richer, and the world may even end up being a somewhat better place to live in.

I also note that over the past few decades we have witnessed quite a number of economic success stories, the great majority of them linked to economic policies that were far better than what went before. We not only experienced the Far Eastern dragons of Hong Kong, Singapore, Taiwan, and Korea, but also Indonesia, Malaysia, and Thailand; other success stories include Spain, Portugal, Greece, Turkey, Chile, Uruguay, Mexico, and now probably also Argentina.

I did not build this chapter around such success stories because I wanted to focus more on the daily lives of practitioners, on what it is like to live in their world. I wanted to show that their lives can be enormously rich and full (in a professional sense) even though most of them never savor the grand triumph of participating in the successful transformation of an entire national economy. But the evidence of one national success story after another should not be left unmentioned, for it shows that major economic transformation—the ultimate fruition of the struggles of our practitioners—is not beyond our reach. This fact should only add to our enthusiasm to give the practitioners of economics, and the study of their craft, as much of an honored place within our discipline as we and others (including medical teachers and scientists) give to the practitioners and the practice of medicine, our sister discipline.

References


Social science research is widely regarded as providing substantial benefits to individuals and to local, regional, national, and international communities. Some social scientists have identified a broad array of categories of such benefits. But they have not often done this in a systematic fashion. On occasion, they have asserted that, if estimated using a monetary measure, these benefits would be large. For example, one claim made for the often-maligned field of macroeconomics is that, as a result of the insights of researchers ranging from John Maynard Keynes to Milton Friedman, developed economies have avoided catastrophic recessions and rampant hyperinflation since the end of the 1930s. By anyone’s accounting, if macroeconomic research has allowed Europe and North America to avoid even a 5 percent decline in gross national product (GNP) in only one of the last 20 years, those savings will have more than paid for the salaries of all social science professionals in the entire postwar period.¹

This does not necessarily mean that too little has been invested in social science research. The ex ante optimal amount of social science research, as with physical science and technology research, can be ideally determined (assuming a risk-neutral decisionmaker) by the intersection of the relevant marginal expected benefits and marginal expected cost curves. It is not the integrals of the marginal expected benefits and cost curves that matter, but the marginal curves themselves.

The above example shows that social science research can yield large benefits for individuals, households, and communities. But social science research can be a double-edged sword. For example, when influential economists are wrong—as economists in the late 1950s and early 1960s clearly were about the policy implications of the Phillips curve—the costs of poor research can be

¹. This observation is not new. Ruttan, for example, emphasized the importance of economists’ contributions in the field of macroeconomics, arguing that one of the most dramatic examples of the effect of new knowledge in the social sciences on institutional efficiency can be seen in the new understanding of macroeconomic relationships associated with the Keynesian revolution (Ruttan 1982, 339).
large. If inflation is costly, then social science research is cast in a more dismal light. In the 1960s, the policymaking response to the argument for the existence of at least a semipermanent trade-off between unemployment and inflation resulted in unwarranted increases in inflation rates in developed economies for almost 20 years. Paul Krugman's observations (1994) on the roles of policy peddlers are relevant here. The damage done by social scientists who, either by accident or intent, pander to the preconceived agendas of politicians can be substantial.

This suggests that social science research can generate both positive benefits, in the form of improved policies for managing economic systems, and negative benefits, in the form of poor policies, over and above any "production" costs associated with carrying out research. Some social science research wells are not merely dry, but poisoned. The relevant question, especially in relation to policy-oriented social science research in general and the programs of specific institutions, is, therefore, "What are the net benefits of social science research?"

The purpose of this chapter is to identify alternative methods for assessing the contributions of social science research, especially in light of the above concerns. Thus, the chapter examines two major questions. First, what are the benefits of social science research? Second, how can those benefits be estimated? One can argue that, in principle, social science is no different from research in the physical sciences because it provides new knowledge that alters the economic welfare of households. Moreover, like science and technology research, the initial impacts of social science research often alter total factor productivity within firms, households, and government agencies. However, social scientists have given little effort to measuring the effects of their work and, therefore, the field of estimating social science research benefits is virtually uncharted territory, perhaps not least because it requires the investigator to examine the house he or she inhabits.  

At the outset, it is worth noting that assessing the benefits of any R&D may either be a prospective (or ex ante) task or a retrospective (or ex post) endeavor. Ex ante assessments are typically needed by research administrators and research policymakers in deciding how to allocate funds between different areas of research and, within any given area of research, between projects. Ex post assessments are also relevant, both in the context of impact assessment and as important elements in assessments of the potential benefits of future research investments. In the social sciences, where improved information is often the major output of the research, ex ante assessments are likely to focus on the economic

2. Although little serious effort has been devoted to this problem, economists and agricultural economists have been willing to identify important research agendas for their disciplines (see, for example, Bonnen 1983; Vickrey 1993). Rare exceptions in the field of agricultural economics are studies by Freebairn (1976a, 1976b), Norton and Schuh (1981), Ruttan (1982), Roe and Antonowitz (1985), Sumner and Mueller (1989), and Carter and Galopin (1993).
value of the new information, often in a Bayesian decisionmaking context (see, e.g., Gardner [Chapter 9 in this volume] and Lindner [Chapter 7 in this volume]). Ex post assessments may well use methodologies that differ in some respects to assess the economic impact of previous social science research because a Bayesian framework is less relevant. These methodologies include growth accounting, productivity, and economic surplus approaches.

Social science includes many disciplines and subdisciplines—anthropology, economics, history, geography, psychology, and sociology. This chapter focuses primarily on economics and agricultural economics and, in particular, on the benefits associated with policy-oriented research rather than "pure" social science research.

A Preliminary: Measuring Basic and Applied Research

Before addressing the two major questions that concern us in this chapter, we consider the empirical utility of the typology of basic and applied research for measuring social science research benefits. One reason for doing this is that, while there is a well-developed body of knowledge on measuring the benefits of applied research, there is a consensus that measuring the benefits of basic or pure research (which is largely a public good) is much harder. It is more difficult in part because identifying the amount of basic research inputs, the immediate changes in the stock of useful knowledge that result from those inputs, and the effects of these changes in the stock of useful knowledge on economic welfare all present extremely difficult problems of measurement or estimation. In particular, if the effects of basic research are to be identified separately from the effects of applied research, then there should be some sensible method of determining what constitutes basic and applied research that can be used to produce effective measurements of the investments in each type of research.

In practice, it is difficult to distinguish between pure and applied research, especially in the social sciences. While some economists specialize in abstract mathematical modeling (for example, in the areas of game theory and general equilibrium modeling) or, alternatively, in highly applied research (assessing the economic feasibility of a food processing plant in a specific location), most scholars include both basic and applied elements in their research. Much work that appears to be applied policy analysis or empirical research often provides the direct or indirect basis for the research programs of others.

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3. The economics of the benefits of applied scientific research is now a significant subfield of economics and owes much to the pathbreaking work of Theodore Schultz (1953) and Zvi Griliches (1958). An exhaustive review of this work as it relates to agriculture is provided by Alston, Norton, and Pardey (1995). Assessing the effects on factor productivity of all research is a more difficult exercise, at least in part because of the difficulty in measuring knowledge and changes in the knowledge base over time (see, for example, Adams 1990).
Unlike specialized applied scientists such as plant breeders, who produce tangible and physical product innovations like yield-improving crop varieties, the research of many social scientists is both basic—in the sense that sooner or later the insights become part of the foundation of other research—and applied—in the sense that they address a current empirical or policy issue of interest to a “client.” An effort to obtain meaningful empirical measures of separate aggregate investments in basic and applied social science research, therefore, is likely to be a sterile exercise. In the same way, approaches to the measurement of social science research benefits through an assessment of the effects of basic and applied social science research on total factor productivity may prove to be difficult to apply, even at modest levels of aggregation.

**The Benefits of Social Science Research**

Categorizing aggregate investments in social science research into the “usual” broad categories of basic and applied research can be an empirical nightmare. Categorizing the benefits that flow from those investments might be easier. Moreover, there is more literature about this, at least in the fields of economics and agricultural economics. The current literature includes two approaches to the problem of identifying the benefits of social science research. One is to develop “grocery lists” of different types of benefits (Norton and Schuh 1981). The other emphasizes the importance of social science research in bringing about institutional change (Ruttan 1982, 1984, 2003). In this section, both of these approaches are reviewed. However, an alternative approach is recommended, which is to focus directly on the effects of social science research on economic welfare and to identify the effects of social science research through their initial incidence on particular sectors of the economy. This is, after all, a necessary precursor for any empirical assessment of economic impacts.

Norton and Schuh (1981, 249) argue that “A common thread running through most types of social science research is that the output is information rather than a new or improved product.” They clearly did not mean to imply that information is not a commodity, but that social scientists, unlike some physical scientists, in general do not directly produce new physical products other than the products needed to create and transmit information, such as books, reports, computer software, and articles.

Norton and Schuh also suggested a “grocery list” typology for categorizing the information through which agricultural and rural communities derive the benefits of social science research. One category, management information, enables producers to implement initiatives that improve technical and allocative efficiency. A second category, price information, improves the accuracy of price forecasts and leads to improvements in allocative efficiency. A third is institutional information, which makes it possible for policymakers to implement welfare-enhancing changes in economic institutions or “rules of the game.”
Information on product and environmental quality improves household consumption choices and decisionmakers’ policy choices. Norton and Schuh’s three other categories are nutrition information, information to aid adjustments to disequilibria, and information to aid in the reduction of rural poverty.

The real theme that underlies this typology is that social science research produces findings that alter the economic welfare of individuals, households, and communities, largely through improvements in total factor productivity (TFP). TFP is broadly defined here to include effects on the productivity of households, government, and the private sector. Only in the case of information to aid in the reduction of rural poverty do Norton and Schuh deviate from this focus—at least in part—to address income redistribution.

In contrast, Ruttan (1982, 1984, 2003) argued that social science research is important primarily because it fosters institutional change, almost to the exclusion of any other type of benefit. Following Commons (1950) and Knight (1952), he defined institutions to include “[B]oth the behavioral rules that govern patterns of relationships and actions as well as decision making units such as government bureaus, firms and families” (Ruttan 1984, 550). Institutional change, in this definition, refers to the creation or destruction of institutions and also to changes in institutional performance. If institutions and institutional change are defined in this manner, then Ruttan is correct. Any outputs from social science research or, for that matter, physical science research are accounted for, but the “typology” provides few insights. If we consider Ruttan’s examples, however, it seems clear that his focus is on major changes in the organization of political institutions and government policies. In this interpretation, Ruttan’s argument is really that social science researchers’ major contributions are to social changes on a large scale rather than on a small scale.

Ruttan suggests that the first step in measuring the benefits of social science research should be to specify the sources of demand for that knowledge. He then argues that the demand for knowledge in economics and in the other social sciences—as well as in related professions such as law, business, and social service—is derived primarily from the demand for institutional change and improvements in institutional performance (Ruttan 1984, 551).

Ruttan’s optimism about “improvements” may not be completely justified, perhaps mostly because the examples on which he focuses refer to large-scale changes in either political institutions or the policies that decisionmakers implement (Ruttan 1982). Krugman (1994) has more recently noted that the public choice literature on rent-seeking implies that ex ante demands often exist for social science research justifying certain institutional changes that will benefit

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4. Douglass North has emphasized the importance of institutions throughout his career. He gave it particular attention in his Nobel laureate address in 1993 (see Appendix Table 2.5).
the parties seeking to make those changes. However, the resulting changes do not necessarily improve aggregate economic welfare. Thus, the observed “market” demand curve for social science research is unlikely to be the social demand curve. This divergence between the social demand and the market demand for social science research should be taken into account in any assessment of benefits.

In fact, if the concern is with the benefits of publicly funded social science research, then the relevant question is, What are the effects of the research on the economic welfare of all affected parties? It may be worthwhile for wealth-maximizing social scientists to have a understanding of the aggregate willingness of the “market” to pay for the rationales for institutional change that participants in the policy process desire, but it is unlikely to lead to an accurate measure of the aggregate benefits of social science.

From an empirical perspective, Norton and Schuh’s approach may be more useful. However, Ruttan’s observation that the benefits from social science research flow to individuals and communities through effects on firms, households, and government agencies (or bureaucracies) is also important. Moreover, we know that the effects of social science research on economic welfare really constitute the benefits of that research. We have thus adopted the approach here of categorizing the outputs of social science research in terms of both separate, or sector-specific, effects and joint effects of the research on the TFP of firms, households, and government agencies and, as a result, on the economic welfare of individuals and communities.

Sector-specific effects initially affect a particular type of organization—the firm, the household, or a government agency. They can be viewed as changing the costs of the resources that each type of organization needs to produce its outputs. It is clear, however, that innovations that have their initial incidence on a specific sector lead to benefits for other sectors, and those benefits must be accounted for in any assessment of benefits (just as they would be in the case of innovations derived from general science research).

Joint effects have their initial incidence on more than one type of organization or sector. They often derive from institutional changes such as adjustments in social and economic policy (e.g., “reforms” to agricultural income-support policies) associated with social science research.

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5. This insight was provided to the authors by Julian Alston in personal communications.
6. In other words, as Alston, Norton, and Pardey (1995) argued regarding assessments of the benefits of science and agricultural science research, social science research effects should be measured and evaluated using the standard techniques and criteria of positive welfare economics.
7. Oehmke (1995) noted that what makes social science research different from much physical science research (especially in agriculture) is that it is explicitly designed to affect people and is concerned with improving individual and social welfare by influencing people’s actions. This view implies the need to focus on more than private sector productivity in assessing the benefits that such research provides.
Regardless of their origin, the impacts of social science research should be viewed within both a static and a dynamic context. Gains in resource-use efficiency occur over time. For example, economic research leading to policy changes that lower barriers to entry in an industry facilitate greater competition. The increase in competition provides incentives and rewards for innovators and provides penalties for those who do not innovate. The effects of increased competition on economic efficiency are clearly dynamic in the crucial sense that the size and distribution of the economic benefits change from one time period to the next as technology-adoption rates change in response to procompetitive policies. Procompetitive policies include changes in antitrust legislation, lowering international trade and international capital flow barriers, removing statutory monopoly powers for state trading enterprises such as marketing boards, allowing competition in public utility industries, and tendering for the supply of government-funded goods and services.

**Separate Effects of Social Science Research**

As with any typology, the clarity of the demarcation lines and how they are drawn and used is subject to question and criticism. However, the emphasis on three types of organization, or sectors—firms, households, and government—has the virtue of being in accord with widely used constructs in national income accounting and models of economic systems.

**Firms**

Social science research provides many different kinds of information that influence the economic welfare of individuals and aggregates of individuals. As Norton and Schuh noted, some of this information can directly affect the supply curves for individual commodities by enabling firms to use resources more efficiently. Norton and Schuh's categories of management information and price forecast information are both relevant here. In fact, general market information, which includes crop forecasts, trade policy updates, and so forth, is an additional important source of potential benefits through its effects on the productivity of firms.

In an agricultural context, for example, management information includes results from applied economic research on the capacity, configuration, and location of processing plants; the management of livestock herds and fisheries; the economically efficient use of inputs such as land, fertilizers, and pesticides; the development and use of new marketing instruments such as options, futures, and derivatives; and the adoption rate for new technology. Social science

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8. Note that only by a considerable stretch of the imagination can these types of contributions, which in the past have been substantial, be subsumed under the rubric of institutional change.
research has contributed to increases in the TFP of firms in these ways (and in many others). The result has been an increase in economic welfare for households through changes in the welfare of both producers and consumers.

Several commentators have identified market information, especially forecasts of prices, as a potential source of benefits from social science research (Freebairn 1976a, 1976b; Norton and Schuh 1981; Antonowitz and Roe 1986; Sumner and Mueller 1989; Colling and Irwin 1990; Carter and Galopin 1993). Much of this information has the characteristics of a public good, and the socially optimal price for access to that information is either zero or close to zero and below the average cost of providing it. It seems unlikely that economists' direct contributions in the form of improved price forecasts (through "better" structural or time-series statistical models of prices) have been, or are likely to be, substantial, at least where futures markets exist. However, better information on underlying production conditions may be more important.

One indicator of the value of U.S. Department of Agriculture (USDA) data on agricultural commodity markets may be the extent to which private firms include that information in newsletters and in other media that they market to agricultural producers, processors, and input suppliers. Another approach, considered by Carter and Galopin (1993), is to examine private returns by market participants (traders on futures markets) to early access to data contained in the reports. Freebairn (1976a, 1976b), Norton and Schuh (1981), and Antonowitz and Roe (1986) have all addressed the question of how to value improved price forecasts under the assumption that improvements in forecasts are shown by reductions in the dispersion of the probability density function associated with the forecasts. Although, as suggested above, it seems unlikely that economists do better than private agents in directly forecasting prices, social science researchers do produce improved market information such as crop forecasts, which indirectly improves the price forecasts of private agents. A third approach to valuing the benefits of this type of market information may be to

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9. There is little or no evidence that time-series and other econometric price forecast models do better in predicting future price movements than do participants in commodity markets through futures prices or even expert forecasts.

10. Sumner and Mueller provided compelling evidence that between 1961 and 1982 futures price changes were significantly larger on the days when the U.S. Department of Agriculture (USDA) released its crop reports for corn and soybeans proportional movements than on the five days before and after the crop forecasts were released. They concluded that the USDA's crop information reports do contain new and significant information. The crop reports thus "passed the first necessary test" (Sumner and Mueller 1989, 7) to justify their existence; that is, they did provide some beneficial information. The authors did, however, point out that this was not a sufficient test; their results did not provide insights about the value of the information relative to its costs.

11. This idea was proposed to the authors by David Zilberman.

12. Carter and Galopin (1993) examined the information content of USDA hog production reports. Their results suggested that, while the reports contained some new information, the value of that information was zero or close to zero.
identify links between the release of market information and the dispersion of price forecasts and then to apply the techniques that have been developed for valuing improved price forecasts.

Households

Contributions of social science research to household productivity have not been ignored in discussions of the benefits of social science research. Norton and Schuh (1981, 251), for example, argue that agricultural economics research that leads to improved nutrition information has the potential to “cause a shift down in the supply curves for many goods and services due to a reduction in medical costs and increased labor productivity.” However, this perspective is a little too narrow. Social science research in many disciplines (including economics, sociology, anthropology, and psychology) has led to innovations that enable household members to function more effectively both in the workplace and in the home.

In general, economists have not been successful in developing widely accepted aggregate measures of changes in nonmarket output and total factor productivity. However, much social science research is directed at improving well-being derived from activities outside the workplace and marketplace (as traditionally defined in national income accounts). For example, the ability to measure the benefits of improved relationships between family members that come from social science research is clearly important in evaluating the benefits of research in the disciplines of sociology, anthropology, and psychology. Nor is this a trivial consideration for economic research that examines and facilitates improvements in resource allocation and total factor productivity within the household regarding food preparation and consumption, other work within the household, education, leisure activities, and so forth.

In this context, particular care must be taken when evaluating the benefits of social science research that affects the health of household members. Norton and Schuh (1981), for example, argue that reduced health care costs are a benefit of research into improved nutrition. Barro (1995) has noted that reductions in tobacco consumption may lower mortality and morbidity, but that this improvement might not lower health care costs for each person. We all become ill and die eventually, and the terminal diseases we suffer from later in life (instead of dying earlier from malnutrition or excessive consumption of tobacco, 13. The need to consider the benefits from social science research that have their initial incidence within households accords well with Falcon’s view that, from the perspective of food and nutrition policy analysis, “the greatest analytical breakthrough in the past decade [the 1980s] was a reformulation of the household as an economic entity” (Falcon 1995). In an evaluation of the benefits from social science research, if the household is important and important social science findings have been made about the household’s operations, then the benefits of those findings should be estimated.
alcohol, and other "recreational" drugs) may involve more extensive and expensive treatments. Reduced tobacco consumption or better nutrition may cause each person to live longer; it may also result in more days of ill health over the course of their lives.

In fact, the relevant benefits from improved nutrition are both the net benefits to the individual of his or her improved health status and any spillover benefits to household members and the community at large. The greatest benefits are probably enjoyed by individuals whose health has improved, not by taxpayers and philanthropists because of the reduced need for income transfers to provide health care for others.

Since the inception of organized research on social science issues, an important (perhaps the most important) role for social science research has simply been to inform people about how their communities work as social and economic entities and how, as individuals, they function in those communities. The significance of social science research in this respect is noted by recognizing that this may have been the primary function of the classic works of Adam Smith, Freud, Rousseau, and Marx. It is also clear that more recent works (for example, in the public choice literature) have had a profound influence on the understanding that the general public has of the roles of institutions. Essentially, as with many other disciplines, economics has provided nonrival, nonexcludable public goods that have made many peoples' lives better.

To the extent that the "general education" role of social science research allows individuals to function more effectively within existing institutional arrangements, it can be viewed as having its initial incidence on household productivity. However, the effects on social institutions and economic productivity of social scientists as different as Karl Marx and James Buchanan have not been limited to their effects on activities within households. Spillover effects on voting and other forms of political action have caused major changes in the economic and social organization and operation of communities and countries. With hindsight, though, some of these "macroeconomic" effects may not always have been particularly desirable, at least from the perspective of aggregate economic welfare (Ruttan 1984; North 1994). It is, in fact, difficult to sort out how the benefits that flow from the educational effects of social science research can be measured with any degree of accuracy using a monetary measure. Nevertheless, there is widespread consensus that they have been and will continue to be important.14

Government Agencies

Much—in fact, the majority—of social science research is commissioned by government agencies. A substantial proportion of this research is directed

14. See, for example, Aaron (1989) and Vickrey (1993).
toward policy issues and is intended to inform policy debates, just the sort of grist for the institutional change mill emphasized by Ruttan. These types of research outputs typically have simultaneous effects on firms, households (as consumers and taxpayers), and government organizations. Some social science research, however, explicitly addresses the operational efficiency of government agencies and leads to improvements in resource use and productivity within those agencies. The analysis of allocations of resources for data collection among competing objectives fits in this category (Gardner 1983; Just 1983; Zilberman 1996). Perhaps a more important set of contributions in this area flows from the work of public choice economists (Niskanen 1971; Peltzman 1976; Becker 1983), which has led to the literature on the size of government agencies and the optimal incentive structure for efficient bureaucracies (that is, bureaucracies that are cost efficient in the sense of minimizing resource costs to achieve the given outputs or services used by their clients).  

### Joint Effects

Many social science research findings have direct implications for changes in institutional arrangements within the household, firm, or government sectors, or throughout an economy or society. Social science research programs often lead to, or are partially the cause of, both large and small policy changes and innovations in the structure of social institutions. The initial incidence of these changes is not limited to one type of organization (firms, households, or government agencies); instead, there are simultaneous direct effects on organizations within two or all three of these sectors. For example, many policy changes that affect tax yields have their initial effects on both firms and households. In fact, most policy changes begin by affecting more than one sector. For convenience, both types of adjustments are lumped together here.

Economists have usually chosen to assess the benefits of policy changes (which may be positive or negative) in terms of how they affect the economic surplus of appropriately disaggregated groups. The effects of potential policy changes on producers, consumers, and taxpayers have frequently been analyzed either in a partial equilibrium setting (Gardner 1983; Alston and Hurd 1990) or in a more general equilibrium setting, where effects on the welfare of different groups of households are evaluated (Innes and Rausser 1989; Chambers 1991; Martin and Alston 1994).

In principle, this approach works well both for marginal changes in existing policy instruments and for nonmarginal changes that involve the creation of new policy instruments and institutions. But the approach requires good information on underlying production technologies and individual (or household)  

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15. See Chapter 17 of Mueller (1989) for a discussion of this literature.
utility functions for all relevant participants in the economy. In practice, it is particularly difficult to obtain parameter estimates either for the relevant technologies or utility functions over the "relevant range" when there are non-marginal changes in policies. The problem is compounded when nonmarket goods such as improvements in environmental quality are at issue or when policy effects are to be assessed in the context of disequilibrium situations such as those that occur during major recessions. In the latter case, the possibility that household preference functions may change dramatically (because of strains created by unemployment) also complicates the empirical assessment of benefits associated with policy innovations.

Because of these difficulties, benefits are often couched in simpler terms. In the case of environmental regulations, these would include reductions in morbidity and mortality. In the case of macroeconomic policy changes, they would include reductions in the rates of unemployment or inflation. This can lead to incomplete accounts of the benefits of policy changes. For example, counting an increase in GNP associated with an increase in employment as a benefit of avoiding recessions misstates the increase in economic welfare. On the one hand, some benefits derived from the leisure time now forgone and some externality costs from environmental degradation are associated with increased output. On the other hand, the economic surplus that households derive from increased consumption made possible by the additional income may be greater than that income. There may also be positive externalities from the economic expansion (associated, for example, with reduced crime rates). These are standard problems in measuring aggregate economic welfare that must also be addressed in assessing the benefits from social science policy research.

**Measuring the Benefits of Social Science Research**

The effects of social science research on economic welfare in general and TFP in particular are largely indirect, partly in the same way that the economic effects of basic and applied research in the physical sciences are indirect. Innovations in the sciences have to be used in technologies and products that have the potential to increase economic welfare. Those new technologies and products then have to be adopted and diffused among users. For some innovations in the social sciences, particularly innovations that have their first effects in a specific sector, the process is identical. For others, especially innovations that stem from research concerning public policy issues, the process is different in

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16. It can be argued that work yields a double benefit for some people in the form of income and increased enjoyment of their time. For many academics, bureaucrats, and other professionals, this is indeed the case. But work is by no means fun for all workers either within affluent countries such as the United States or (especially) in many of the world’s poorer communities.
two crucial respects. First, the research itself often includes evaluations of existing policies or of policies that have been proposed by policymakers or other participants in the political process (including interest groups that do not directly control policy but that influence its formation). Second, it is often hard to attribute changes in economic and social policies that increase or diminish welfare to specific research findings by social scientists. For example, changes in agricultural policy that increase economic surplus may have been advocated by some economists, but adopted purely out of considerations of the number of votes to be won (perhaps because of reductions in budget deficits) and lost (because of reductions in income transfers to well-organized interest groups). Any assessment of the benefits of social science research should take account of these problems. The latter is particularly relevant for policy-oriented social science research, and any method of identifying and valuing the benefits of policy-oriented social science research should deal carefully and honestly with the issue.

Benefits from social science research can be measured at different degrees of aggregation. At a minimum, relevant assessments can be carried out for (1) individual social scientists; (2) academic departments or research teams; (3) social science research institutions; (4) social science disciplines and sub-disciplines (economics, agricultural economics, sociology, rural sociology, anthropology, and so forth); and (5) all social science research. The focus here is on the second, third, and fourth levels of aggregation in relation to economics and agricultural economics. Assessing the benefits of the work of individual social science departments (or research teams) and institutions (aggregation levels 2 and 3) is important in many countries. In Australia, for example, the Australian Bureau of Agricultural and Resource Economics (ABARE) has received a substantial proportion of the public resources made available in that country for agricultural economics research (Lawrence 1995). It often employs more agricultural economists than all other state agencies and universities in Australia. In the United Kingdom, the Ministry for Agriculture, Fisheries, and Food, through its own staff and its relationships with a relatively small number of university academic departments, was responsible for most of that country's agricultural economics research projects for over 50 years. The situation is similar in the Netherlands, where much agricultural research is concentrated at the University of Wageningen.

Honest and accurate assessments in these environments yield important insights about the total benefits derived from public investments in social science research in specific disciplines, and they provide information to guide policymakers’ decisions about the amount of resources to be allocated to the social sciences in general (as opposed to other uses, including support for research in the physical sciences) and between disciplines within the social sciences. A second reason for carrying out such evaluations is to assess the performance of competing institutions.
An overall assessment of the benefits of social science research in each discipline or subdiscipline (aggregation level 4) is most important in determining the allocation of resources. Ideally, such assessments should be made ex ante rather than ex post. As has been typical in physical science research, however, ex post evaluations are likely to be more feasible and are, therefore, the focus of the following discussion.

Conventional Approaches to Estimating Productivity Effects of Research: Relevance for the Social Sciences

If the focus is on how social science research affects productivity, then the standard issues associated with the econometric measurement of the effects of research apply. There are, however, some wrinkles that deserve special emphasis. Most of the work that links the effects of research to changes in productivity is concerned with effects in sectors that produce marketed commodities for which there are, to some extent at least, well-constructed measures for outputs (such as bushels of wheat or corn, kilowatt hours of electricity, railcars, oil tankers), inputs (such as hours of effort by class of labor, generator capacity, energy use), and corresponding sets of prices. However, much social science research—including research in agricultural and consumer economics—is directed toward improving the productivity of households and government agencies, areas of economic activity for which few consistent time-series measures of outputs or inputs have been constructed. As the social sciences are more concerned with these two sectors than are the physical sciences, ignoring the benefits that accrue directly to these sectors can increase biases in assessments of the benefits derived from those research programs.

To the extent that social science research programs affect the productivity of firms within specific sectors of the market economy, such as agriculture or the automobile industry, for example, or where data on outputs and inputs can be defined, those effects can be assessed conceptually using standard techniques.\textsuperscript{17} Alston, Norton, and Pardey (1995, Chapter 3) provide a detailed discussion of these methods and the difficulties associated with their empirical implementation in scientific research. They identify three methodological approaches: parametric, nonparametric, and index number procedures.

Parametric procedures are divided by Alston, Norton, and Pardey into primal and dual models. They subdivide primal models into production function models (in which output is the dependent variable), response models (in which

\textsuperscript{17} For example, some social science research programs are intended to improve the health of individuals. If changes in the health of the target group—the outputs—can be identified along with changes in major inputs such as food consumption, then the creative econometrician concerned with estimating the benefits of the relevant social science research program may be in business.
output is expressed per unit of a single input, such as land), and productivity models (in which output is expressed per unit of aggregate inputs). Dual parametric models use either a profit function or a cost function, and parameters are estimated from the associated systems of factor demand and (in the case of profit functions) output supply relationships. Nonparametric approaches (see, for example, Chavas and Cox 1992) evaluate data on outputs, prices, and inputs for their consistency with axioms of rational producer behavior such as the "weak axiom of profit maximization." Finally, Alston, Norton, and Pardey (1995, 95) note that index number approaches are "used either directly or in conjunction with econometric approaches to assess sources of growth in . . . output or . . . productivity" to identify the share of output growth or TFP growth attributable to research.

As Alston, Norton, and Pardey and others emphasize, while some approaches are almost surely preferable to others on theoretical grounds, none of the approaches is worth much if the data on which they are based are inadequate. The problems associated with measuring traditional inputs such as labor, land, energy, and capital are well documented. Particular difficulties, however, are associated with data for science research and technology transfer. These difficulties are also relevant to data on social science research. They include problems associated with lengthy time lags between investment in research, innovations, and the adoption and diffusion of technology. The decay of the usefulness of knowledge generated by previous research also presents estimation problems. In the same way as new technologies depreciate or become obsolete, insights from social science research become less useful, losing value both to organizations (firms, households, and governments) and in the policy arena.

These problems create serious difficulties in evaluating the benefits of scientific research (as noted by Pardey and Craig [1989] and Chavas and Cox [1992]). They are at least as problematic for an evaluation of social science research.

Measuring the quantity of inputs into social science research may also be difficult. Alston, Norton, and Pardey identify three categories of expenditures for research in agricultural science: core funding, other government funds, and donor funds and grants. As they note, there are significant difficulties in determining which research funds are directed toward projects that enhance productivity for specific commodities when a large proportion of all research funds are allocated

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18. In agriculture, technology transfer is often closely associated with and measured by investments in agricultural extension.

19. At one stage, it was almost axiomatic among agricultural economists that price stability increased welfare and, therefore, that government-supported price stabilization schemes were good. This view no longer dominates the literature, even when the policymaker's welfare function only includes the utility of producers. See, for example, the discussion of the Canadian Wheat Board in Carter and Loyns (1996) and the analysis of the Australian Wool Price Stabilization scheme in Bard-sley (1994).
to multicommodity projects. These difficulties are compounded in the social sciences, where multidisciplinary projects are common and much research support is embedded in budgets that are ostensibly for other purposes, such as teaching and administration. This is a common phenomenon in all disciplines, but it probably presents more of a problem in the social sciences because explicit research funding represents a smaller fraction of the total compensation and support received by social scientists actively involved in research.

Concerns about research with effects that cross the boundaries of regions and countries are also relevant to social science research. Although some social science researchers (Dube 1982; Atal 1983), especially in developing countries, have argued that the effects of these "spillovers" from social science research in other countries (typically research done by researchers from Europe, North America, and Australia) have been negative because of cultural limitations, it seems clear that they are large.

In addition, it is difficult to see how the welfare benefits that flow from policy-oriented social science research can be easily captured by conventional econometric approaches to estimating the returns from research. Many of these benefits, which are often asserted to be large, are not captured in established time-series data sets, or they may occur in "lumps" and are difficult to identify. One possible approach, however, may be to estimate econometric models for quantifying the benefits of economic policy changes, especially at the broadest level of effects through changes in institutional structures (such as property rights regimes), using cross-country data series. Illustrative studies include Przeworski and Limongi (1993) and Barro and Sala-i-Martin (1995). These studies would involve large and well-documented differences in such structures, and it is conceivable that impacts on variables such as real GDP growth rates could be identified.

**The Case Study Alternative**

If estimating social science research benefits using the "usual" techniques is difficult at best, what should be done? One approach that has been suggested is to use case studies. It has even been implemented in at least one research institution (ABARE). The idea is simple. The benefits of the research output of a discipline,

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20. Oehmke (1995) provides an interesting empirical discussion of this issue in the measurement of agricultural social science research in Michigan.

21. To recognize that spillover effects can be large in the social sciences, all that is needed is to recognize the effect of the public choice literature on the property rights policies adopted by some of the emerging democracies in central and eastern Europe. In the context of agricultural productivity, the work of Heady and others on optimal fertilizer use has been important in many countries while, in the context of agricultural policy, the work of Gardner (1983) and others on the decoupling of income transfers has been important.
subdiscipline, or research institution should be assessed by selecting and estimating the benefits from a sample of research projects taken from the population of projects carried out by the entity of interest (or, if the relevant institution or discipline research program is small, the population of all research projects).

The approach is both challenging and full of pitfalls. First, given that a research project has been selected as a case study, there is the issue of what should count as a positive (or negative) benefit. The answer here—as Alston, Norton, and Pardey (1995) suggest for agricultural research projects—is any change in economic surplus, as conventionally defined by economists. Effects should also be estimated for each relevant aggregation of individual households, because policymakers in particular and societies as a whole are unlikely to place the same weights on welfare changes for each group. For example, information may be needed about effects on the economic welfare of low-income and high-income households, rural and urban households, farm households and nonfarm households, and food consumers, taxpayers, and food producers. Behind this response to the question of what counts as a benefit lies the presumption that the benefits of social science research should be assessed using the analytical frameworks developed by economists to measure changes in economic welfare and, to the greatest extent possible, using a monetary measure.

A second important issue is the definition of the population of research projects from which case studies are to be drawn. If the evaluation is to be made of the benefits derived from an institution’s research program over a specified, reasonably long, time period (say, the last 20 years), then it may be empirically feasible to simply use the agency’s annual reports and other records to identify all projects that have been carried out during the period. While the problem of identifying the population of interest is no more difficult conceptually if the evaluation is to be made of the benefits from economic research in, say, the United States, it may not be feasible to obtain a list of the population of all relevant projects. But, as discussed below, all may not be lost if the goal is to obtain objective measures of research benefits from a discipline.

A third issue is the selection of the sample of projects for the case studies. In a recent self-assessment, ABARE reported “case study” estimates of the benefits from a small number of high-profile projects. It is not clear how those projects were selected. However, an objective approach would be to use a random sample of the population of relevant projects. For example, if an institution identified 250 projects in its research portfolio for the relevant period and case studies could be carried out for only 25 projects, then a random sampling approach would be used. This approach would ensure that projects that yielded modest or even negative benefits would have a chance of being included in the

22. The time period should not be too short for two reasons. First, as noted above, the effects of social science occur with long and variable lags. Second, estimates of the benefits can be biased if the intertemporal focus is too myopic.
assessment and that benefits estimates could be unbiased in a statistical sense. If the population of research projects included a few large projects and many projects that were small in terms of research resource costs, then a stratified random-sampling approach could be adopted to ensure that the sample adequately represented the institution’s research portfolio.

If the objective were to obtain estimates of the benefits from research by an entire discipline (such as economics, where it may not be possible to list the relevant population of research projects), an alternative approach would be to identify the institutions involved in research (universities, government agencies such as the Bank of England, and private for-profit and nonprofit groups, such as the Brookings Institution). These could be stratified, perhaps by the volume of external grants and contracts. Then a random stratified sample of institutions could be drawn and all (or samples of all) research projects carried out by those institutions during the relevant period could be evaluated.

In both of these cases, the objective is to ensure that a statistically representative group of case studies is identified for the assessment of benefits. The alternative approach, convenience sampling, is subject to the criticism that the benefits assessment process is simply a self-serving exercise. It is also a two-edged sword in that critics and supporters of social science research could both use similar case study approaches, but “conveniently” select different cases to make their points. In addition, a focus on a small number of conveniently selected case studies fails to provide meaningful information about expected payoffs from such research or the shape of the marginal benefits function. Information about the latter could perhaps be obtained for a discipline by assessing the benefits for work performed at institutions with different research rankings.

A fourth concern, especially in relation to policy-oriented social science research, is the crucial issue of the extent to which benefits derived from policy changes can be attributed to social science research programs. Evaluating these effects is clearly an art rather than a science, likely to involve a great deal of historical investigation, and to be the subject of furious disputes. For example, is it really fair to accuse Samuelson and Solow (1960) of causing policymakers to adopt overly expansionary polices that created unnecessarily high rates of inflation in the 1960s because of their article on the Phillips curve? Similarly, should the work of agricultural economists be credited for changes in U.S. farm programs in 1990 and 1996 that may have lead to increased economic efficiency in that sector? It is clear that these types of questions have to be addressed carefully and honestly, and full or partial credit or blame assessed, if the benefits of social science research are to be estimated using case studies.23

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23. Within an economy, policy innovations tend to be adopted on a “lump sum,” or zero-one, basis, rather than on an incremental basis analogous to the “S” curve adoption patterns frequently associated with science and technology innovations. This creates special difficulties in linking economics and other social science research to policy changes using statistical techniques.
Conclusion

This chapter has addressed two major questions: What are the benefits of social science research, and how can those benefits be estimated? It has been argued that, in principle, social science research is no different than research in the physical sciences because it provides new knowledge that can alter the economic welfare of households. It is likely to be useful in identifying those effects by explicitly recognizing that the initial incidence of social science research often occurs through changes in productivity within households and government agencies, as well as within firms. Because so much social science research is oriented toward relationships and activities within households, a failure to account for benefits that originate there—within the household—is a particularly serious source of potential bias in such research.

Conventional econometric techniques are less effective in estimating the effects of social science research on productivity due to difficulties associated with a lack of relevant time-series data and those that arise from the policy focus of much of social science research. One possible alternative is to rely on case studies. However, serendipity or convenience is an inappropriate basis for selecting the case studies. Instead, the population of relevant research projects should be identified and random (or stratified random) samples of research projects should be selected to ensure that the estimates of research benefits derived from analyses of those case studies can be viewed as statistically representative. Otherwise, any estimate of benefits will be viewed with suspicion by policymakers and research administrators (most likely correctly) as a biased and, probably, self-serving waste of resources.

References


6 Adding Value through Policy-Oriented Research: Reflections of a Scholar-Practitioner

C. PETER TIMMER

A methodology for evaluating the benefits of policy-oriented social science research could have radical consequences for the size and composition of the financial support given for such research. In the field of medical research, experience suggests that a neutral and widely accepted methodology for evaluation—double-blind clinical trials, for example—may quickly weed out less promising approaches. Financial resources for research can then be devoted to more promising lines of inquiry, thereby improving resource allocation. More generally, experimental sciences offer rational and reproducible methodologies for testing hypotheses—the “scientific method.”

Although the scientific method underlies the logical approach of most social science research, policy analysis based on such research usually cannot rely on data from carefully designed experiments to test hypotheses. Instead, policy analysts must, at least implicitly, design models of the societies they are investigating, estimate the parameters for the models from whatever historical data are available, and then run the models with counterfactual assumptions about the world in order to assess how policy changes alter outcomes. Whether models are formally written down and estimated statistically, or are based on the intuition and knowledge of an experienced policymaker, tests of alternative policies are often less than scientific. This inability to design ideal scientific experiments is perhaps the biggest difference between practitioners of economics

1. There are, of course, significant exceptions to this generalization. Experimentation with different health insurance schemes by RAND, different land-tenure arrangements in separate provinces in China, and different educational reform packages in randomly selected schools in Kenya illustrate the potential for creating controlled experiments even in the social sciences. There is also a growing literature on “natural experiments” that exploit exogenous changes in otherwise similar environments to test cause-and-effect hypotheses.

2. The crucial distinction for policy analysis is “with and without” the policy, not “before and after” the policy. The distinction is especially important in analyzing the impact of structural adjustment policies, where the alternative without reform was often an even steeper drop in welfare than what was seen after the policy reform. See Sahn, Dorosh, and Younger (1996) for the dimensions of the problem in Africa.
Apart from the lack of credible experimental evidence, three fundamental problems make it difficult to evaluate the benefits from policy-oriented research. First, establishing a causal link between research and the ultimate outcome of a policy is almost always controversial, not least because evaluating policy outcomes is often controversial. Not all exogenous variables can be held constant even in the most complex economic models, and causal links between policy changes and particular outcomes can never be established with great confidence. Social outcomes are always subject to the possibility that they were caused by something else or would have happened anyway, a point emphasized by Krugman (Chapter 3, this volume).

Second, policies are acts of government. In principle, such acts should be designed to solve problems that private agents cannot solve independently in a manner that is optimal for society. Economic theory suggests that problems will often occur in establishing the value of a policy that closes the gap between an outcome's private and social profitability. Economists have not ignored this problem, as methodologies that use contingent valuation or maximization of combined producers' and consumers' surplus attest, but no one would argue that the problem has been fully resolved. Establishing the value of social science research in the design and implementation of policy changes is even more problematic, because it is only one of several inputs to the policy process.

Third, even if the benefits could be quantified and clearly linked to both the policy and the research that went into the policy design, an evaluation of whether society gained or lost would still be plagued by the difficulties in making interpersonal comparisons when the policy is not Pareto-improving. Even for potentially Pareto-improving policies, when feasible redistributions are not actually made, a gain in social welfare cannot be claimed without an explicit social welfare function. Social choice theory focuses on the mechanisms available to a society that permit such a welfare function to be defined. A major problem is that under democratic institutions and voting schemes, an explicit social welfare function cannot be designed (Arrow 1951).

This chapter is not concerned with the nonexistence problem that bedevils welfare theory. Instead, the concern is much more specific: identifying the links in the policy process between policy research and social outcomes by bridging the gaps between basic social science research and policy analysis, advice, design, implementation, outcomes, and evaluation. Not all policy processes

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3. Considerable frustration has been expressed over this line of thought. Democratic institutions do indeed make choices that affect income distribution and leave some citizens worse off, even with compensation. The point, however, is that social choice theory offers little insight into how these choices are made.
establish and maintain these links, but in the specific cases where they have, opportunities exist to identify and quantify the cause-and-effect relationships that connect research to the outcomes of policy in ways that are simply impossible to make as generalizations. With enough such examples, it may be possible to learn about the relationship between research and outcome and the key variables that affect the social profitability of the underlying research. Even a handful of examples would sharpen the discussion considerably and point the way toward the most productive forms of social science research designed to influence policy.

These themes are pursued at two levels. The distinction between social profitability and private profitability establishes the theoretical difficulties of identifying benefits from policy analysis. These difficulties set up the first theme, a sequence of lessons on how to make policy analysis more effective in practice, and thus to raise its net social profitability.

This chapter tracks the interactions among research, the analysis of policy and policy advice given to policymakers, and the design and implementation of policy within the field of agricultural price policy. This section builds on earlier analyses of rice market interventions in Asia and demonstrates the complexity of establishing credible causal links from one stage of the policy process to the next (Timmer 1988). When bureaucratic actors, political forces, and economic shocks also affect policy outcomes, it is difficult to see a clear trail from research to outcome.

Second, this problem is addressed by highlighting the issues of Indonesian rice price policy prior to 1997. Indonesia has poured substantial resources into implementing its rice policy, but has also made efforts to understand analytically how to design and evaluate that policy (Timmer 1986a, 1991). As a "scholar-practitioner" who has participated in virtually all stages of this policy process since it was first designed by Mears and Afiff (1969), I will argue that a plausible train of causation can be established by showing the relationship among policy analysis, subsequent changes in the policy, and effects of the policy changes on the economy.

Making Policy Analysis Useful: The Policy Cycle and the Importance of Feedback

If governments let world markets determine domestic prices, price interventions would be redundant. It would also be possible to intervene heavily in markets without an analysis of the likely outcome. But such an idiosyncratic and unsystematic approach to agricultural pricing has proven ineffective in helping societies achieve food policy objectives. The alternative approach is price policy analysis, a somewhat formal effort to understand how existing and proposed price policies affect these objectives. The principles and basic methodological frameworks for this analysis are presented elsewhere (Timmer, Falcon, and
Pearson 1983; Timmer 1986b). The goal here is to examine the experience of Asian countries in applying these principles and frameworks and to identify some common issues that must always be addressed. In this sense, the story runs parallel to Arnold Harberger's Richard T. Ely address on the role of policy practitioners (edited and included as Chapter 4 in this volume).

Four issues seem pervasive. Only the first—How does an analyst know which policies are best?—is analytical, and even this issue is not readily resolved. A broad set of objectives and constraints must typically be incorporated into the analysis, as well as a clear recognition of the actual starting point for the food system.

The second issue concerns how the results of policy analysis can be communicated effectively to policymakers. This communication effort puts the analyst into a negotiating role in which pedagogy may determine the outcomes. Although this role requires a subtle change in the analyst's task from that of understanding to advising, it does not necessarily require specific policy recommendations. Instead, the analyst must remain an advocate for the analysis itself and for an understanding by policymakers of the trade-offs identified in the analysis.

The third concern is whether a new policy can be implemented. A frequent criticism of policy analysts, especially of economists, is that they are excellent at designing technically efficient policies, but not ones that governments can implement. In fact, analysis that ignores implementation issues is simply bad analysis. The problems might be economic, political, social, or cultural, but they must be incorporated into the analysis if policy implementation is to be successful.

The fourth issue is whether the new policy actually works. After the analysis, communication, and implementation are done, the policy must be evaluated. Much can be learned from the evaluation process because unexpected problems always arise. Being able to distinguish elements in these problems that are systematic from those that are purely idiosyncratic provides valuable lessons for the next cycle of policy analysis.

Analysis, or How to Know Which Policies Are Best

Determining which policies are best can only be done with respect to a set of objectives. If the objectives conflict with each other, or if a policy promises improvement in one goal at the expense of others, some mechanism for weighting outcomes is needed. The analyst's task is not usually one of providing these weights; his task is to explain carefully, and as quantitatively as possible, the effects of alternative policies on social objectives. The objectives themselves are often uncontroversial. One widely cited source lists the following: efficient growth in agriculture; improvement in income distribution through productive employment; a nutritional floor for the poor; and national food security (Timmer, Falcon, and Pearson 1983).
The objectives of individual policymakers, indeed, of entire governments, are sometimes sharply at variance with these social-welfare-oriented goals. If so, analysts must understand how this divergence affects the prospects for successful communication and implementation. But analysts must avoid becoming "yes men" in the halls of political economy," to quote Schultz (1978, 9). This is a delicate task. The initiative for firing the analyst, or not asking him or her to do the analysis in the first place, lies with the government. Still, economists provide a unique understanding of the importance of allocative efficiency and the factors that influence its achievement.

Whether an analyst can successfully push on behalf of the objectives of broad social goals depends partly on the environment and partly on the skills and reputation of the analyst. Coping with all four issues discussed here, not just the narrower analytical issues, helps policy analysts walk the fine line between not rocking the boat and "Speaking Truth to Power" (Wildavsky 1979). In developing countries, especially those under the more authoritarian governments found in much of Asia, policy analysis has often been a substitute voice for the interests of those disenfranchised or too poorly organized to affect policy deliberations on their own behalf. Large corporations, unions, students, the military, and even the government's own bureaucracy can easily and effectively make their interests known in policy debates. Small farmers, poor consumers, and many workers and entrepreneurs in the informal sector are often voiceless in the policy process unless analysts specifically incorporate their welfare into the analysis and the policy debate.

What kind of methodological framework can illuminate this broad range of issues? Having clear microeconomic foundations for understanding the effects of a change in policy is particularly important when small farmers and enterprises dominate economic activity. When adjusted to address the issues raised here, the basic neoclassical economic model of households, firms, and markets provides a broad base on which to build the analysis. Many of the assumptions underlying the pure neoclassical model do not hold, however, and relaxing them often has severe implications for normative conclusions that are derived from the principles behind the model (Stiglitz 1987). But as an empirical framework, with parameters carefully estimated from a country's own experience, household-firm-market models provide powerful insights into the workings of most countries' microeconomies. It is still difficult to achieve multimarket consistency for the results of such microeconomic analysis, though this problem has been at the frontier of economics research for more than a decade.4 These difficulties

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4. As a starting point, see the review by Robinson (1988) of computable general equilibrium (CGE) models, the comparison of results of using six different CGE models to evaluate agricultural price policy in de Janvry and Sadoulet (1987), and various papers by Braverman, Hammer, and their colleagues on multimarket models for analysis of agricultural price policies (for example, see Braverman, Hammer, and Gron [1987]).
should not discourage the analyst. The goal is to improve the situation—to move policy in the right direction. Sensible and empirical applications of a microeconomic framework, with some attention to market spillovers and macroeconomic effects, have proved to be remarkably robust approaches toward achieving this goal.

**Negotiations, or How to Communicate the Results**

The analyst’s ability to transmit analytical results to policymakers depends on their relationship. Several roles can be identified. Analysts might work within an agency under the daily supervision of a policymaker, or for international agencies, such as the International Monetary Fund (IMF) or the World Bank, that make virtually no contact with the country’s policymakers until final briefings. The role to be played determines the difficulty of communication. Pedagogy is often ignored in training policy analysts, but one of the reasons why effective classroom teachers often make good policy advisors is precisely because they think critically about how to communicate effectively with an audience.

Analysts who work on terms of reference provided by their supervisors know that there is a demand for the analysis. But there is no guarantee that the results will be communicated effectively to higher levels of the agency or will ultimately affect the policy debate. Even in markets, demand does not always call forth effective supply. In bureaucratic settings, the potential for a mismatch is greater. One possible approach is for the policymaker to specify the problem clearly so that the analyst has a straightforward task of cranking out results. But the policymaker often does not know the exact nature of the problem. In this situation, the analyst can pursue a variety of approaches and topics, but the burden of communicating the results to a potentially skeptical superior is greater. When communication channels are open, an iterative process can be established to generate a larger degree of trust.

The problems are more serious when the analysis is not conducted by the affected agency. Ministries of finance, central banks, and central planning agencies are often responsible for reviewing policy proposals from line agencies, and then for coordinating policy analysis and debate within the cabinet. The establishment of close collegial links between, for example, analysts in the ministry of agriculture and analysts in the planning agency is one of the most difficult tasks in developing countries. Failing to establish those links is one reason why “institution building” has such a weak track record. This lack of success is at least as much a failure in the design of programs to build institutions

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5. This is similar to the situation that Harberger (1993) describes, where it would be better to write the terms of reference for the consultant after the research is completed.
as it is an inherent part of the task. Most such programs are inward looking; they focus on the analytical skills of personnel within the agency rather than on their communication skills and collegial interaction. But this is a two-way street that can work only if the coordinating agencies reciprocate.

Analysts are sometimes outside consultants to a ministry or policymaker. Although there is some difference between foreign and national consultants in this role, the crucial distinction is that the analysts come from outside the government, usually from academic or research institutions, rather than from staff positions. Such outsiders are often perceived as less likely to defend the agency’s narrow interests and to be better able to provide critical appraisals of policy initiatives or to evaluate existing policies. This presumption breaks down if those consultants develop continuing ties to an agency and come to expect future assignments for income or access to research data. Then they become more like regular staff within the agency, with all the difficulties and advantages that might entail. Bringing bad news to the boss is hard in these circumstances.

A more productive role for the outsider is often in cross-ministerial communication. In this case, it can be a distinct advantage to be a foreigner who is exempt from those cultural restrictions that inhibit direct face-to-face discussions between low-level analysts and high-ranking officials. This role is most productive when the advisor is perceived as an impartial broker. Such a reputation can be established only over long periods of time and with an accumulated track record that instills trust and confidence. No individual is likely to be able to play this role in more than one or two countries. On the other hand, it becomes difficult for such individuals to work themselves out of a job because, as their experience increases, their services become more valuable.

Policy analysts also work in donor agencies such as the IMF, the World Bank, or the U.S. Agency for International Development (USAID). To improve the effectiveness of the aid process, such multilateral and bilateral agencies increasingly conduct independent policy assessments. These assessments can simply be offered to policymakers as inputs to their own process of policy analysis and design. In these cases, little controversy generally arises.

However, assessments of policy by donors increasingly form the basis for a policy dialogue with governments, the object of which is to induce policy changes that the donors think advisable. If the analysis has been conducted in a way that illuminates the problems facing a country, these dialogues can be extremely productive. This is not always the case. Sharp disagreements often arise over the directions of appropriate changes in policy. In many cases, the donor analysts have the economics right but fail to understand other ingredients in effective policy analysis. Hence, they fail to communicate the desirability of a change in policy to government officials, who are concerned about other dimensions of the effects of a policy.

The importance of basic models to policy advice is linked to the short time horizons in which donor analysts must work. Three-week trips to unfamiliar
environments mean that analysts must rely on readily accessible data, basic models with wide applicability, and a willingness to let fairly restrictive assumptions determine policy results. This approach to policy analysis relies on an underlying set of ideas about appropriate policy interventions rather than on an understanding of the complexity of any given country's policy environment. A particular problem with development economics has been its vulnerability to wide swings in the prevailing political ideology and the resulting enthusiasm for particular approaches to the development process.

An advantage of watching this process in one country over a long period is the realization that intellectual fads come and go, but the basic structural problems that need to be addressed remain. From this perspective, for example, Indonesia is a "transitional" economy. By the mid-1990s, the country was probably halfway toward becoming a fully open, market-oriented economy (but much less than halfway in developing a modern financial system). Such a transition takes place over decades, not months or years. Thus, the only way to improve the effectiveness of the policy dialogue between donor and country is for both sides to recognize the long-run nature of the development process and the need for policymakers to live with the complex outcomes of policy changes in the short run.

Implementation, or How Policies Can Be Carried Out

Many observers feel that policy implementation is the most difficult aspect of government intervention in development. Frequent implementation failures create wide gaps between objectives and outcomes, between rhetoric and results. They lead to widespread disenchantment about the potential for governments to improve on simple market-determined outcomes. Part of the reason is that the world is unpredictable and government policies respond more slowly to changed environments than do markets. But much of the problem stems from efforts to implement unrealistic policies. For a policy to be adopted, effective policy analysis must be communicated to policymakers clearly and convincingly. The analysis must also incorporate the problems that will be faced after the policy has been approved for implementation. Although incorporating constraints on implementation vastly complicates the task of the analyst, accounting for these constraints simplifies communications with policymakers because it becomes clear that the analyst understands the problems the policymaker faces in policy management.

Many constraints impinge on the potential success of a policy. A major reason why successful policy analysis requires extended time in and knowledge of the country concerned is that constraints on policy in any country are unique and idiosyncratic. The following discussion suggests where the analyst should look, but does not tell the analyst what will be found.
The Budget

Economists are often of two minds about the role of budgetary expenditures in the implementation of a policy. In one view, the budget is simply a transfer mechanism that reallocates the control of resources. Neutral fiscal transfers do not affect the efficiency of resource allocation. If a policy is good for society, it is acceptable to take money from society to implement it. As long as the analyst does not actually have to account for the taxes that raise the resources, this general principle makes it easy to take for granted that whatever resources are needed for the policy will be available. In this view, designing the policy and finding the budgetary resources to implement it are separate tasks for separate agencies.

The alternative view that has emerged in modern public finance holds that raising budgetary revenues to fund government expenditures will distort the economy. Private citizens and firms can almost always spend and invest their money more effectively than governments, and they will not incur the costs of tax collection. In this view, all government projects and policies must justify their costs, not just in terms of some general social benefit, but also with respect to the marginal costs to society of raising the public revenues needed to implement them. Because there are substantial philosophical differences over the value of resources in private hands compared with those in public control, the debate over the means used to incorporate budgetary costs in policy analysis is often contentious.

From a practical standpoint, whoever controls the budget ultimately controls the policy debate. Given the tight budgetary environment in most Asian countries, this means that if an agricultural price policy requires large budgetary expenditures, the minister of finance is likely to be a major actor in its implementation. This has two implications. First, the impact on the budget must be a key aspect of any price policy analysis. Second, techniques to circumvent direct budget requests and still carry out policy will always appeal to implementing agencies. This explains much of the popularity of agricultural pricing policies that place most of the financial burden on producers or consumers rather than on the treasury (Anderson and Hayami 1986).

Bureaucratic Capacity

Some bureaucracies are more effective than others. This holds across agencies and governments, and from country to country. The Korean bureaucracy can implement policies that would be impossible in Indonesia or Bangladesh; for example, the Korean government has adopted a dual price policy that would be impossible in the Philippines. The Sri Lankan government distributed universal rice rations successfully because no means test was needed, but it lacked the skills needed for a fair and efficient means-test-based food-stamp program.
Malaysia cannot control its border with Thailand, and it is often said that, because of its extensive coast line, "God meant Indonesia for free trade." But South Korea and Japan control their rice trade completely.

The bureaucratic capacity to implement policies is an obvious constraint on the development of sensible policies. Designing a policy that requires extensive bureaucratic resources without the capacity to carry it out in the short run results in poor implementation and widespread criticism. Because trust and confidence in government policy is an important element in generating positive expectations on the part of investors and the general population, such failures have ramifications well beyond an inability to support a specific price or exclude a particular village from food-stamp benefits. By including bureaucratic capacity in policy design, analysts help the government avoid such problems (Rodrik 1989).

The nature of the bureaucratic constraint depends on the policy. Sometimes the skills of agency staff are adequate, but management personnel and processes prevent them from being used effectively. Sometimes staff members are poorly trained for the tasks at hand, and even dynamic and effective leadership can have only limited success in implementing new policies. Sometimes both staff and management are adequate to the task, but the agency's structure and incentive system stifles initiative and effective policy management. Bureaucratic reform is rarely high on the list of activities for policy analysts, but identifying constraints in the bureaucracy and participating in the long-run process of institution building might be the most productive use of their time.

**Physical Infrastructure**

Implementing a floor price policy for rice means that the food logistics agency must have access, through leasing or ownership, to trucks, grading equipment, mills and dryers, and warehouses. Roads must be passable if grain is to be moved out of surplus areas; ports must be able to handle imports or exports when required. Physical infrastructure, like bureaucratic capacity, is not fixed in the long run. Analysis that identifies important bottlenecks can point to productive investments in either area. But in the short run, policies cannot be implemented that require an infrastructure that is still at the blueprint stage.

Constraints on infrastructure tend to be an issue of costs and degree, not fixed and rigid, even in the short run. Where the grain market already functions before a floor-price intervention, for example, traders have some capacity in place to buy, mill, store, and transport grain. Much of this capacity will disappear overnight if the government attempts to commandeer it for its own use, but rental arrangements or payment for services when price margins encourage participation by the private sector in marketing may mean that the new policy will succeed even in the face of significant short-run bottlenecks in the physical infrastructure. This success does not happen automatically, of course. The analyst must design the policy in the first place so that it fits the situation.
Supply and Demand Parameters

How farmers and consumers respond to price changes is an essential component to the implementation of any price policy. If, as many policymakers seem to believe, supply and demand elasticities are near zero in both the short run and the long run, price policy will have only a limited effect on the efficiency of resource allocation. The objectives of food pricing for income distribution could be implemented with few concerns for economic distortions and slower growth. Policy analysis would thus be a matter of focusing on who gains and who loses rather than on how the economy is affected.

Unfortunately, the world is not so simple. The empirical record throughout Asia shows that both farmers and consumers respond to price signals. Supply and demand elasticities for rice are high in the long run; values of 0.5 to 1.0 are often found for both supply and demand when adjustments between 5 and 10 years are permitted. They are also significant even in short-run changes measured year to year. But for policymakers, the short run is often measured in days or weeks, so that small changes that are a year away seem minuscule and irrelevant.

The imperfect match between policymakers’ perceptions of the time horizon they face and the responsiveness of producers and consumers can devastate the implementation of a well-designed food price policy. Using prices to call forth desired changes in the balance between supply and demand requires faith in the capacity of markets to transmit signals and elicit appropriate responses from millions of farm and consumer households. Behavior does not change overnight, nor does it change much from one year to the next. This inertia makes policymakers nervous, especially when buffer stocks are low or prices of imports have risen sharply. If supplies to a government-controlled market are suddenly limited, a significant crop shortfall is reported, or rapid income growth puts pressures on demand, low supply and demand elasticities mean that market prices will rise sharply in the short run to restore balance. It is precisely this instability that governments seek to avoid, and the appeal of attempting to place direct quantitative control over prices is understandable.

The constraints imposed on price policy by inflexible supply and demand responses can be identified in the short run and alleviated in the long run. New technology and the widespread use of purchased inputs tend to make output more sensitive to producer prices. The availability of other staple foods and diversification that comes with higher incomes contribute to more flexible responses by consumers. Marketing efficiency is critical if price signals are to be transmitted quickly and accurately; governments have a range of investments and policy options that lower marketing costs, improve access to small traders, and make markets more reliable and effective vehicles for the implementation of government policy (Timmer 1997). Price policy analysts have the task of determining the actual sizes of the adjustments that producers and consumers
make to changes in prices. They must also understand the types of initiatives that could speed these adjustments and make production and consumption more flexible. This understanding comes partly from econometric analyses of price and quantity data and partly from visits to villages to observe how farmers actually cope with the day-to-day variability that causes production and market risk. Unless the analyst understands the small farmer's decision-making environment, he or she cannot serve as a "substitute voice" in the policy debate.

**Politics**

Politics is frequently invoked as the reason why good economic policies cannot be adopted. Sometimes this means that a broad, popular opposition to a policy can jeopardize even elected governments. Sometimes politics means that narrow vested interests will be harmed by the policies proposed and they can be expected to use their influence on policymakers to prevent the change. For example, unions or the military often oppose increases in food prices or devaluations of a country's currency. Sometimes politics just means that the minister does not think a change in policy is a good idea. In addition, when officials are criticized for a lack of political will to implement needed policy reforms, it is forgotten that virtually all changes in agricultural price policy hurt someone's interests, vested or not.

In some political environments, policy analysis based on economics is irrelevant because a political constraint is binding. Such systems tend to be dominated by an individual personality or a powerful ideology, rather than by an orientation to economic growth or a complex amalgam of rent-seeking interest groups. But societies do change from one type of political system to another, often in astonishingly short periods of time, and policy analysts who are irrelevant under one regime may be essential under the next. A society that can train and preserve its analytical talent even when that talent has no influence on policy can avoid the long and painful process of rebuilding this capacity. China's anti-analytical ideology in the early 1970s nearly destroyed its indigenous capacity to evaluate policy trade-offs in a more market-oriented rural economy. By comparison, the growth-oriented politics of Indonesia introduced after the termination of the personal and ideological politics of Sukarno placed a cadre of economists in the cabinet and made the country more conducive to effective policy analysis. However, the influence of these economists waned in the early 1990s as President Suharto's children became increasingly influential players in both economics and politics, reducing the potential for policy analysis to identify and deal with policy problems (Grindle and Thomas 1991).

For good reasons or bad, the political constraint is always important. The task of the policy analyst, however, is not to incorporate the constraints into the analysis, thus hoping to design policies that are acceptable. Rather, analysts need to determine which dimension of a policy is objectionable, to whom, and
to what degree. The analyst must ask whether it is possible to design compensating programs or an information campaign to clarify exactly who gains and loses under the new policy. This approach can be risky, especially when the vested interests in question are close to power, or are simply powerful. Sometimes policy analysis can be a feeble instrument for inducing change; at other times, courage and simple facts bring surprising results.

An entirely new field of positive political economy has emerged since the 1970s to help analysts understand why politicians make rational policy choices that do not increase social welfare. Although many of the "rational choice" models seem contrived or highly dependent on election schemes that bear scant resemblance to reality, the basic paradigm offers a powerful insight: Few politicians will choose a policy that is likely to force them from power. Economic analysis that is consistent with this insight is more likely to be adopted, and to have measurable benefits, than analysis that fails to clear this basic hurdle. As Harberger (1993) stressed, such an approach does not mean that the analyst has abandoned the search for efficiency and improvement in overall social welfare. But part of a loaf is often better than none at all.

Evaluation, or How to Know Whether Policies Are Working

Evaluation is the poor relation of the policy analysis family. Once an analytical design has been made, policy negotiations have been conducted, and a policy has been implemented, few individuals or institutions have much energy or budget left for evaluation. If the policy works, it will be obvious; if it fails, it is better not to stir up a hornet's nest. In fact, opportunities should not be missed to understand why a policy went awry and to channel this information back into policy analysis and design. Policy evaluation not only completes the linear process of design, communication, implementation, and evaluation, but it also provides an important input into policy design itself. Several steps are needed to exploit this potential.

The first step is for analysts to recognize at the stage of analysis and design that evaluation is an integral part of the policy process. Monitoring needs to be built into the process from the beginning, including baseline surveys where possible. If a full research effort has gone into the analysis, much of the baseline data may have been gathered as an input. The task is to make it possible to continue to gather data within a consistent sample frame and to allocate analytical resources to monitor and evaluate these and other data from conventional sources, such as market reports, surveys by statistical offices, and even newspaper coverage.

The second step is for the original policy analysts to be involved in implementing the policy and monitoring its outcome. This may heighten their sense of responsibility because they must live with the problems created by their
own design. It also creates the continuity of insight for individual analysts that is important for building an intuitive sense of the likely response of the economy to shocks and interventions. To reinforce this "intuition building," analysts can participate in the troubleshooting that is an essential part of making a new policy work. When this role in ongoing implementation and short-run evaluation is built into the analyst's original terms of reference, the policy analysis and design often becomes more pragmatic and successful. Few countries have an adequate supply of analyst-practitioners who can conduct this amalgam of thinking and doing. Few universities have positions for scholar-practitioners who can develop the methodological tools for policy analysis. Ideally, such tools are outgrowths of teaching, field research, and experience with the design and implementation of policy.

Gaps in current approaches to improving policy analysis are painfully obvious. Academic scholars and methodologists are drawn to narrower topics, while practitioners become more disenchanted with what they perceive as the irrelevance of the new techniques. To close these gaps, academics need to serve as practitioners, at least often enough to understand the complex reality in which policy analysis and design actually take place.

The goal of such cross-fertilization is not simply to improve analytical methodologies for messy policy problems, although that is reason enough. The intended outcome of keeping policy analysts involved in the complete circle of the policy process is to improve policies. Lessons that are learned about design and implementation problems should feed back to analytical methodologies but should also be incorporated regularly in policy adjustments. The need to monitor and adjust policies, rather than merely evaluate them for the historical record, has important implications for policy design. Policy then becomes a process rather than a result, flexibility and the capacity to change policy become virtues rather than signs of governmental weakness, and continuity and consistency in the government's economic strategy are judged by the government's pragmatic attention to problems rather than by ideological yardsticks.

An Example: The Evolution of Rice Price Policy in Indonesia

The basic border price paradigm used by neoclassical economists to analyze the effects of government intervention in the formation of food prices argues that such intervention creates deadweight efficiency losses and lowers social welfare. Since the Great Depression in the 1930s, every country in Asia has intervened in the formation of domestic rice prices, many continuously. There is probably no wider gap between the received wisdom of mainstream economics and the point of view of policymakers. It is useful to examine the benefits from policy analyses since 1970 that have attempted to close this gap. The Indonesian case is especially well documented.

My introduction to price policy issues in Indonesia was unnerving. On my first day in the country, in April 1970, with my newly awarded Harvard Ph.D. degree not even framed, I was included in a meeting at the National Food Logistics Agency (BULOG) in which a problem with imports of PL-480 wheat flour from the United States was discussed. Staff members solicited my views on the spot, and I had barely a clue to what the right response might be. After more than two hours of discussion, I slipped away, confused about whether I really knew enough to be of use to the country, a feeling that returned often during the two years I served as resident advisor with the Harvard Advisory Group.

Remarkably, my Indonesian mentors never lost confidence that my analysis was useful. I was taken on many field trips to study problems in rice markets in Palembang, Pontianak, Surabaya, and elsewhere, and to listen to patient explanations. I tried to make sense of what farmers, traders, and consumers were saying and to understand why market prices were too high or too low. The notion that there was a correct price, and that the food logistics agency needed to defend it, was firmly implanted in my mind.

The thought that rice prices should not be stabilized never entered my head. Every country in Asia sought to stabilize rice prices, some more successfully than others. The problems to be solved were operational, not theoretical or conceptual. My academic career has been shaped by this starting point in Indonesia's rice markets and my interaction with the country's economic policymakers, many with doctoral degrees in economics from universities in the United States. Unless rice prices were stable, we believed, there would be no government to implement a development strategy and no economic development.

This approach sounds naive in a world where complete markets, perfect information, and free trade are assumed. In such a world, farmers and traders hedge the risks of price changes in rural credit and risk markets, and no government agency is needed to stabilize rice prices. Many of my academic and institutional colleagues believe that such a world exists and that price stabilization is not beneficial, distorts an economy, and acts as a drag on economic growth (World Bank 1994; Jones 1995).

Since the early 1970s, the policy analysis I have done at the university has tried to merge the determination of Indonesian policymakers to stabilize rice prices with the mainstream economic paradigm that finds all departures of domestic prices from world prices to be welfare reducing, regardless of their ability to cause economic distortions and resource misallocations. Virtually all of the insights I have gained into the importance and feasibility of rice price stabilization come from my work in Indonesia, especially with the food logistics agency.

It might seem surprising that technical economic analysis was significant in the formation of rice price policy in Indonesia, in view of the clear preference...
of policymakers for an outcome that is not valued by the prevailing economic paradigm. But the technical economic analysis conducted for the food logistics agency and the makers of macroeconomic policy provided two key elements in the widely recognized success of Indonesia’s rice price policy. Mears and Afiff (1969) laid out the original price stabilization mission; economic analysis of the Indonesian rice economy provided the foundation for the agency’s role and the measures it could implement (Timmer 1997). Much of this analysis was conducted by foreign advisors early in the agency’s development, but the agency’s ability to analyze problems itself rose significantly in the 1980s. Staff members returned from abroad with sophisticated analytical skills, and the agency invested heavily in upgrading its middle management through intensive courses on food policy analysis and applied problem solving. Thus, technical economic analysis enabled the agency to structure its mission in line with realities in the rice economy. In turn, in the 1980s, its greatly increased analytical capabilities allowed the agency to be at the forefront of the policy agenda in the Economic Cabinet on issues of direct relevance to the agency.

Three areas in which technical analysis reinforced this institutional development are discussed briefly here. Each analysis engendered professional debate, the application of methodologies to other countries, subsequent methodological developments applicable to a wider range of issues and settings, and, often, renewal of the policy debate in Indonesia.

**Marketing Margins**

An underlying goal of early price stabilization efforts was to integrate Indonesia’s regional and local rice markets as well as to defend floor and ceiling prices in individual locales. The food logistics agency was intended to be a buyer and seller of last resort rather than a monopolist in rice markets. Thus, these margins over space, time, and form were important parameters in the design and implementation of price policy (Mears 1961). As the agency became more successful in setting and defending floor and ceiling prices, and as transportation networks were reestablished, the structure and size of margins changed substantially.

The most important margin for policy purposes was the difference between the floor and ceiling prices. This margin contained all three components of marketing functions: transformations in space (farm price to urban price), time (harvest price to pre-harvest price), and form (unhusked rice to milled rice). Each component required analytical attention.

For example, little was known about the Indonesian rice milling sector in the early 1970s. An analysis of alternative rice milling techniques revealed the rapid development of a small-scale milling industry on Java and illuminated two important concerns. The analysis showed that rice milling in small mills by thousands of small entrepreneurs was the appropriate choice of technique. It was not economically optimal to either hand-pound rice or process it in large-scale in-
tegrated mills using mechanical drying and bulk handling (Timmer 1974). An intermediate technology in terms of capital intensity was economically efficient, a lesson first shown empirically in this setting. Second, the Indonesian planning process was shown to favor capital-intensive projects. This discovery legitimized longstanding concerns in the planning agency (BAPPENAS) about creating employment and reducing poverty in addition to maximizing the rate of growth. Demonstration of the economic efficiency of creating greater employment meant that these concerns could be integrated into the mainstream activities of the planning agency.6

The analysis of rice milling technique choices also provided a concrete example of links between microeconomic behavior and macroeconomic policy. All key “macro prices” influenced by government policy—foreign exchange rates, interest rates, and wage rates—are important determinants of technology choice and hence of the amount of employment, the distribution of incomes, and the efficiency of resource allocation. These micro-macro connections, crucial to any understanding of basic economic development policy impacts on rural areas, are better understood in Indonesia than in most developing countries.7

Each of the three components of the marketing margin between the floor price and the ceiling price—space, time, and form—influences private traders and the rice economy in separate and analytically distinct ways. Yet each component is affected by the decision that determines the size of the margin itself. On several occasions, the government has consciously narrowed the margin in order to ease the food price dilemma, which is that any change in food prices has opposite effects on the welfare of producers and consumers. Because the private sector handles a large share of the rice marketed in Indonesia, a decision to squeeze the margin is simultaneously a decision to squeeze the private sector. This squeeze alters the tasks for the food logistics agency; a simple model shows that the agency’s role in procurement and distribution tends to be directly proportional to the extent it squeezes the private marketing sector, but the financial burden rises with the square of the squeeze.

Despite this insight, there were constant pressures to squeeze the marketing margin. Thus technical analysis and a thorough understanding of the functioning of rice markets were subordinate to political objectives, specifically President Suharto’s goal of self-sufficiency in rice production. Suharto received a gold medal for this accomplishment from the Food and Agriculture Organization of the United Nations in 1985. An award for “food security,” rather than

6. Both topics are reviewed in Timmer (1975).
7. Chapter 5 of Timmer, Falcon, and Pearson (1983) contains a generic discussion of these micro-macro links. They are discussed more concretely in the Indonesian setting, using the rice milling example as the vehicle, in Chapter 3 of Timmer (1986b). Pearson et al. (1990) addresses the issues for the entire economy.
“self-sufficiency,” might have meant that the short-run management of rice price policy could have been more flexible.

**The Fertilizer Subsidy**

A contentious debate has been waged since the early 1980s over the social profitability of Indonesia’s fertilizer subsidy. Modest subsidies had been given for fertilizer since the earliest days of the Rice Intensification Program to stimulate adoption of a technological package that included high-yielding varieties along with fertilizer and pesticides. The subsidy became a substantial part of the Indonesian budget in the late 1970s and early 1980s, when, after the nominal price of fertilizer had been held constant for more than half a decade, the nominal floor price for rice nearly doubled. By 1983, as budgetary pressures began to be felt after the first drop in petroleum prices, Ministry of Finance officials and World Bank analysts pointed to the fertilizer subsidy as an obvious place to cut expenditures and improve resource allocation efficiency.

At the same time, the Ministry of Finance sponsored a major review of the Rice Intensification Program. Valuable village-level surveys were carried out that highlighted the extreme regional diversity of Indonesia—a diversity ill-suited to the monolithic approach of the government. The use of fertilizer on rice increased rapidly during the 15 years of the program, but administrators of the Rice Intensification Program had no control over price policies for fertilizer and rice. The initial draft of the evaluation report had no discussion of the role of prices (Development Policy and Implementation Studies 1983). In 1983, the analysts conducting this study discussed their results with the price policy analysts. A quick and rough study of the role of price policy in successful rice production was then conducted. Using aggregate time series from 1968 to 1982, fertilizer demand and rice production functions were estimated, and the social profitability of the fertilizer subsidy was calculated in total and at the margin.

The analysis and conclusions raised troubling issues for most development economists. Trends in experience and analysis in the late 1970s and 1980s reinforced the emphasis that economists placed on the superiority of economic growth under market prices to growth under prices altered by government taxes or subsidies. However, the high and robust social profitability of Indonesia’s fertilizer subsidy seemed to challenge the role of free markets. At the very least, the results confirmed what economists knew in principle but hoped to neglect in fact—that such pricing interventions could only be evaluated empirically. There were perfectly sound theoretical reasons why market failures might justify a fertilizer subsidy on efficiency grounds—the subsidy corrected a dynamic disequilibrium. No a priori arguments based on static models could settle the issue. Only empirical evidence could address the problem.

Accordingly, the debate spurred a massive flow of empirical and semi-empirical research. Nevertheless, Indonesian price policy for fertilizer attempted
to follow a consistent goal: to reduce the size of the budget subsidy while maintaining balance between production and consumption of rice through maintaining stocks held in the food logistics agency’s warehouses. When stocks were large and surplus rice was exported at subsidized prices, increases in the nominal floor price for rice were kept below the inflation rate, while fertilizer prices rose by more than the inflation rate. In short, the fertilizer price was used in tandem with other prices and programs to fine-tune the Indonesian rice economy around a trend of self-sufficiency. With imports difficult to arrange for political reasons, fertilizer price policy added a degree of freedom to an otherwise over-constrained set of policy objectives. Given Indonesia’s large influence on the world rice market, this approach may have been the only way to guarantee long-run food security for the country.

**Rice Price Policy**

The relationships between the floor and ceiling prices of rice and between the floor price and the fertilizer price are only two of several important price relationships for rice. Another is the real price of rice—that is, the price of rice relative to the costs of other goods and services—and the price of domestic rice relative to the price of imports or exports of rice.

The issues to be treated here are broad: What are the consequences of policy on the level and stability of rice prices for consumption, the growth of the rural economy, the stability of the macro (political) economy, and economic growth? High rice prices during the world food crisis in the mid-1970s led to concern about effects on poor Asian consumers. By the mid-1980s, world rice prices had collapsed, and attention turned to the subsequent influences for farmers and the health of rural economies.

This has turned out to be a difficult problem. In analytical terms, the issue cannot be treated in a partial-equilibrium framework because of the significant spillover of effects from rice markets to labor, land, and credit markets in rural areas. Rice is important as a wage good, and rural-urban migration affects equilibrium wages in the nonagricultural economy. These changes in rice prices raise significant issues for the economy as a whole. As noted earlier, however, computable general equilibrium models constructed so far for the Indonesian economy have several serious limitations. The structure of the models does not reflect the apparent complexity of market-clearing in rural Indonesia. Also, the models do not include investment functions from rural incomes, and the parameters used to generate them are not based on solid empirical evidence.

Progress has been made in understanding the mechanics of rice price stabilization and its contribution to social welfare, a topic of analytical interest to economists. Indonesia has been a proving ground for examining the hypothesis that society places a large premium on stability of food prices for reasons not apparent in some economic models of the effects of price stabilization policies.
Research demonstrating the social profitability of these policies has provided arguments used by the Indonesian government to maintain and improve its stabilization policies, despite opposition by negotiators from the World Bank, the International Monetary Fund, and the General Agreement on Tariffs and Trade/World Trade Organization.8

**Generalizations**

Is the outcome of long-term policy analysis (and advice from a single individual, on a narrow range of topics) an adequate indicator of the general benefits that might flow from this type of analysis? My experience is not unique. Many analysts, some in academia, have had similar careers and can document significant effects from research that informed the advice they gave policymakers. These experiences have a strongly idiosyncratic personal flavor. One person’s career cannot serve as a guide for everyone. Nonetheless, there are likely to be enough case studies to generalize about the replicable factors that lead to success. For the case study reported here, these factors would seem to include the following.

- The establishment of a long-term involvement with the same policymakers, or at least the same policy settings.
- The need and desire to balance the tension that confidential analysis and policy advising have with the ultimate publication of key models and results.
- A willingness to stay within the mainstream analytical paradigms of the economic profession while examining the impact of particular deviations from underlying assumptions.
- A continuing demand from policymakers for problem-oriented analysis.

These factors, in turn, raise controversial issues about how to train policy analysts to be effective in empirical settings and how to make policy analysis more useful and influential.

The debate and controversy have important implications for how policy analysts should be trained. At a minimum, policy analysts should be trained well enough as technical economists to engage in analytical and methodological debates with their professional colleagues. The goal is not necessarily to have them win the argument, but simply to have it take place, because many useful insights can emerge from vigorous debate. However, winning such debates on occasion, especially in a public forum where policymakers are listening, empowers good policy analysis.

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8. See Timmer (1989, 1996) for a review of these issues and a preliminary effort to quantify the costs and benefits of Indonesia’s rice price stabilization program.
A major stumbling block to working in policy analysis and advising is the lack of a career path that rewards analysis that actually makes a difference in the outcomes of policy. Some universities acknowledge a practitioner’s need to work in developing countries. The university receives two benefits. First, teaching courses that have direct relevance to the real world can stimulate interest among students in pursuing such research. Second, when policy analysis is done well, the methodologies and empirical results can be published. This multiplies their impact on students and colleagues.

In a development context, synergistic effects emerge when research is carried out over time. Within the country, trust builds between policy analyst and policymaker, and demand for policy analysis increases. Getting the policy questions right in the first place is essential. Policy advice and implementation that is successful can build momentum. Each country, and each unique economic setting, contains a wide array of problems that can be addressed by policy-oriented research, but with care lessons can also be transferred across countries. Skilled policy analysts can multiply their impacts by carrying solutions learned in different regions or countries back and forth between settings that are similar enough so that their relevance is obvious to policymakers.

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PART III

Economic Information in the Policy Process
Some seventeen years ago, I wrote: “Agricultural economists need to evaluate their own research priorities. The main difficulty in doing so is to value the types of information generated by economic research. Bayesian decision theory provides a framework for valuing information, and . . . most of the other determinants of research priorities can be encapsulated in a target return ratio measure” (Lindner 1987, 1).

The suggestion that the principal output of social science research is information, that such information needs to be valued if we are to establish meaningful research priorities, and that Bayesian decision theory provides the appropriate conceptual framework for valuation was not a novel idea then, and it is even less novel today. Nevertheless, in the intervening years there has been a paucity of studies that apply the proposed approach either to measuring the benefits of agricultural economics research or to developing, refining, or demolishing it. The invitation to revisit this topic and to reexamine some of the ideas put forward in my earlier paper afforded a good opportunity to reassess critically the case for using such an approach, and to speculate on the reasons for the apparent lack of adoption.

Outline of a Framework for Economics Research Evaluation

I start this chapter with an excerpt from my 1987 Australian Agricultural Economics Society presidential address, but without the normal preliminaries and some of the detail on Research Evaluation Guidelines, which was not specific to the evaluation of social science research. At this point, I do not elaborate on some of the caveats noted in that paper, or rectify omissions of some subjects that in hindsight should have been included at least peripherally but were not mentioned because of time constraints.

Specifically, the following section contains an edited version of the core of my original address, and for the most part follows the same structure. Topics covered include:
• The premise that the typical product of social science research is information.
• An outline of the principles of Bayesian decision theory relevant to the estimation of the value of information.
• A (now dated) literature review of the application of this approach to the estimation of the value of economic information.
• Some general guidelines for the assessment of agricultural economics research.
• An illustration of the proposed approach using three selected areas of agricultural economics research.

What Benefits Agricultural Economics Research?

It can be argued that certain types of farm management and production economics research are directly analogous to agricultural science research in the sense that the aim is to develop innovations in the form of new decisionmaking strategies with the potential to lower average costs of production. Examples include research on management strategies for integrated pest management, the application of linear programming to whole farm planning, and the development of optimal marketing rules. Procedures comparable to those used to estimate the benefits from scientific research can be applied to these cases.

Most social science research does not generate innovations in the sense noted above, which means that a different conceptual framework is needed if the benefits of such research are to be valued.

A pioneering attempt to provide such a framework was made by Norton and Schuh (1981), who started from the proposition that the output from social science research is information rather than a new or improved product. In their view, even if this information leads to someone producing a better product, it is not the research per se that actually creates the product.

They classified the types of information provided by social science research into seven basic categories:

1. Management information.
2. Price information.
3. Institutional information.
4. Product and environmental quality information.
5. Information on human nutrition.
6. Information to aid in adjusting to disequilibria.
7. Information to help reduce rural poverty.

For all of these categories of social science output, the essential point is how to value the information that research produces. In an earlier paper, Hirshleifer (1973) identified the following economically significant attributes of information:
- **Applicability** refers to the number of decisions, or decisionmakers, to which the information is applicable.
- **Content** refers to the uncertain "state of nature" to which the information is applicable, such as consumer tastes, endowments or resources, technology, and market characteristics such as price or quality of traded goods.
- **Certainty** refers to the degree of concentration of posterior belief distributions dictated by the information. (Fully certain information assigns 100 percent probability to a single value of the variable being predicted.)
- **Decision relevance** refers to information quality, sometimes referred to as reliability, or degree of informativeness.

Just as a precondition for applied agricultural research to contribute to economic growth through greater efficiency is the adoption of the innovations generated by the research, the analogous condition for this research is that the information so generated must be used as an input to decisions made by consumers, producers, or government. Therefore, a starting point for the evaluation of a particular area of economics research is to establish the content of the information produced by the research, that is, to delineate those uncertain states about which it is intended to provide information. Once established, this content can be used to identify those decisions that might be influenced by the research results. This step is equivalent to the identification of the group affected by technological innovation in agriculture.

In identifying those decisions for which different types of research information might be valuable, farmers are the most obvious, but not the only, potential users of agricultural economics research information. Others include agribusinesses and consumers, as well as the voters, politicians, and bureaucrats who comprise government. Ethridge (1985, 1) argues, "It is the farm sector that must be accessed to supply most of the information, but it is predominantly other sectors that use and interpret the information." He suggests that nonfarm agribusiness firms are likely to utilize agricultural data for the following types of decisions: "production and inventory, peak workloads, hiring practices, pricing strategies, magnitude, and timing of investment in capital and research, ... market development activities, etc."

If the number of decisions that could be affected were enumerated consistently before new research was initiated, research priority setting would most likely benefit from the discipline and objectivity so imposed, not least because this measure provides a crude proxy for the all-important scale effect. To be useful, however, evaluation needs to go beyond consideration of the scale effect to identify any systematic differences in the "per decision" value for various types of research information. Although this value of information per unit of scale is likely to depend on firm-specific variables as well as on the type of information and on the nature of decisions, it is convenient to ignore this interfirm variability.
so as to concentrate in this chapter on the determinants of the value of information for an "average" decisionmaker. Aggregate research benefits can then be treated as the product of the number of decisions (scale) and the average value per decision. Discussion of this admitted oversimplification is deferred to a later section.

Various methods of valuing information have been reported in the literature. For instance, Eisgruber (1978, 903) identified the following three different schools of thought in his literature review: (1) the decision theoretic, (2) the net social benefit, and (3) the scoring approaches. But even he admitted that the latter scarcely deserves the title of an "economic theory of information." Freebairn (1978) also identified three alternative models for quantifying the benefits of outlook information, but included the "information theory" model in lieu of the scoring approach.

Marschak (1968) argues that the basis of "information theory" is an engineering-type concept that only permits the amount of information transmitted by a communication channel to be quantified in physical units. In his view this theory does not naturally lend itself to assigning economic values to "inquiry" (that is, the production of data) or to "deciding" (that is, the use of data by decisionmakers). Theil (1967) has shown how the basic framework can be so extended, but, as Leuthold (1971) demonstrates, when information theory is used to put an economic value on information, the approach is effectively the same as that of Bayesian decision theory.

The Bayesian Decision Theoretic Approach to Information Evaluation

Bayesian decision theory, in my view, provides the most logical and insightful conceptual framework for valuing information. The essential concepts are quite simple. To facilitate the exposition consider the case of a potential adopter faced with a simple two-action decision problem of whether to adopt a newly developed innovation on the one hand, or on the other to persist with previous practice. The various consequences of this choice are depicted in Figure 7.1.

The decision problem is characterized as the selection of one of a number of alternative and mutually exclusive acts. For the illustrative example of innovation adoption, there are only two such acts, namely adoption or rejection of the innovation. Choice of the best alternative is complicated by the fact that the decisionmaker is uncertain about the outcomes of the alternative acts, specifically the benefits of adoption. Despite the decisionmaker's uncertainty about whether innovation adoption is in fact beneficial or not, a choice must be, and is, made each time an opportunity emerges to alter the method of production.

Hence the possibility exists that the chosen act will prove, ex post, to be suboptimal. This event carries an associated opportunity cost (or loss or regret) relative to that act that is in fact optimal. The possibility also exists that the
FIGURE 7.1 A classification of four possible types of innovation usage decision given imperfect knowledge

<table>
<thead>
<tr>
<th>STATES: Defined by act chosen with complete knowledge</th>
</tr>
</thead>
<tbody>
<tr>
<td>Innovation is “good.”</td>
</tr>
<tr>
<td>Increases welfare.</td>
</tr>
<tr>
<td>Innovation is “bad.”</td>
</tr>
<tr>
<td>Does not increase welfare.</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>BELIEFS: Defined by act chosen with incomplete knowledge</th>
<th>CASE 1</th>
<th>CASE 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Adoption is “good.” Perceived to increase welfare</td>
<td>Correctly adopt</td>
<td>Incorrectly adopt</td>
</tr>
<tr>
<td></td>
<td>No error</td>
<td>Type 2 error</td>
</tr>
<tr>
<td>Adoption is “bad.” Perceived to decrease welfare</td>
<td>Incorrectly reject</td>
<td>Correctly reject</td>
</tr>
<tr>
<td></td>
<td>Type 1 error</td>
<td>No error</td>
</tr>
</tbody>
</table>

chosen act is identical to the (truly) optimal act. In the latter case, there would be zero opportunity loss. A priori, both possibilities need to be recognized, and the expected value of this opportunity loss is commonly referred to as the cost of uncertainty.

Another conceptually distinct cost of imperfect knowledge has been the focus of much greater attention by economists. This is the cost of risk, the size of which is a function both of the perceived level of risk and decisionmakers' attitudes to risk.

Information is valuable because it has the potential to reduce either or both of these costs. At best, more information can eliminate both of these costs. Consequently, we have an upper bound on the possible value of the research being evaluated if we can estimate, however crudely, both the cost of uncertainty and the cost of risk.

Of course, information is rarely, if ever, perfect. In practice, the expected value of (imperfect) information will be some proportion of the expected value of perfect information. Precise quantification of the actual reduction in the costs of uncertainty or risk can be computationally difficult, but guidelines for obtaining crude bounded “guesstimates” can be derived from the basic principles of Bayesian decision theory, as well as from established findings in the literature.

A standard result from both statistical sampling theory and business decision theory is that the value of information increases monotonically with extra information (that is, more messages), even if at a progressively declining rate. Value also is a monotonic nondecreasing function of the accuracy, reliability, or precision of the information. Therefore, guesstimation of the value that a decisionmaker will place on information produced by social science research can

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1. For instance, see Byerlee and Anderson (1982) or, in a different context, Campbell and Lindner (1985).
be simplified by partitioning it into two stages: (1) guesstimate the components of the expected value of perfect information (that is, the cost of uncertainty and the cost of risk) and (2) guesstimate the likely proportionate reduction in expected value of perfect information achievable by the particular type of research being evaluated.

This suggested partitioning is based on the premise that the expected value of perfect information is state specific, whereas the achievable degree of reduction in expected value of perfect information is information specific. In other words, the expected value of perfect information is determined solely by the characteristics of those decisions likely to be affected by the results of the research. Characteristics of such decisions include:

- Flexibility (the range of alternative courses of action available).
- Payoff sensitivity (sensitivity of outcomes to decision choices).
- Preference sensitivity (sensitivity of choices and returns to attitudes to risk and so on).
- Degree of prior uncertainty (as indexed by variance of prior beliefs).

In other words, it is hypothesized that the expected value of perfect information is generally much simpler to compute than the actual value of a particular type of imperfect information. The former value is independent of characteristics of the information and underlying research process being evaluated. Consequently, for each different type of agricultural economics research, it might be possible to economize on computation costs by using a common set of guesstimates of expected value of perfect information.

Conversely, the second stage, involving guesstimation of the proportionate reduction in expected value of perfect information, is determined principally by the characteristics of the information system in question, and so offers less scope for exploiting economies of size. In formal Bayesian terms, the two relevant characteristics of the information set are the number of messages and the "informativeness" of these messages as expressed by the likelihood function. In everyday terms, the greater the accuracy, reliability, or precision of the information, the higher the proportionate reduction in expected value of perfect information.

For the simple two-action, two-state-of-the-world type of problem discussed earlier, the perceived accuracy of information from research can be expressed in the form of likelihood probabilities, that is, the probability of predicting a state of nature conditional on it in fact being the true state. In an empirical study of the value of a weather forecast assuming multiple discrete states, Doll (1971) found the marginal value of information to be an increasing function of the likelihood probability. For the purpose of research evaluation, assuming a proportional relationship simplifies the process and may well be the most reasonable approach given current knowledge.
Paradoxically, the development of simple guidelines to estimate the value of research information as a proportion of expected value of perfect information is actually easier for more complicated problems in which an infinite number of possible states exist, and the decisionmaker is faced with an almost continuous range of possible courses of action. An example is the production of information about market prices. Fortunately, there is a small but instructive body of literature on the evaluation of market outlook research, including the pioneering article by Hayami and Peterson (1972) and a more recent set of articles by Freebairn (1976a, 1976b, 1978). In my view, the intriguing aspect of Freebairn's work is that his results are functionally equivalent to those obtainable from Bayesian decision theory even though he developed a modified version of the net social benefits approach to valuing outlook information pioneered by Hayami and Peterson (1972).

In Bayesian decision theory terms, price prediction is an example of the class of intimate action problems known as point estimation. Winkler (1972) shows that, for this class of problem, determining the expected value of perfect information is especially simple as long as the loss function is quadratic. Furthermore, the optimal point estimate is the mean of the belief distribution, and the expected value of perfect information is directly proportional to the degree of uncertainty as measured by the variance of normally distributed beliefs. Likewise, the proportionate reduction in expected value of perfect information attributable to extra information simply equals the ratio of the number of observations embodied in this extra information to the total number of observations embodied in posterior beliefs.

Freebairn (1976a, 1976b) also found that the value of price predictions derived from outlook research is proportional to the reduction in variance of forecasting error. This functional equivalence to the Bayesian results is explained by Freebairn's demonstration that the net loss of social welfare from price forecasting error is a quadratic function of the size of the error as long as farmers' forecast prices are unbiased estimates of realized price. Freebairn posited a rational expectations justification for this assumption; however, as noted above, such an assumption also is consistent with the decision theoretic approach as long as farmers' prior beliefs are unbiased. Either way, the result suggests a simple procedure for estimating the value of information from agricultural economics research for those situations in which the prior degree of uncertainty can be expressed as a variance or the effect of research information can be estimated in the form of a proportionate reduction in prior variance.

Some idea of the likely magnitude of expected value of perfect information for some types of decision, or of the possible proportionate reductions in expected value of perfect information for some types of information, can be gauged from the results of empirical studies in which the value of specific types of information has been estimated. A selection of these estimates is summarized in Table 7.1.
TABLE 7.1 Selected empirical estimates of value of information per year to farmers

<table>
<thead>
<tr>
<th>Author</th>
<th>Risk attitude</th>
<th>$E[\Pi^I]^a$ (U.S. dollars)</th>
<th>Unit</th>
<th>Type of information</th>
<th>$E[VSI]^b$ (U.S. dollars)</th>
<th>Reduction (percent)</th>
<th>Product</th>
<th>Decision type</th>
</tr>
</thead>
<tbody>
<tr>
<td>Eidman, Dean, and Carter (1967)</td>
<td>Neutral</td>
<td></td>
<td>Head</td>
<td>Price outlook</td>
<td>0.06</td>
<td></td>
<td>Turkeys</td>
<td>Selling strategy</td>
</tr>
<tr>
<td>Bullock and Logan (1970)</td>
<td>Neutral</td>
<td></td>
<td>Head</td>
<td>Price outlook</td>
<td>1.81</td>
<td></td>
<td>Cattle</td>
<td>Selling strategy</td>
</tr>
<tr>
<td>Freebairn (1976b)</td>
<td>Neutral</td>
<td></td>
<td>Aust</td>
<td>Price outlook</td>
<td>1.10m</td>
<td></td>
<td>Wool</td>
<td>Production level</td>
</tr>
<tr>
<td></td>
<td>Neutral</td>
<td></td>
<td>Aust</td>
<td>Price outlook</td>
<td>0.09m</td>
<td></td>
<td>Wheat</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Neutral</td>
<td></td>
<td>Aust</td>
<td>Price outlook</td>
<td>0.004m</td>
<td></td>
<td>Barley</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Neutral</td>
<td></td>
<td>Aust</td>
<td>Price outlook</td>
<td>0.16m</td>
<td></td>
<td>Potatoes</td>
<td></td>
</tr>
<tr>
<td>Norton and Schuh (1981)</td>
<td>Neutral</td>
<td>0.71</td>
<td>Bus</td>
<td>Price outlook</td>
<td>0.51</td>
<td>72</td>
<td>Soybeans</td>
<td>Selling strategy</td>
</tr>
<tr>
<td></td>
<td>Neutral</td>
<td>0.71</td>
<td>Bus</td>
<td>Price outlook</td>
<td>0.21</td>
<td>30</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ryan and Perrin (1974)</td>
<td>Neutral</td>
<td>200c</td>
<td>Ha</td>
<td>Response function</td>
<td></td>
<td></td>
<td>Potatoes</td>
<td>Fertilizer application</td>
</tr>
<tr>
<td>Doll (1981)</td>
<td>Neutral</td>
<td>2.50</td>
<td>Acre</td>
<td>Weather forecast</td>
<td>1.30</td>
<td>52</td>
<td>Corn</td>
<td>Growing strategy</td>
</tr>
<tr>
<td></td>
<td>Neutral</td>
<td>2.50</td>
<td>Acre</td>
<td>Weather forecast</td>
<td>0.05</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Byerlee and Anderson (1982)</td>
<td>Preferring</td>
<td>480</td>
<td>Farm</td>
<td>Weather forecast</td>
<td>52</td>
<td>11</td>
<td>Wool</td>
<td>Drought strategy</td>
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<tr>
<td></td>
<td>Neutral</td>
<td>520</td>
<td>Farm</td>
<td>Weather forecast</td>
<td>59</td>
<td>11</td>
<td></td>
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<td></td>
<td>Averse</td>
<td>550</td>
<td>Farm</td>
<td>Weather forecast</td>
<td>58</td>
<td>11</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bosch and Eidman (1985)</td>
<td>Preferring</td>
<td>4.20</td>
<td>Ha</td>
<td>Soil moisture</td>
<td>1.00</td>
<td>24</td>
<td>Corn and soybeans</td>
<td>Irrigation scheduling</td>
</tr>
<tr>
<td></td>
<td>Preferring</td>
<td>4.20</td>
<td>Ha</td>
<td>Soil moisture</td>
<td>1.40</td>
<td>33</td>
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<td></td>
</tr>
<tr>
<td></td>
<td>Neutral</td>
<td>6.98</td>
<td>Ha</td>
<td>Soil moisture</td>
<td>3.68</td>
<td>53</td>
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<td></td>
</tr>
<tr>
<td></td>
<td>Neutral</td>
<td>6.98</td>
<td>Ha</td>
<td>Soil moisture</td>
<td>5.04</td>
<td>75</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Averse</td>
<td>16.30</td>
<td>Ha</td>
<td>Soil moisture</td>
<td>14.40</td>
<td>88</td>
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<td></td>
</tr>
<tr>
<td></td>
<td>Averse</td>
<td>16.30</td>
<td>Ha</td>
<td>Soil moisture</td>
<td>15.00</td>
<td>92</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Preferring</td>
<td>4.20</td>
<td>Ha</td>
<td>Weather forecast</td>
<td>1.00</td>
<td>24</td>
<td>Corn and soybeans</td>
<td>Irrigation scheduling</td>
</tr>
<tr>
<td></td>
<td>Neutral</td>
<td>6.98</td>
<td>Ha</td>
<td>Weather forecast</td>
<td>1.08</td>
<td>15</td>
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</tr>
<tr>
<td></td>
<td>Averse</td>
<td>16.30</td>
<td>Ha</td>
<td>Weather forecast</td>
<td>0.70</td>
<td>4</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**NOTE:** Aust, Australia; Bus, bushel; Ha, hectare; m, million.

*a* Expected value of perfect information.

*b* Expected value of "sample" information.

*c* Not based on Bayesian decision theory.
It would be dangerous to attempt to draw too many general conclusions from such fragmentary evidence. One noteworthy aspect is that, with few exceptions, the proportionate reduction in expected value of perfect information is greater than 10 percent, and often greater than 50 percent. As for guidelines to the value of information relative to gross value of production, Freebairn (1976b) found that even with a 50 percent reduction in forecast errors, net benefits to society would increase by less than 1 percent of gross value of output. In one sense, this estimate is biased upward by the implicit assumption of complete adoption. In another sense, it is biased downward because it ignores benefits to other economic agents besides farmers. On balance, I suspect that it is an upper bound estimate of realized benefits.

To extrapolate to other potential demands for information from such empirical estimates of expected value of perfect information as do exist, some knowledge of the nature of the relationship between information value and its determinants is needed. In a comprehensive review of findings to that time, Hilton (1981) concluded that in general there is no monotonic relationship between the value of information and any of the determinants of expected value of perfect information. While the nihilism of this conclusion rivals that of Arrow’s impossibility theorem, Hilton partially rectifies this by cataloging the findings from a number of studies of particular cases that provide some evidence of fairly consistent relationships between expected value of perfect information and its determinants. In Table 7.2, his summary has been supplemented by the inclusion of some additional results, but a systematic effort to augment the information contained in this table would be of general benefit.

Research Evaluation Guidelines

As for any other form of research, under risk neutrality the criterion for investment in social science research should be that the expected net present value $E[\text{NPV}] > 0$, with

$$E[\text{NPV}] = \sum (-C_t + p_t d_t G_t)(1 + i)^{-t}$$

(7.1)

where $C_t =$ cost of research in year $t$; $G_t =$ potential gross annual research benefits in year $t$; $p_t =$ probability that research year output is available by year $t$; and $d_t =$ proportionate level of adoption of research output by year $t$. This expression reduces to:

$$E[\text{NPV}] = C[pGC^{-1}[(1 + i)^n - 1](1 + i)^{-(h+m+n)} + (1 + i)^{-h} - 1]/I$$

(7.2)

if $C_t = C$ for years 1 to $h$, and zero thereafter; $d_t = 1$ for years $(h + m)$ to $(h + m + n)$ and zero for other years; $p_t = p$ for years $h$ to $(h + m + n)$ and zero for other years; and $G_t = G = (RVT)$ for years $h$ to $(h + m + n)$, where $V =$ gross value of production affected by research; $I =$ expected value of perfect information per
<table>
<thead>
<tr>
<th></th>
<th>Increasing</th>
<th>Nondecreasing</th>
<th>Decreasing</th>
<th>Indeterminate</th>
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<tbody>
<tr>
<td>Decision flexibility</td>
<td>Merkhofer (1977)(^a)</td>
<td>Hilton (1979)(^a)</td>
<td>Ohlson (1965)(^a)</td>
<td>Campbell and Lindner (1985)</td>
</tr>
<tr>
<td>Decisionmaker’s wealth</td>
<td>Ohlson (1975)(^a)</td>
<td></td>
<td>Ohlson (1965)(^a)</td>
<td>Campbell and Lindner (1985)</td>
</tr>
<tr>
<td>Decisionmaker’s prior risk</td>
<td>Hilton (1979)(^a)</td>
<td>Marschak and Radner (1972)(^a)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Information accuracy</td>
<td>Doll (1971)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Wilson (1975)(^a)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Ijiri and Itami (1973)(^a)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Hilton (1979)(^a)</td>
<td></td>
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<td></td>
<td>Doll (1971)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Byerlee and Anderson (1982)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Information amount</td>
<td>Freebairn (1978)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Various(^b)</td>
<td></td>
<td></td>
<td>Kihlstrom (1974)</td>
</tr>
</tbody>
</table>

\(^a\)From Hilton (1981).

\(^b\)For instance, see Winkler (1972) and Degroot (1970).
unit value of production; and $R =$ proportionate reduction in expected value of
perfect information due to research.

Expressing the formula in this way permits exploration of the sensitivity
of $E[\text{NPV}]$ to changes in the following variables: (1) annual research costs, $C$;
(2) the discount rate, $i$; (3) implementation delays, $m$; (4) realization period, $n$;
and (5) the ratio of expected gross annual research benefits to costs, $pRVI/C$.

Note that $E[\text{NPV}]$ is linear in $C$. This is convenient as it permits average
annual research costs to be treated as the numeraire, thereby reducing the num-
ber of variables requiring explicit discussion. By contrast, $E[\text{NPV}]$ is nonlinear
in terms of research duration, $h$.

Of the other parameters in the equation, the implementation delay encom-
passes both the discovery lag while potential users of the research results become
aware of their existence, and the average time lag to adoption by potential users.
This implementation time lag and the length of time over which potential re-
search benefits are likely to be realized are both hypothesized to differ system-
atically between different research areas.

The last factor is the composite variable, $pRVI/C$, or target return ratio.
This ratio subsumes many of the crucial and difficult-to-measure determinants
of net research benefits, such as probability of success, level of adoption, the
potential reduction in the costs of uncertainty and of risk per unit of scale, and
the proportionate reduction in these costs actually realized.

Given estimates for each case of the time to complete the research, the de-
lay before implementation, and the number of years before the results become
obsolete, it is a simple matter to calculate the target return ratio for expected net
present value to be positive. Computed ratios for selected parameter values are
presented in Table 7.3.

Subjective estimates of probability of success, scale effect, expected value
of perfect information per unit of scale, proportionate reduction in expected
value of perfect information, and annual research costs can then be combined
to assess whether or not the target return ratio is likely to be exceeded.

Research Evaluation—Three Hypotheticals

To illustrate how the suggested framework can be used to guide the evaluation
of agricultural economics research, I will use three examples. The first concerns
choice of technique decisions by farmers, the second utilizes the literature on
the ex ante evaluation of outlook research, while the third is a more speculative
attempt to apply the same principles to the evaluation of agricultural policy
research.

First, consider linear programming studies designed to identify a more
profitable allocation of fixed farm resources utilizing established technology.
Such studies can be completed quite quickly and implementation delays will be
relatively short, but benefits also will be short lived because of the volatility of
some parameter values and the rate of obsolescence of some knowledge encoded in the model. It can be seen from Table 7.3 that if all three lags equal two years, then the target return ratio for research will be slightly more than one. Other calculations show it could be as low as 0.5 for research taking only one year but yielding benefits for at least four.

Given that the requirement for creative effort is modest, the likelihood of success is quite high—almost certainly greater than 0.5 provided that the research staff are competent. If, for example, annual research costs are about $50,000, a conservative estimate of the potential annual research benefits necessary might not exceed $50,000 but could easily rise to $500,000. The information generated by this type of research is quite location specific, and even firm specific, so the scale effect will not be large. Thus, the value of research information per farm would need to be large, perhaps as high as $5,000. It is unlikely that the cost of uncertainty plus the cost of risk would be this large even if existing farm plans are based solely on tradition. Any lack of credibility concerning the validity of the results would exacerbate the difficulty of providing a positive net return.

A somewhat different picture emerges if the linear programming models are developed to evaluate new farming practices. The required target return ratio is likely to be smaller than that in the previous case because the time period
over which benefits are realized should be longer. For most innovations, the scale effect also is likely to be appreciably larger than for the previous case. Furthermore, considerable uncertainty exists about potential performance of any new innovation, so the expected value of perfect information per farm also is likely to be substantial. It is debatable whether advice by "academic" economists will have much credibility with potential adopters. If it does not, then the research results will not reduce the expected value of perfect information very much, if at all. On the other hand, if farmers regard the results of such research as reliable, then actual information should justify the research. Biotechnology and information technology are now "flavors of the month," and some agriculturalists are predicting that they will generate a further revolution in farming practice. If these predictions are fulfilled, then relatively pedestrian research investigating the profitability of the seeds of this revolution should be accorded more status by the profession than it currently enjoys.

My second example concerns outlook research. As this area of research has already been the subject of more investigations than most others, I will be brief and focus mainly on the probability of success. In the context of outlook research, success needs to be defined as achieving a reasonable reduction in forecasting error. The a priori likelihood of achieving such an aim varies widely depending on the uncertain "state of nature" that the research is attempting to predict.

Consider wheat planting intention surveys and subsequent crop forecasts as a case in point. The probability that such research will succeed must be close to 100 percent. In addition, research costs are comparatively modest, the period of research duration and the implementation delay are both very short (that is, less than one year), and the number of potential users of such information is extremely large. Depending on the marketing arrangements for the commodity in question, farmers may or may not use crop forecasts to "fine tune" production and harvesting decisions, or selling strategies. Irrespective of farmers' demand for this information, it is likely to be used by input suppliers to estimate inventory needs, by transport firms to predict traffic loads, by processing and storage firms to plan labor requirements, by merchandising firms to reassess price expectations, and by financial firms to calculate financing needs.

Thus even if benefits to individual decisionmakers are very small and "adoption" levels correspondingly low, net research returns are likely to be positive notwithstanding the ephemeral value of the information produced. In contrast to the value of surveys for wheat, the value of equivalent surveys for minor crops may be too low because the number of potential data users will be correspondingly smaller. For some crops, this may be compensated for by a less rigid marketing system that increases decisionmakers' flexibility of actions and with it the value of information per decision.

In contrast to the above case, some parameters are exceedingly difficult to forecast. Tomorrow's exchange rate and next month's futures prices are cases
in point. For such parameters, the probability of successful forecasts may be so close to zero as to ensure that \( E[\text{NPV}] < 0 \) almost irrespective of the size of the potential value of more accurate forecasts.

Finally, I offer some conjectures on policy research. In my opinion, this is an area where implementation delays are likely to be substantial. On the other hand, an extended duration of research benefits is problematic given the penchant of newly elected governments to do something, to do anything to convince voters that they are in charge. In Table 7.3, note that if the implementation lag is 32 years and if research benefits last only four times as long as it takes to complete the research, then the target return ratio can be as low as one for a short-lived project, and as high as six for an extended project. Furthermore, this is an area of research where casual empiricism suggests that the probability of successful adoption of the research results is very low. Consequently, the ratio of potential annual benefits to annual research costs would need to be greater than 50, or perhaps even 500, for investment in this area of research to be justifiable. I leave the question of whether or not particular types of policy research can satisfy such a requirement for others to debate.

**Caveats, Concerns, and a Critique**

**Caveats**

At best, the above framework is applicable only to "mission-oriented" agricultural economics research, and bypasses the virtually intractable problems of basic research. Mission-oriented economics research is defined to include all research undertaken by economists that generates information of direct relevance to consumption, production, investment, or policy choices. The tacit assumption that information has value only to the extent that it (potentially) improves decisionmaking is almost certainly unduly restrictive. Some individuals will almost certainly derive utility directly from the knowledge content of information. Whether this consumption value of social science research is sufficiently large to warrant consideration in setting priorities for agricultural economics research is a moot point that has not received much attention.

The framework outlined above also is unduly mechanistic, and fails to recognize the human factor. As Koestler (1964) so exceptionally portrayed, the process of scientific discovery is a creative activity, and as in any creative activity, the human factor is a critically important determinant of research productivity.

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2. No doubt practitioners of the art will object that, without their efforts, agricultural policy would be in even greater disarray. If so, the avoided incremental cost of even larger butter mountains, lakes of olive oil, and grain pools forms part of the benefits of such research.
Furthermore, the danger with any formal system of research evaluation is the onset of diminishing and even negative returns to effort, the opportunity cost of which is actually doing research. Because research is a creative endeavor, probably any research evaluation is best based on self-assessment of the allocation of scarce time between alternative professional activities. Perhaps all that is needed is a conceptual framework that can be used from time to time in an informal, subjective manner when mapping out career plans and making similar pivotal choices.

Concerns

A cursory scan of the literature indicates that the approach advocated in my earlier paper has had very little impact. Moreover, there seems to have been little additional research since 1987 seeking to quantify the value of information applicable to management or policy decisions. It is not difficult to invent numerous "explanations" for this outcome. While it would be gratifying to be able to claim that the task was complete, and that there is no need for further research, this is unconvincing. So is the equally simplistic explanation that Bayesian decision theory has been totally discredited, although it may be that this theory is out of fashion as a representation of how decisionmakers use information to reduce the twin costs of uncertainty and risk. A more instructive approach may be to reassess critically some of the essential premises on which my 1987 paper was based, and a few ideas are presented below. In the main, however, this task is best left to others.

At the core of my earlier paper is the premise that Bayesian decision theory provides a logical conceptual framework for valuing information from social science research. This proposition is at variance with Eisgruber (1978), who argued that neither the theory nor the methodology exists to address the economics of information adequately.

Before reexamining the utility of Bayesian decision theory as an appropriate conceptual framework for valuing information, the extent to which information fits the definition of a public or private good merits some discussion. Implicit in my earlier paper was the conventional view that information is a public good, at least in the sense that it has the characteristic of being nonrival in use. On this basis, I postulated that aggregate benefits from information generated by research can be treated as the product of the number of decisions that potentially might utilize the information and the average value of the information per decision. This position mirrors that of Hirshleifer (1973), who listed the number of decisions, or decisionmakers, to which the information is applicable as one of the economically significant attributes of information.

Clearly, social science research does produce information that is nonrival in use. Market intelligence information and findings about more efficient policy options are just two examples, at least for the small-country case. On reflection though, it is clear that other types of information are at least partially rival in
use, and producers of such information often go to some lengths to try to make it price excludable. For instance, Salin et al. (1998, 115) note that “Information is often most valuable to the extent that it is tailored to firm-specific markets and conditions. Information that is sufficiently specific to be valuable is often proprietary. . . . [and that] . . . Some information services are best provided by the private sector. Firms generate proprietary data to assist in strategic decision making, but do not share such information with competitors.”

In other words, certain types of information produced by a form of social science research are more akin to a private than a public good, because use by others diminishes the value of the information to the initial users. Consequently, the public and private values of such information may differ significantly.

I did not recognize the possibility of a market for proprietary information with private good characteristics in my earlier paper, and the framework advocated therein is inadequate to value this type of information. Nor will collective willingness to pay as revealed by market transactions necessarily provide an accurate measure of value. While proprietary information may be rival in use, multiple use of the same information is possible and likely. Hence the need to account for the externalities arising from more or less widespread use of such information is one, but not the only additional complexity involved in the valuation of such information. Salin et al. (1998, 115) observe that “It may be difficult for users to assess the quality of the information services they purchase from private sources.” This difficulty arises because ex ante, potential purchasers cannot independently determine the accuracy, reliability, or precision of the information that is being offered for sale. As a result, actual prices paid for this information may either over- or undervalue the information. The complex issues faced by purchasers of information with unknown characteristics are treated thoroughly in a paper by Gallini and Wright (1990).

A Critique

Notwithstanding evidence that some information does not conform to the characteristics of a public good, most does. Whether Bayesian decision theory provides a suitable framework for valuing this type of information remains an issue. Many of the key propositions of Bayesian decision theory are based on impeccable logic, and still seem applicable to the task of valuing information generated by social science research. Such propositions include:

- The essence of decisionmaking is the selection of a preferred act from a number of different and mutually exclusive alternatives.
- Choice is complicated by uncertainty about the “state of nature,” which codetermines the outcomes of the alternative acts. Despite such uncertainty, a choice must be made.
- Ex post there is the possibility of regret (that is a loss from a “wrong” choice).
• Ex ante, the expected value of regret is the cost of uncertainty.
• Another cost of imperfect knowledge is the cost of risk.
• Perfect knowledge would eliminate the costs of both uncertainty and risk.
• The expected value of perfect knowledge is an upper bound on the value of information from research.
• These propositions establish the potential value of information, which reduces imperfect knowledge about the "state of nature," and so can be used by decisionmakers to make better decisions.

There are, however, other tenets of Bayesian decision theory that are more controversial in the context of valuing information from social science research. In particular, the following contentious propositions are implicit in accepted procedures for calculating the expected value of social information:

• Information generation is stochastic but objective in the sense that it effectively involves sampling at random from the "true state of nature."
• As a corollary, all essential aspects of the information generating process can be summarized by the likelihood function.
• Any information will reduce the twin costs of uncertainty and risk.
• Decisionmakers use any and all information about which they become aware.

This first proposition is pivotal and at the heart of reasons why the other propositions are contentious. In some social sciences, it is fashionable to deny the validity of the "state of nature" as an objective and independently verifiable concept. Even if a more traditional view is accepted, characterizing all social science research as a process of objectively sampling at random from the "true state of nature" strains credulity.

To be candid, if we are to make progress in valuing the output from social science research, we need to recognize that sometimes so-called research produces disinformation rather than information. To take the next step, and attempt to classify a particular piece of information as one or the other, or a shade of gray in between, is to enter dangerous territory.

In Lindner (1987, 98), I did draw attention to the significance of information quality, and noted that "As a profession we have barely scratched the surface of the economics of disinformation." However, at that time I believed that these problems could be accommodated within the framework of Bayesian decision theory. Specifically, I envisaged that the reliability or informativeness of information could be captured in the variance of the likelihood function, while veracity or falsity of information could be treated analytically as bias in the information generating process. This now seems rather naïve, at least in terms of dealing with information veracity in a satisfactory manner. Bias in the sense
used in statistical sampling theory is a knowable, even if unknown parameter. Bias in the context of social science research is a much less tractable notion.

To use the logic of decision theory, an infinite amount of information will lead to certain knowledge of the "true state of nature." What are the consequences of an infinite amount of misinformation? If it is not perceived to be misinformation, then presumably decisionmakers will reach false conclusions about the "state of nature," and so increase the probability of bad decisions. This would seem to imply that more misinformation will increase the expected value of regret (cost of uncertainty), an intuitively appealing result. But what can be said about the cost of risk? If more misinformation is informative in the sense that it reduces the variance of the decisionmaker's subjective beliefs, does it follow that the cost of risk has been reduced?

On the other hand, if misinformation is perceived as such, then a common reaction is simply to ignore it. In this case, it has no impact on the twin costs of uncertainty and risk, and presumably no value. A more sophisticated response to perceived misinformation would be to draw inferences about the "true state of nature" from the nature of the misinformation being peddled. While such a reaction has similarities to the correction for known or suspected bias in statistical sampling theory, it is probably quite rare in practice.

For these reasons, I am no longer confident that all essential aspects of the information generating process can be summarized by the likelihood function. As a corollary, some of the procedures that I suggested might be used to simplify the valuation of information from social science research no longer seem so appealing. For instance, consider the claim that "If we can estimate the cost of uncertainty, and the cost of risk, however crudely, then immediately we have an upper bound on the possible value of the research being evaluated" (Lindner 1987, 101). This claim rests on the already discarded premise that any information will reduce the twin costs of uncertainty and risk, as does the proposition that "In practice the expected value of imperfect information will be some proportion of the potential expected value of perfect information" (Lindner 1987, 101).

A related suggestion was that the process of valuing information from social science research could be simplified by partitioning it into two steps: (1) guesstimate the components of the expected value of perfect information (that is, the cost of uncertainty, and the cost of risk); and (2) guesstimate the likely proportionate reduction in expected value of perfect information achievable by the particular type of research being evaluated.

This suggestion now seems courageous rather than farsighted. First, consider quantification of the actual reduction in the costs of uncertainty and/or risk. As previously noted, this task can be computationally difficult, but that is only the start of the problems. Recall that despite some exceptions, most social science research produces information with public good characteristics, so the aggregate value of each "piece" of information is the sum of its value to each decisionmaker who actually makes use of it. In theory, this means that not only
do estimates need to be made of the total number of “adopters” (that is, information users), but the distribution of the amount of individual benefits across this population also needs to be estimated. Unless the population is particularly homogeneous with regard to all relevant factors, it will not suffice to make estimates for a typical case, and treat the estimate as an average applicable to the whole population. Consequently, I no longer believe that “Guidelines for obtaining crude bounded ‘guesstimates’ can be derived from the basic principles of Bayesian decision theory, and/or from established findings in the literature.”

The second proposed step always seemed the more difficult of the two, but I took comfort from the presumed bounded value of the expected value of perfect information. Given the above arguments challenging this premise, the feasibility of the second step depends even more critically on being able to summarize essential aspects of the information quality in the likelihood function. The conceptual, let alone practical problems of doing so were discussed earlier.

Needless to say, the results of individual empirical studies of information valuation, even if most competently carried out, no longer seem to me to provide general insights into the expected value of perfect information for specific types of decision. Nor are they likely to indicate possible proportionate reductions in expected value of perfect information for some types of information. Consequently, the estimates summarized in Table 7.1 are probably of only passing interest. Maybe that is the reason for the small number of studies of this type being carried out.

Likewise, the assumption that decisionmakers make use of any and all information about which they become aware has already been queried. As the potential value of information will be realized only if decisionmakers are both aware of the information and make use of it to direct their decisions, the extent to which information is acquired, let alone used, is an empirical issue that needs to be investigated.

Conclusions

To conclude on a positive note, there are a number of themes in the 1987 paper to which I still subscribe. Some deserve more space than I gave them at the time. The importance of creativity as an essential determinant of scientific productivity in all areas of research, including social science research, is one example.

With regard to the benefits of social science research, the premise that information is the typical product of social science research, and that this information has value primarily because of its potential to improve decisionmaking of one form or another, is neither contentious nor remarkable. Nor is the observation\(^3\) that the key economically significant attributes of information are

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applicability, content, certainty, and relevance. To this list could be added reliability (or informativeness) and veracity (or falsity), as well as whether the information has the attributes of a public or private good, or even elements of both.

The evaluation of basic research is one of the great challenges to which no satisfactory solutions have yet been found. This is as true of social science research as it is of physical and biological scientific research. Since the output of basic research is knowledge that becomes an input into further creative activity, and the output of most social science research is information that becomes an input into decisions of one form or another, it may be that solving the challenge of either type of research will also provide the basis for valuing the other.

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8 Trade Policy and Economic Development: How We Learn

ANNE O. KRUEGER

Improvements in living standards, increased life expectancy, and economic growth in developing countries rank among the most important economic success stories since World War II. While progress has been far from uniform, there are grounds for optimism that future economic growth prospects will be even better. One factor accounting for that success is that economic policies much more conducive to economic growth have become better understood and more widely adopted than in the 1950s and 1960s. This has at least partly resulted from the interaction of improved research and experience with economic development policy. Ideas about trade policy and economic development have changed radically since the early 1960s. While it has long been recognized that trade policy is central to the overall design of policies for economic development, in the early days there was a broad consensus that trade policy for development should be based on import substitution. This meant that domestic production of import-competing goods should be initiated or expanded to satisfy the domestic market through whatever incentives were necessary, up to and including import prohibitions. The hypothesis was that import substitution in manufactures would be synonymous with industrialization, which in turn was seen as the key to development.

By contrast, it is now widely accepted that growth prospects for developing countries are enhanced through an outer-oriented trade regime and fairly uniform incentives, primarily through the exchange rate, for production across exported and import-competing goods.¹ It is generally believed that, at a min-

¹ John Williamson (1994, 26–28) summarized the set of policy prescriptions he believed most policymakers and academics concerned with development subscribed to. An outer-oriented trade policy is prominent on his list. He dubbed this set of views “the Washington consensus.”
maximum, import substitution has outlived its usefulness (if it was ever a valid hypothesis) and that liberalizing trade and payments is an essential ingredient in the optimal policy mix for both industrialization and economic development.

While both academic researchers and policymakers still disagree over particular aspects of trade policy, the current consensus represents a distinct advance in terms of both knowledge and the prospects the new consensus offers for rapid economic growth. Moreover, there is no question of going back to the early “import substitution” paradigm for industrialization and growth. Several interesting questions about this change in thought and policy arise: How could the economics profession, for which the principle of comparative advantage was and remains a key tenet, have embraced such protectionist policies? How did economics research contribute to this sea change in thinking, policy prescriptions, and politicians’ acceptance of the need for policy reform? And what sorts of economics research best informed the policy process? In a nutshell, what did economists and economics research contribute to the process? The issues are important because the question of what types of research inform good policy is important. In attempting to answer these questions, I first sketch the initial approach to trade policy in development research and thought in the 1940s and 1950s. Next, I trace the evolution of thought, research, and experience with respect to trade and development over the next several decades, which led to the “conventional wisdom” of the 1990s. I then consider the role of research and the sorts of research that proved most fruitful in guiding policy and changing the consensus.

Before proceeding, two caveats are necessary. First, it is very difficult to disentangle views of the proper role for trade policy in development from views about the appropriate role for the state. Partly as a legacy of the Great Depression and partly because of the belief that the Soviet Union had succeeded in its developmental and industrial aspiration through central planning and the perceived success of wartime controls, there was widespread agreement in both developed and developing countries that states should play a major role in economic activity, not only in affecting aggregate demand, but also in regulating private markets and augmenting or supplanting them with state-owned enterprise production of manufactured and other goods. Second, to focus on research that influenced thinking about economic policy is not to denigrate research that does not appear to have had immediate policy relevance, because basic research often informs more applied research. In addition, some research that provides little of lasting value demonstrates the infeasibility of certain policy paths or the futility of further explorations.

Nonetheless, ex post it is clear that some lines of research encouraged policymakers to remove high walls of protection, while other research lines were irrelevant or served largely to reinforce prejudices and perpetuate the old wisdom.
Evolution of Theory, Understanding, and Policy

The Early Years

As developing countries gained independence from their colonial rulers, their new leaders had a political mandate to achieve higher living standards and rapid economic growth. Today it is difficult to recall that the world economy was neatly split into industrialized and underdeveloped countries, or the "first world" and "third world." Underdeveloped countries had markedly lower average educational attainments (including high illiteracy rates and a large population segment with no schooling), poor health conditions, and very little infrastructure. They were also generally heavily specialized in the production and export of primary commodities and imported most of their manufactured goods.

The new field of development economics was thought to cover underdevelopment because conventional economics did not apply (Hirschman 1982). Development studies focused on how the developing countries should shape policies for accelerating growth and raising living standards.

Accepted Stylized "Facts" and Premises

Development economists based their early trade and development theories and policy prescriptions on some widely accepted "stylized facts" and premises about underdeveloped countries. These tended to be a mixture of tourist impressions, half-truths, and misapplied policy inferences. In fact, it can be argued that improved understanding of trade and development came about in large part through research that effectively demonstrated the falsity of these initial premises:

Premise 1: Low living standards in developing countries were the result of dependence on primary commodity production and export. This premise was based on the observation, certainly true at the time, that the production structures of developing economies were heavily oriented toward primary commodity production. Dependence on foreign trade was believed to be extreme, as there was virtually no production capacity for manufactured goods outside a few light mass-consumed commodities. Many observers therefore attributed low living standards in developing countries to their reliance on the production of primary commodities.

Premise 2: If developing countries adopted policies of free trade, their comparative advantage would forever lie in primary commodity production. It

2. Latin American nations and a few other countries (including China, Thailand, and Turkey), then deemed "underdeveloped," were not formally colonies prior to World War II. However, it was widely believed that they had been "economically dependent." The leaders and elite in most poor countries shared the perception that their economies were different from those of industrialized countries and like those of other developing countries. The G-77 (77 countries), or nonaligned nations, were all developing countries whose leaders perceived themselves to be in a similar economic situation with similar goals of rapid growth and improved living standards.
followed that industrialization and hence development would not take place if free-trade policies were adopted.

Premise 3: Because both the global income and price elasticities of demand for primary commodities were low, export earnings would grow only slowly, if at all. This premise was termed "export pessimism."3

Premise 4: The labor force in developing countries, predominantly engaged in agricultural activities, had a product of labor that was "negligible, zero, or even negative" (Lewis 1954, 141). The stylized fact that there was surplus labor, or disguised unemployment, in less developed countries (LDCs) was widely accepted.4 Many analytical formulations explicitly or implicitly assumed that labor was a free good while capital was the scarce factor of production.

Premise 5: Capital accumulation was crucial for growth, and in early stages of development it could occur only with the importation of capital goods. As the demand for capital goods imports and imports of other products used in the production process was expected to grow rapidly while foreign exchange earnings would not, it appeared that growth could follow only if domestic production of import-competing goods could expand rapidly.

Premise 6: Developing countries experienced little response to price incentives. According to this premise, peasants were "traditional" in their behavior, and there were "structural" problems within the economy.5

Based on these stylized facts and premises, it was a straightforward step to believe that the process of development was that of industrialization, essentially meaning the accumulation of capital for investment in the manufacturing industry and related infrastructure. Moreover, as most manufactured goods were imported, it seemed to follow logically, as stated by Chenery (1958, 463) among many others, that "Industrialization consists primarily in the substitution of domestic production of manufactured goods for imports."

Initial Policies

Policy prescriptions were derived from these propositions and stylized facts. Many observers thought that industrialization was necessary for development

3. Another widely held view, closely related to export pessimism, was the proposition that the terms of trade had inexorably deteriorated against primary commodities and would continue to do so. Investigation of this proposition tended to demonstrate that the deterioration had been much less than was believed. Spraos (1980) provided a classic review of the evidence.

4. A modern interpretation would be that there are many people in developing countries with very low marginal products of labor. While they are too poor to remain unemployed, the process of development entails equipping people with the capabilities (partly through education) and opportunities to increase their productivity.

5. This gave rise to a great deal of literature based on "structuralism." According to some, it was the absence of responsiveness to price that made developing countries "different." Structuralism was also used as an argument that inflation was necessary to achieve growth. See Chenery (1975) for a fuller description.
and that free trade would leave underdeveloped countries specialized in primary commodity production. Hence it followed that developing countries needed to invest in new manufacturing industries whose output would substitute for imports. Further, it was widely believed that new industries in poor countries could not possibly compete with their established counterparts in the developed world. Thus industries would have to be protected during their initial phases, and import-substitution policies became the hallmark of development strategies and, therefore, trade policy.\(^6\)

The case for import substitution was based both on the premises outlined above and also on a received doctrine: the infant–industry argument for domestic protection. Many economists had accepted the argument that dynamic considerations and externalities might imply that an industry, although economic, would not be established by private agents as a legitimate exception to the case for free trade.\(^7\) If a low-cost producer or producers were already in operation abroad, a potential entrant in a developing country would be faced with an initial period of high costs, but could in the longer run compete. However, in the presence of dynamic externalities (presumably inside the industry), it was believed that no individual producer would find it profitable to start production. In these circumstances, the infant–industry argument could justify temporary intervention to make entry into the new industry privately profitable provided that, over the longer term, its costs would decline below the imported cost by enough to yield an economic return on the intervening loss. The transitional distortions could be viewed as an investment.

Although the infant–industry argument was, in a first-best world, an argument for a production subsidy, which would presumably equal the unit value of the externality and might apply to production for exports as well as for the domestic market, it was combined with the appeal for import substitution\(^8\) to justify protection of newly established manufacturing industries in developing countries.

However, assuming that (1) industrialization would have to take place through substituting for imports, (2) infant industries required initial intervention,  

\(^6\) Many important subthemes are not elaborated on here, as they are not essential to the main argument. There were many who believed that the situation of developing countries was structural and that marginal changes would not matter. It was then concluded that a "big push" was needed, with many new investments simultaneously generating additional demand and then becoming profitable. Nurkse's (1958) "balanced growth" prescription reflected the same viewpoint.

\(^7\) See Baldwin's (1969) classic analysis of the argument, which not only sets up the conditions under which there might be an infant industry, but also carefully and critically scrutinizes the various circumstances in which those conditions might hold. Baldwin's article was an important contribution to a better understanding of the empirical relevance of the theory.

\(^8\) It was also believed that there was a revenue constraint, making the first-best production subsidy infeasible. More recent analyses would also point to the greater potential for corruption inherent in production subsidies as yet another reason why protection might be preferable.
and (3) export earnings were unlikely to increase, the stage was set for trade and industrialization policies.

The premises underlying import-substitution policies were so widely accepted that developing country exceptions were even incorporated into the General Agreement on Tariffs and Trade (GATT) articles. Article XVIII explicitly protected developing countries from the obligations of industrialized countries and permitted them to adopt tariffs and quantitative restrictions. They also were entitled to "special and differential treatment" in other regards under GATT. That GATT, the upholder of an open international trading system, would accept an exception for developing countries shows how deeply entrenched were the views supporting import substitution. It is arguable that this exception not only legitimized developing countries' inner-oriented trade policies, but also removed pressures to adopt trade and payments regimes more conducive to economic growth.9

Resulting Evolution of Policies

In one way or another, country after country acted to restrict imports once domestic production became feasible. In Brazil, a "Law of Similars" provided that firms importing goods that were similar to those available domestically would lose their government privileges, which included access to credit and tax treatment, eligibility to bid on government contracts, and a variety of other valuable rights. India licensed importers, and in the event of domestic production, would-be importers were required to obtain letters from any supplier that government officials thought might be capable of producing the good attesting to the supplier's inability to meet the specifications. In Turkey, goods were removed from the list of items for which import licenses could be granted once domestic production capacity was available. Most developing countries used similar provisions, or very high tariffs, to encourage import substitution.10

Some countries and industries used trade regimes as the key policy instruments to provide incentives for import-substituting investment and production by private firms. In other circumstances, state-owned manufacturing enterprises were established with direct investments by the state. None of these trade policies provided any means of identifying where dynamic externalities were largest, nor was there any provision to reduce protection after an initial period. Indeed, protection was virtually automatic for any new import-substitution industry.

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9. See Dam (1970, Chapter 14) for a full discussion.
10. In Argentina, an effort was made to liberalize the trade regime by lowering tariffs in the late 1970s. To the surprise of officials, there was no apparent effect of the first round of tariff cuts. Subsequent investigation revealed that the tariffs in question had been between 500 and 1,000 percent, and that they had been above the rates at which domestic producers could compete.
As countries embarked on ambitious development plans, inflation rates rose to levels significantly higher than those in industrial countries (although far below inflation rates prevailing in some developing countries today). Demand for foreign exchange rose rapidly in response to the development plans, rising incomes, and domestic inflation. Policymakers in most developing countries nonetheless maintained fixed nominal exchange rates. In part, this reflected the perception that there was little response to prices and that maintaining the nominal exchange rate taxed agriculture while subsidizing capital goods imports. In part, exchange rates were held fixed because it was believed that so doing made imports of capital goods cheaper and increased investment in manufacturing. The net result was, of course, real appreciation of the exchange rate, which further intensified ex ante payments imbalances, reduced foreign exchange availability, and induced greater restrictiveness in import licensing.

The 1950s and 1960s were a time of unprecedented economic growth for the industrial countries and for world trade. Buoyed by international markets and increased investment, per capita incomes rose markedly relative to historical levels in most developing countries, although most remained well below those in industrial countries. Even the growth of industry itself was fairly rapid, as the "easy" import-substitution opportunities were usually taken up first.\(^\text{11}\)

However, with real exchange rate appreciation and the pull of resources into newly profitable, import-competing industries, the growth of foreign exchange earnings inevitably slowed. The developing countries' share of world agricultural commodity exports declined from 44 percent in 1955 to only 31 percent in 1970.\(^\text{12}\)

As demand for foreign exchange grew and supply slowed, foreign-exchange difficulties were inevitable: the "export-pessimism" premise had become self-fulfilling. The initial response by most policymakers was to impose rationing of scarce foreign exchange on imports (and require the surrender of foreign exchange from exports), and the resulting system had little to do with encouraging infant industries.

Controls over foreign trade generally became more restrictive and complex over the next two decades, in response to growing foreign exchange short-

\(^{11}\) See Prebisch (1984) for the argument. It can be argued that, with uniform incentives, import substitution would have taken place first in those industries with least comparative disadvantage. In fact, the use of import licensing and prohibitions meant that rates were not uniform even across import-competing activities. In addition, monopoly power in the domestic market was conferred to domestic producers, so that profitability hinged more on the price elasticity of the demand curve than on producers' abilities to reduce costs and compete with imports.

\(^{12}\) Agricultural protection in Japan, Europe, and the United States may have contributed somewhat to this result. But in most developing countries, the demand for food was growing more rapidly than the supply, as producer prices were suppressed relative to the prices of industrial goods, and, thus, the supply (demand) curve for exports (imports) of agricultural commodities was shifting down (up) (see Krueger 1990, 95).
ages, the unfairness of the undifferentiated controls, and evasion of the regimes.\(^{13}\) Periodic balance-of-payments crises arose in reaction to the overvaluation of real exchange rates, increased indebtedness, and the failure of export earnings to grow.

Eventually, International Monetary Fund (IMF) “stabilization” programs had to be adopted, simplifying import regimes and altering nominal exchange rates, but usually to a new fixed exchange rate in the face of continuing inflation.\(^{14}\) Even IMF programs, however, seldom tried to change the underlying trade policies related to import substitution: The intent, rather, was to rationalize the trade regime and find ways to induce more foreign exchange earnings to finance the capital goods that would be imported to undertake additional import-substitution investments. Growth proceeded in “stop-go” fashion, as periods of foreign exchange crisis were followed by tighter monetary and fiscal policies, a consequent reduction in excess demand for imports, and an increase in foreign exchange earnings. When the trade regime was again relaxed, growth resumed and the demand for imports again mushroomed until the next crisis.\(^{15}\)

**Research Directions and Contributions**

Most research in the 1950s and 1960s was based on the premises outlined above, and supported the basic thrusts of policy. Some focused on the possible existence of externalities and the need for balanced growth, as it was assumed that expansion of any one industry alone would not be feasible because of the limited size of the market.\(^{16}\) This prescription, of course, was based on the premise that manufactured exports could not be developed. Another line of supportive research focused on planning models, concentrating in large part on interindustry flows and linkages.\(^{17}\) Empirical research on patterns of development began, focusing on the structure of economies and their growth performance. For more than a decade, the growing disparity between theory and practice was all but ignored.

Research also provided a rationale for protection of new industries and import substitution, demonstrating that domestic distortions could warrant trade

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13. For a description, see Bhagwati (1978).

14. See Cline and Weintraub (1981) for analyses of some of these episodes.

15. See Díaz-Alejandro (1976) for an analysis of the “stop-go” cycle in Colombia.

16. For a modern presentation of the “big push” needed for balanced growth, see Murphy, Shleifer, and Vishny (1989). The notion of balanced growth and “big push” in the 1940s and 1950s was associated with such analysts as Rosenstein-Rodan (1943) and Nurkse (1958), among others.

17. See Chenery and Clark (1959) for an exposition. Economists in India probably carried planning models the furthest into practice. The Indian Second Five-Year Plan was explicitly based on the Mahalanobis (1955) model, and contained estimates of output levels for the subsequent five years that were used as a basis for granting investment licenses. No licenses were issued once the increased capacity had been allocated. See Bhagwati and Desai (1970) for an account.
intervention\textsuperscript{18} in a number of situations. Hagen (1958), for example, set up a model assuming that urban wages exceeded rural wages exogenously, and demonstrated that a tariff could improve welfare by inducing resources into the (artificially) higher-cost urban industries.

Work also continued on structuralist models, as a number of authors sought reasons why developing countries’ economic structures were different and why, therefore, the usual economic analysis would not apply.\textsuperscript{19} Chenery and Bruno (1962), Chenery and Strout (1966), and Chenery and many other coauthors developed the “two-gap” model, using the stylized fact that foreign exchange was scarce in developing countries. In this model, export earnings were exogenous and grew more slowly than the demand for foreign exchange. Investment was limited by the more binding of two linear constraints: the available savings and the available foreign exchange. There were thus two gaps—between savings and investment, and between demand for and supply of foreign exchange. Growth was constrained either by savings or by foreign exchange availability, and the model demonstrated the high potential productivity of foreign aid (in providing foreign exchange), enabling otherwise redundant domestic savings to be used in capital formation. The model, reflecting the views of the day, had little role for the price mechanism.\textsuperscript{20}

One example of an analytical effort to clarify circumstances under which one of the stylized facts could be realized was Bhagwati’s (1958) and Johnson’s (1967) exposition of the possibility of open “immiserizing growth,” whereby a country might increase its output, only to find the price of exports falling so much that the country was worse off. As Bhagwati showed, the conditions under which that might happen were fairly extreme.

An important development was the theory of shadow pricing, an offshoot of programming and planning models. It was initially used to demonstrate how reliance on market prices might yield an inappropriate resource allocation. Quickly, however, analysts pointed to the distortions between domestic prices of import-competing and exportable goods resulting from trade regimes. There is little doubt that cost–benefit techniques improved project selection and governmental decisionmaking with, among other things, the insistence on use of border prices. The publication of the Little and Mirrlees (1969) manual served as a milestone that removed any question about the need to use border prices in project evaluation.

\textsuperscript{18} There was a huge literature on this subject. See Bhagwati (1971) for a synthesis of many of the papers.

\textsuperscript{19} See Bliss (1989, 1194) for a modern statement of the proposition that if demand and supply are sufficiently inelastic, prices do not matter.

\textsuperscript{20} See McKinnon (1966), who provided the first demonstration of this important proposition at the time.
In a related and important development, the theory of effective protection was developed by Balassa (1965), Johnson (1965a), Corden (1966), and others, providing a framework for analyzing the protection accorded to industries engaged in light processing and much higher value-added activities on a comparable basis. The notion of domestic resource costs (Bruno 1965; Krueger 1966), showing the uneven allocation of resources to earning and saving a unit of foreign exchange across activities, was developed to respond to the argument that market prices failed to reflect opportunity cost. This research provided a tool with which economists could measure the wide disparities in protection accorded to different import-competing industries.

Recognizing that these estimates were based in part on partial equilibrium analysis, several researchers began to develop techniques for computing general equilibrium results. Based on newly developed solution algorithms, techniques were devised for models that endogenized prices and moved away from the linear models used earlier for analysis.21

By the late 1960s and 1970s, significant contributions undermined some of the premises on which import-substitution strategies were based. At an analytical level, one line of research focused on whether the stylized “facts” of “market failure” warranted the imposition of trade restrictions. Bhagwati and Ramaswami (1963), Johnson (1965b), Bhagwati (1969), and others demonstrated that a trade instrument (tariff or quota) was usually not a first-best, or even second-best, instrument for achieving the objectives for which protection had been granted. The equivalence of tariffs and quotas, an old result in international economics, was revised and refined, as quotas became more frequently used.22

Researchers also began analyzing other aspects of the ways in which protection actually worked, focusing on rent-seeking (Krueger 1974) as a byproduct of protection—and indeed, as a user of resources as lobbyists sought protection (Bhagwati and Srinivasan 1980) and as resources were used to obtain valuable import licenses, thereby incurring deadweight costs. This showed that protection was more costly than was indicated by simple areas under triangles. Moreover, it became clear that both those who benefited from protection policies and those who administered them would be in the forefront of the opposition to policy changes. At the same time, other researchers worked on the theory of over invoicing and under invoicing (Bhagwati 1974) and smuggling (Sheikh 1974; Pitt 1981), again focusing on flaws in import protection trade regimes.

As trade regimes became more chaotic, empirical work began to document these problems, bolstered by the development of the measurement tools embodied in the concepts of effective rates of protection and domestic resource

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21. For an exposition of the development of these models into the 1970s, see Dervis, de Melo, and Robinson (1982).
22. See the survey in Bhagwati (1969).
costs. Researchers discovered that Pakistan actually experienced negative value-added from import substitution policies in some circumstances, suggesting that it would have been cheaper to pay workers to stay home and to import the final product.23

The Organisation for Economic Co-operation and Development (OECD) sponsored a series of country studies on industrialization led by Little, Sci-tovsky, and Scott (1970). The synthesis of the results showed how high and indiscriminate protection levels were and demonstrated the extent to which import substitution had failed to achieve many of its objectives. A later series of country studies undertaken under the auspices of the National Bureau of Economic Research, synthesized in works by Bhagwati (1978) and Krueger (1978), provided further systematic empirical evidence of the economic wastefulness and irrationality of the inner-oriented trade regimes.

East Asian Experience

At the same time, as evidence of the high costs of import-substitution regimes accumulated, another important development occurred. Starting first in Taiwan, several East Asian economies began growing rapidly under policies diametrically opposite to those prevalent under import substitution. Interestingly, the Taiwanese government seems to have listened carefully to the views of S. C. Tsiang,24 a Cornell University professor specializing in international economics. Following the precepts of comparative advantage, Tsiang advocated growth through industrialization, but with industrialization taking place through increased capacity for exports, as well as for the domestic market. Taiwan’s transformation from a high-inflation, inner-oriented, aid-dependent economy to a major exporting economy is well known.

Korea, whose initial conditions appeared even less conducive to growth than those of Taiwan, followed the same pattern. In the late 1950s, Korea’s exports had averaged only 3 percent of gross domestic product (GDP) and were growing slowly, if at all, while imports represented 13 percent of GDP. The current account deficit was financed largely by foreign aid, and the domestic savings rate was virtually zero. In the early 1960s, Korea initiated major policy reforms that greatly increased the return to exporters. There were fairly uniform incentives to all exporters and assurances that the real exchange rate would not appreciate to their detriment. Reforms also reduced the protection to import-competing producers and permitted exporters duty-free importation of needed intermediate goods and raw materials.

23. See Corden (1971, 51) for a summary of that literature.
24. For an account of Taiwan’s turnaround, see Tsiang (1985).
Korean economic performance was transformed, with double-digit growth rates and improved living standards. Through policies designed to encourage exports, Hong Kong and Singapore also became part of the East Asian “miracle,” with growth rates exceeding all previous projections.

It was not until the 1980s, however, that the importance of the differences became unarguable. After the second oil price increase of 1979, the worldwide recession of 1980–82, and the accompanying “debt crisis,” the East Asian newly industrializing countries (NICs) rapidly resumed growth, whereas other heavily indebted countries were unable to service their debts and were hard hit by events in the international economy. Researchers found that debt/GDP ratios were not significantly different between the two groups of countries. However, debt/export ratios were significantly different: The East Asian countries were able to maintain debt servicing and resume growth because of the greater flexibility of their economies. 25 Even prior to the debt crisis, the rates of growth of inner-oriented developing countries had not increased despite substantial increases in their savings rates. 26

This is not the place to debate the factors contributing to the success of the East Asian “tigers.” Whether or not government intervention in “picking the winners”—that is, selectivity in incentives confronting different industries—was a key component of the growth strategy, 27 all recognize that the reversal from an import-substitution strategy, the opening up of the economy, and imposition of relatively uniform incentives across the board were necessary. It is ironic that the East Asian experience stimulated attempts to identify the dynamic factors in exporting that are absent from production for the domestic market, thus creating a complete turnaround. In the 1950s and 1960s, economists rejected the neoclassical argument for an open trade regime on the grounds that it was static and ignored dynamic considerations, yet today most agree that the benefits of an open trade regime are largely dynamic in nature and go well beyond trade gains under static models. As with the infant-industry argument, however, there is a question as to how to identify and measure these dynamic gains.

25. See Sachs (1985) for an early development of the argument.
26. The World Bank (1983) documented that this phenomenon of a greatly increased average savings rate with no increase in the growth rate and, therefore, a presumed relatively sharp increase in the incremental capital output ratio, affected most developing countries.
27. It can be argued that this is a difference between those who see the East Asian trade policies as “free trade” and those who see them as intervention, but of a different type, from that under import substitution. The critical difference is probably between those who would stress uniformity of incentives for earning or saving foreign exchange (and, therefore, would argue that the East Asian NICs were arbitrarily close to a free-trade regime), and those who believe the “dynamic externalities” earlier associated with infant-industry protection really call for the “right kind” of intervention and argue that the trade strategy was actually one of “export substitution.”
How Did Economists and Research Go Wrong?

The "Washington consensus" is different from the policy consensus that led to the adoption of import-substitution policies in the 1950s and 1960s. While there no doubt will be refinements in that consensus with further experience and research, it is highly unlikely that the ideas of the 1950s and 1960s will be revived.

One can raise three questions about the change in viewpoints:

1. How could so many in the economics profession, whose consensus on the principle of comparative advantage was at least as great as that on any other policy issue, endorse a highly protectionist policy stance?
2. What factors contributed to changing the entrenched views of the 1950s and 1960s?
3. What types of research were most (and least) productive in bringing about better understanding of the role of trade and trade policy in development?

The first issue is how the principle of comparative advantage could have been so blithely abandoned. In hindsight, it is almost incredible that such a high fraction of economists could have deviated so far from the basic principles of international trade. What led them to do so? Can any lessons be drawn to avoid (or shorten the duration of) similar mistakes in other applied fields when new policy problems arise?

But recall the stylized facts that were widely accepted: People responded to incentives, export earnings were predetermined and slowly growing at best, industrialization was necessary for development, supply response was lacking, and so on. These stylized facts, which were at best simplistic and in most instances simply wrong, permitted economists to conclude that developing economies were "different."

However, it took theory to support these conclusions. Here, one can distinguish several failures: (1) misapplication of good theory, (2) the "theory of negative results," which could be used to provide a rationale for virtually any trade intervention, and (3) good theory harnessed to erroneous stylized facts.

Misapplication of Good Theory

Misapplication of good theory was significant. The identification of comparative advantage with the two-factor, two-good model, and the assumption that free trade implied that developing countries would forever specialize in primary commodities, was an important misapplication. One of the puzzling aspects of

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28. Another example of misapplication of good theory was the early defense, such as that of Hagen (1958), of protection because of a domestic distortion. But it took the development of the theory of domestic distortions to correct that, as is discussed below.
the evolution of thinking about policy is the degree to which proponents of open trade regimes failed to refute the allegation that free trade would forever leave developing countries specialized in production of agricultural commodities.\textsuperscript{29}

It was not until the 1970s (Jones 1971b; Krueger 1977) that models—motivated in part by the East Asian experience—were developed in which three factors of production (land, labor, and capital) were allocated among sectors, each of which could produce many commodities. As the three-factor models demonstrated, comparative advantage lies within manufacturing and within agriculture, and not between them. Thus poor, unskilled, labor-abundant countries have a comparative advantage in labor-intensive agricultural \textit{and} unskilled labor-intensive manufactured commodities, while countries with a much higher land/labor ratio have a comparative advantage in more land-using agricultural commodities, and their comparative advantage in manufacturing lies more in goods with higher capital/unskilled labor ratios. In these models, the overall trade balance in manufactures is a function of the size of the manufacturing sector, itself a function of past capital accumulation and the land/labor ratio.

A second serious misapplication of good theory arose because of the non-operational nature of the theory itself and the failure to identify circumstances under which policy implementation might be incentive compatible and could potentially increase welfare. A key culprit in this case was the interpretation of the infant-industry argument, which was widely touted as a basis for import substitution and generally recognized as a legitimate reason to depart from free trade.

One can hardly argue with the proposition that the presence of a positive externality provides a basis for intervention; if the externality is dynamic and temporary, then temporary intervention, such as infant-industry protection, can be called for.

The problem with the argument, as a basis for policy, is that it fails to provide any guidance as to how to distinguish between an infant that will grow up and a would-be producer seeking protection because it is privately profitable. It is not even clear how one could begin, empirically, to identify the domain of the externality. Moreover, even if there were a producer or producers whose increased production would generate dynamic externalities, it does not follow that any level of protection is warranted. And there is nothing in the infant-industry argument per se to provide guidance for quantifying or estimating the likely magnitude of the externality.

Indiscriminate protection in developing countries was defended on infant-industry grounds with arguments about capital market failure, labor market

\textsuperscript{29} Some of Johnson's (1958) research on trade and growth went some way toward refuting this proposition, but still in a two-by-two framework. Moreover, Johnson's work implied that labor-abundant countries would, while accumulating capital, undergo "ultra-anti-trade biased" growth, which seemed to support import substitution.
failure (as the costs of training, presumably, would be borne by first entrants into industries and then not recouped as others hired workers away), costs of investments in technology, and uncertainty. Baldwin’s (1969) seminal article demonstrated that, even when the presumed imperfection existed, it was unlikely that infant-industry protection would help correct it. As Baldwin cogently argued, later entrants to an industry might speed up their investments if protection made domestic production more profitable, and the first entrant might even be worse off. It was only after critical examination of these circumstances that defenders of the infant-industry case for import substitution became less vehement.

The infant-industry argument also is an excellent example of a theory that is nonoperational because criteria for bureaucrats to identify cases have not been put forward. Aside from the unpredictability and immeasurability of the future time path of costs in new factories and the moral hazard associated with asking individual entrepreneurs to indicate how much protection they need, I know of nothing in the literature specifying how the policymaker might instruct a bureaucrat to identify (much less, measure) a dynamic externality if it were present, how an incentive-compatible mechanism might be devised for improving welfare, how the bureaucrat might measure the height of warranted protection, or how policymakers might credibly commit to temporary protection. Even after the fact, it is not entirely clear how one might identify an industry as a successful infant: Simply because a firm became profitable and exported does not prove that there was either an externality or a dynamic process at work.30

Negative Results

Much of the theorizing was concerned with “negative results.” That is, analysts sought to find reasons why, for example, an exception to free trade should be made. Once the principle of comparative advantage was laid down as a basis for policy, there was little left for theorists supporting an open trading system to prove, so the challenge was to find conditions under which the free-trade precept did not hold. As theory, these findings were significant, but for policy they were unhelpful and probably served to perpetuate inappropriate policies.

In most real-world circumstances, one strongly suspects that protection exists where theoretical exceptions do not justify it, and that moves to first-best policies would on average lower, and not raise, protection. Judged by that metric, research output relevant for policy would consist more of attempts to measure the costs of these excess levels of protection. Whereas theory suggests criteria

30. The same is true of the optimum tariff argument. In the presence of many goods with varying degrees of monopoly power, the formula becomes hopelessly complex. It is true that many tariff structures would lead to lower, rather than higher, welfare in the presence of monopoly power in trade. Yet, in practice, many policymakers have been misled into thinking that they could defend very high tariffs, sometimes even on goods that their countries import in small quantities, on optimum tariff grounds.
for departures from free trade that normally would result in different levels of protection for different industries, a widely used prescription for policymakers is that, if there is to be protection, a uniform tariff is usually preferable to any alternative structure. This proposition rests on several considerations:

1. Only a uniform tariff can generate a uniform rate of effective protection in the import-competing sectors and, if different goods are subject to different tariff rates, the resulting differences in effective rates of protection will lead to resource misallocation even within the import-competing industries and have no relation to underlying "dynamic" or market-failure considerations.

2. A uniform tariff simplifies customs administration, making tariff evasion or bribery of customs officials more difficult.

3. A uniform tariff greatly reduces the opportunities for resource losses in rent-seeking and lobbying.

4. Given international prices, international value-added is more likely to be maximized under a uniform tariff structure than under a variable one.

None of these considerations implies that a uniform tariff is optimal. And, indeed, it is straightforward to develop models in which a uniform tariff is nonoptimal, especially in the presence of income-distribution considerations. In theory, costs of protection can be minimized by imposing higher tariffs or taxes on goods for which supply and demand functions are relatively more price inelastic. These arguments are all couched in terms of demonstrating the falsity of the proposition that a uniform tariff is preferable and that a departure from uniformity can potentially improve welfare. But those arguments do not provide a criterion for which departures from uniformity might improve welfare, because a model considering, for example, income-distribution, cannot simultaneously address issues of corruption and administration. And the fact that income-distribution considerations can warrant a nonuniform tariff structure does not prove that any nonuniform tariff structure is preferable to a uniform one. As such, a negative result gives little or no guide for policy. Nonetheless, it arms lobbyists and others with ammunition to discredit technocrats' efforts to maintain a less irrational structure of protection.

Most policies are implemented by government officials who cannot be expected to have advanced or even undergraduate degrees in economics. Many bureaucrats will lack the degree of sophistication needed to interpret research results. As pointed out by Johnson (1970, 101), "The fundamental problem is that, as with all second-best arguments, determination of the conditions under which a second-best policy actually leads to an improvement in social welfare requires detailed theoretical and empirical investigation by a first-best economist... it is therefore very unlikely that a second-best welfare optimum will result based on second-best arguments."
Good Theory Assuming Counterfactual Situations

The final abuse of theory was primarily a fault of inappropriate stylized facts. Nonetheless, in many instances, analysts assumed signs of variables that were questionable, modeled the situation neatly, and then drew policy conclusions that could hold only if the posited signs were valid. Yet their claims often went beyond the "if these facts . . . then" variety of conditional recommendations.

One widely cited paper illustrates the point. The paper represents good theory, but interprets it, for policy purposes, with dubious stylized facts. Anand and Joshi (1979) considered a world such as that envisaged by Hagen (1958) in which workers in the advanced sector receive a higher wage than workers in the rest of the economy, owing to union pressures or other (presumably unalterable) circumstances. They then asked whether maximizing international value-added for given employment of domestic resources is an appropriate criterion when income-distribution considerations cannot be separated from productive-efficiency considerations.

In their setup, the clear answer is no, because tradables are produced by the advanced (presumably unionized) sector, and hence maximizing international value will pull more resources into that sector at the cost of a deteriorating income distribution. Interestingly, they do not address the question of whether the advanced sector is labor or capital intensive. If, as is true for outer-oriented developing countries, the exportables are labor intensive relative to import-competing activity, removing protection to induce more workers to move to the advanced high-wage sector would presumably increase wages of those workers and also those in the rest of the economy: A more equal income distribution would be obtained at the expense of lower real wages for all. Without regard to factor intensity, however, Anand and Joshi (1979, 350) conclude that: "The motivation behind the theory of distortions has been to criticize and to guide trade and industrialisation policies. . . . Our analysis emphasises the need for caution. . . . Departures from technical efficiency may be called for as part of the rational response by governments to the limitations they face in carrying out desirable income distribution policies. . . ."31

Anand and Joshi assumed that moving toward economic efficiency in tradables requires paying higher wages because of a distortion. Yet, in fact, the evidence suggests that it has been the highly protected, import-competing in-

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31. Another example of the "negative results" research arises from early findings (see Jones 1971a; Bhagwati and Srinivasan 1973) that the resource pulls associated with raising an effective rate of protection did not necessarily accord with those associated with the rate of protection. These findings did not significantly affect research efforts, in part because the authors made clear the relatively extreme conditions necessary to generate the "perverse" resource pull, and partly because other researchers were able to demonstrate that there seemed to be few, if any, empirical counterparts to the perverse pull cases.
dustries that have been able to pay above-average wages; removing protection has led to rapid expansion of employment in labor-intensive industries. If the latter stylized fact is correct, and if income-distribution considerations are important, it would indicate that the policy implications of the Anand-Joshi analysis are the opposite of what they suggest—namely, that policymakers should encourage a shift of resources out of protected industries (presumably by removing protection) and into exportable industries.32

What Research Has Contributed to Improved Policies

Policies inconsistent with policymakers' growth objectives were cloaked in respectability in the 1950s and 1960s by theory and stylized facts of the type already described. So far, the properties of some theories that made them susceptible to misapplication or misuse have been discussed.

A second question is equally important, however. That is, how did the change in economists' policy prescriptions come about? What led to the recognition of the importance of an open economy after the conversion to advocacy of import substitution in the 1950s and 1960s? Much of the answer is implicit in the description of the evolution of developing countries' trade policies.

Three sets of research efforts were particularly useful in informing changes in policy:

- Research analyzing how import-substitution policies were actually working.
- The refinement and more appropriate interpretation of theory.
- Research demonstrating the feasibility of the alternative.

Challenging the Stylized "Facts" and Understanding How Import-Substitution Regimes Worked

Analyses of the evidence regarding the key stylized "facts" were in hindsight important steps in undermining the intellectual consensus. Demonstration that there were significant responses to incentives undermined the policy case for ignoring prices. Proof that the terms of trade had deteriorated very little, if at all, began to undermine export pessimism.

Empirical work on the ways in which import-substitution regimes functioned was crucial. Comparative analyses such as those of Little, Scitovsky, and Scott (1970), Bhagwati (1978), Krueger (1978, 1983), and Michaely, Papa-georgiou, and Choksi (1991) contributed to awareness that the effects of import-substitution policies were not idiosyncratic to individual countries. The comparative studies indicated the shortcomings of reliance on import substitution. Evidence that protection was not temporary, that protection levels were high

32. See Bardhan (1996), who assumed the "efficiency-equity" trade-off to be largely false.
and idiosyncratic, that there was great discrimination against exports, and that “foreign exchange shortage” was a function of policies and not an exogenously given datum were all important in challenging the protectionist trade policies still prevailing in most developing countries in the 1980s.

The workings of trade policies in any one country revealed ample grounds for criticism of inner-oriented trade policies—including the monopoly positions they conferred on domestic producers, the high costs of doing business, the rent-seeking, low quality of products, and so on. It was still possible, however, to conclude that policymakers in that particular country had been inept or had simply failed to implement policies appropriately. As evidence mounted across countries, however, the similarity in the evolution of regimes and their consequences was striking. It was increasingly difficult to dismiss evidence from a particular country as being sui generis or a failing of policy execution in that country.

But widely accepted measurement tools underpinned the analyses of individual country situations, either in comparative studies or individually. The empirical studies could not have had their impact without the development and use of these tools. With cost-benefit analysis techniques, it became increasingly difficult to justify some highly uneconomic projects. And as measurement of effective rates of protection was undertaken in country after country, the high level and erratic nature of protection became evident. Techniques for cost-benefit analysis and measurement of effective rates of protection provided analysts with tools to demonstrate the chaotic nature of import-substitution policies. In addition, even before the policy consensus changed, there is little doubt that some of the earlier extreme irrationalities of policy were curbed through use of these tools. It became difficult to defend the high average of, and wide variance in, effective rates of protection.

Early demonstrations of the great variation in effective rates of protection showed some of the problems with trade regimes and prevented at least a few of the worst excesses. Generally, recognition and reintroduction of the proposition that there is a response to incentives that cannot be overlooked in policy formulation, combined with the evidence on the erratic and arbitrary nature of incentives provided by trade regimes, forced policymakers to reexamine the premises on which import-substitution policies were based.

Yet another contribution of empirical research was to focus on the actual workings of policy implementation. Early on, policymakers had naïvely assumed that enunciating a desired outcome was sufficient to achieve it. This naïveté was dispelled, as theories regarding bureaucratic behavior, rent-seeking, smuggling, and overinvoicing and underinvoicing enabled observers to examine more critically the unanticipated side effects of alternative policy prescriptions.

Refinement and More Appropriate Interpretation of Theory

Some of the intellectual underpinning of import-substitution policies was provided by economists’ inappropriate interpretation of theory, or their failure to
take into account key institutional or behavioral variables when applying the theory. Analytical developments focusing on conditions under which these interpretations were valid, or examining the ways in which results had to be modified to consider these institutional and behavioral aspects, were clearly important in improving understanding.

The literature on optimal interventions in the presence of domestic distortions demonstrates that earlier interpretations of theory failed to examine relevant alternatives and that, in most circumstances, the presence of a distortion warranted a first-best policy intervention other than a tariff. For example, in Hagen’s (1958) employment-generating case for protection, this literature demonstrated clearly that a first-best intervention would be in the labor market, and that a tariff or quota could not achieve a first-best outcome.

Similarly, developments showing that the comparative advantage results were not the simple “specialize forever in primary products” precept helped policymakers to reevaluate trade strategies. Baldwin’s (1969) critical examination of the infant-industry argument encouraged policymakers to consider carefully the effectiveness of the policies they had adopted in achieving their desired goals.

Finally, economists developed theory in response to the functioning of import-substitution regimes. Here again, the theory of rent-seeking, as it pointed to the ways in which bureaucrats and others made protection very costly, was important. When it was recognized that bureaucrats, businessmen, and others would attempt to thwart policy initiatives not in their self-interest, that they had an interest in maintaining established systems, and that resources were expended in operating the system, it had to be acknowledged that changing the system would be politically difficult.

Understanding the incentives for underinvoicing and overinvoicing of exports and imports and for smuggling under exchange-control regimes worked in the same direction: Not only could these activities prove costly to the exchequer and drain resources, but the very recognition of their presence reminded policymakers of the limitations of their instruments.

Finally, good analyses demonstrating how individual import controls actually worked contributed to economists’ and policymakers’ understanding and made empirical work more effective. The further refinement of theory concerning tariff-quota equivalence has already been mentioned—and rent-seeking also comes to mind. But, in addition, individual mechanisms for encouraging import substitution each had their own, often idiosyncratic, incentive effects. A good example is Grossman’s (1981) classic analysis of domestic content regulations and their effects.

**Demonstrating the Viability of Alternative Trade Policies**

Research into the contrast between East Asian and other developing countries influenced thinking about trade policy. In a way, research on the East Asian
experience dealt a final blow to the earlier uncritical acceptance of the stylized facts. The East Asian experience demonstrated, as nothing else could have, the feasibility and viability of alternative trade policies: It was no longer possible to associate comparative advantage with reliance on primary commodity exports, and the East Asian experience showed that developing countries could develop rapidly while relying on integration with the international economy.

The experience of the East Asian exporters provided concrete evidence that a developing country could achieve industrialization without relying on domestic markets to absorb almost all additional output and demonstrated the fallacy of the earlier view that industrialization could take place only through import substitution. Also, the East Asian trade regimes offered significant opportunities for empirical research, and evidence mounted that properties formerly thought to be those of all developing countries were, in fact, properties resulting from inner-oriented trade and payments regimes.

It cannot be said that either research results or the contrast in economic performance alone led to the change in policies in other developing countries. Both research, especially that which brought the sharply contrasting experiences of the East Asian exporters and the import-substituting countries into focus, and experience contributed.

Whether one should regard the East Asian experience as entirely separate from economic theory, however, is an interesting question. Tsiang (1985) was himself an international economist, and it was in significant part his efforts that led the Taiwanese authorities to abandon inner-oriented policies and attempt to develop through exports. The theory of comparative advantage was, at least in that instance, a pillar on which policy was built. And, while a variety of factors contributed to the Korean adoption of outer-oriented trade policies after 1960, the favorable experience of Taiwan undoubtedly facilitated the willingness of decisionmakers to try the new approach.

The East Asian exporters put to rest the mistaken belief that developing countries relying on the international market would forever specialize in the

33. To be sure, there are still doubters. Some claim that South Korea and Taiwan were major recipients of foreign aid, which is said to account for much of their rapid growth (although the announcement that foreign aid would diminish was what triggered policy reform in Korea). The status of Hong Kong and Singapore as city-states is alleged by some to render their experience of little relevance. Even today, those resisting policy changes assert that conditions in the 1950s and 1960s were conducive to export expansion in ways in which the world market of the 1990s is not—despite the rapid expansion of exports from China and Southeast Asian countries.

34. Some have argued that the East Asian outer-oriented trade strategy might not have succeeded without an earlier stage of import substitution. In that view, East Asia moved away from import substitution at the right time, whereas other countries stayed with the strategy too long. See Ranis (1984) for one such argument.

35. For that matter, trade policy reform is still resisted in many countries, notably most of Sub-Saharan Africa.
production of primary commodities. They also showed that growth rates well above those realized even in the most rapidly growing import-substitution countries such as Brazil and Turkey could be realized.

**What Lessons Can Be Learned for Research in New Applied Fields?**

Perhaps the most obvious generalization from the various factors that have been discussed is as follows. Empirical research that tests for the presence and order of magnitude of stylized facts that are used in modeling and policy formulation can be invaluable. If the right stylized facts can be used as a basis for theory, and theorists have good indications of the relative quantitative importance of various phenomena, it is far more likely that the theory itself can make a useful contribution.

Demonstrating that there were responses to incentives, that developing countries could expand export earnings, and that they did have comparative advantage in other than primary commodities helped economists and policymakers understand the relationship of trade to development.

For that reason, high marks must go to the analytical research that pointed to measurement techniques such as effective protection and cost-benefit analysis, enabling policymakers and their analysts to obtain empirical quantification, however rough, of the relevant magnitudes.\(^{36}\)

In like manner, the empirical demonstration of the similarity of policy responses across developing countries and of the wide and largely irrational variation in incentives for import-competing industries showed what was wrong with existing policies.

Overturning or, more accurately, interpreting the accepted stylized facts, therefore, was a prerequisite for developing a better theory of trade policy for development. But theory was important in many ways, in addition to pointing to appropriate measurement tools. First, good policy-relevant theory provided blueprints for those windows of opportunity in which governments genuinely sought to improve economic performance, as was the case in Taiwan and Korea in the early 1960s, and in Chile, Mexico, and India in later decades.\(^{37}\) Having the blueprints from good theory on hand is obviously a major contribution. Second, theory was invaluable when it showed why simple interpretations of received doctrine were in fact wrong. This was the case with the theory of first-best

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36. There is another example from a related field. As is well known, multilateral negotiations with regard to agricultural protection were completely stalled until the 1980s, when economists at the OECD proposed the use of a "producer subsidy equivalent" to measure the degree of government intervention in various agricultural commodities across countries. That tool permitted negotiations to begin restricting and dismantling agricultural protection.

37. See Harberger (1993) for a discussion of the roles played by economists in some key policy reform episodes.
intervention to correct domestic distortions, and where comparative advantage was interpreted to mean developing countries would specialize in the production of primary commodities, and with the infant-industry argument.

These considerations suggest that research results, in order to be relevant to policy implementation, should be interpretable into phenomena that are observable, quantifiable, and recognizable by the policymaker. A negative result can be counterproductive precisely because the policymaker is informed only that a certain generalization, such as comparative advantage and the value of free trade, is not without exception; the generalization can then be ignored.

A more general statement of the problems inherent in theorems that propositions are "not generally true" would encompass all theory cast in terms of "anything can happen." While there are conditions under which a wide range of outcomes (Pareto-inferior, a bad equilibrium; Pareto-superior; and so on) are possible from the same instrument, it would challenge the skills of even a superb theorist to develop a case for the chaotic politics prevalent in Turkey in 1957, in Ghana in 1983, and in Argentina in the late 1980s. An exception does not rationalize all the possible policy alternatives to free trade.

There is a criterion for efficient resource allocation, equating domestic and international marginal rates of transformation. Even "dynamic" factors that contravene part of the static efficiency criterion are measurable. Yet the "anything can happen" theories do not show how the phenomena under examination may be quantified, and thus provide rationalizations for policies that cannot pass muster.

Perhaps the lesson is that there is a danger that economic theory will be misinterpreted in the policy arena, and researchers should distance themselves from policy conclusions that are not warranted by their analysis. Theoretical papers that end with "It has been shown that, under conditions x and y, policy z may no longer represent an optimum. . . . Therefore policy should . . ." are obviously overstepping their bounds when the empirical relevance of x and y is not yet established, and even more so when conditions other than x and y also may be important (as, for example, with rent-seeking).

But many good theory papers are written for an audience of other theorists. In such instances, good theory may be misused, and some individuals will harness it to their own ends. Applied economists, as well as theorists, should be careful to interpret the policy relevance of results in ways that minimize the scope for misinterpretation. This is as true for those seeking to find dynamic aspects of exporting, or endogenous aspects of a "big push," as it should have been for those developing the infant-industry or optimum-tariff arguments. Complex results, such as those noted by Johnson (1970), are particularly suspect because they can be interpreted selectively to suit the decisionmaker or lobbyist.

Finally, there is theory that provides no guidance as to when or how to observe the phenomenon. In such instances, it is difficult to find policy implications that will not be captured. One challenge for theorists might be to ask for
at least one plausible incentive-compatible mechanism under which policymakers and bureaucrats might improve on the inefficiencies identified by the theorists. The existence of infant industries, of cases in which there are rents that might be captured by appropriate strategic trade policy, and of informational asymmetries and other market imperfections cannot be doubted. But until the magnitude of these phenomena can be measured or incentive-compatible mechanisms for correcting them can be devised, theorists asserting the presence of such phenomena simply provide carte blanche to policymakers and bureaucrats to intervene in whatever ways they like—and ammunition that special interests will seize on to bolster their causes.

Economists who recognize these realities are more likely to develop research results that can contribute positively to policy formulation.

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Measuring the returns to social science research is a task so daunting that it might be best to avoid even attempting it, except for one fact: Decisions have to be made about funding such research. As a former U.S. government official in charge of arguing the merits of budget proposals for the Economic Research Service of the U.S. Department of Agriculture, I felt keenly the need for reasonably objective criteria for evaluating the program proposed. Although this need was never satisfied, budgetary arguments were laid out and decisions made. Some lessons from this experience will be discussed later. I will begin by going to first principles to consider how a systematic evaluation of the merits of policy-related research might be organized conceptually. Then I examine several cases. Unfortunately, these can be only feebly quantified. The evidence available indicates that the returns to policy-related research in agricultural economics have been larger than the costs of the research, most likely by a substantial amount.

**Analytical Methods for Measuring Returns to Policy Research**

The valuation of policy research requires estimation of the difference the research makes in peoples’ actions and its economic and social value. Most of the actions involved are political; that is, the actions of policymakers in government, as well as lobbying actions of private citizens seeking to influence government. For example, evaluations of the returns from publicly funded research on agricultural production influence both decisions about public spending on agricultural research and lobbying actions by producer or consumer groups for or against such spending.

*Policy Research as Information for Decisionmakers Facing Uncertainty*

Policy research is an intermediate product, an input into a political decision. It is helpful to think of the product of policy research as information. Following Hirshleifer and Riley (1992, Chapter 5), information is not a stock of certain knowledge, but a flow or increment of “news” or “messages” of uncertain
reliability about a state of affairs that is itself uncertain. This characterization of
the output of policy research lends itself naturally to a treatment of policy actors
as Bayesian decisionmakers under uncertainty. Lindner (1987) takes a similar
view of agricultural economics research generally, whether policy-directed or not.

To demonstrate how this characterization works analytically, Figure 9.1 uses
as an example a two-state world. State 1, $S_1$, is a world in which export demand
for a commodity is inelastic. State 2, $S_2$, is a world in which export demand for
that same commodity is elastic. It is not known which of these two states the world
is in. A goal for a research program is to determine which state prevails.

Two policy actions are possible: $A_1$, an acreage control program; and $A_2$, a
production subsidy program. The value of any policy outcome is measured by
$V(i, j)$ where $i$ is the state of the world and $j$ is the policy chosen. In Hirshleifer
and Riley, as in decision theory generally, $V$ is utility, but for this chapter, $V$ is a
weighted sum of benefits to producers and consumer-taxpayers. There are four
possible outcomes, shown as points $M_1$, $N_1$, $M_2$, and $N_2$. If policy $A_1$ is chosen,
the result is point $M_1$ if demand is inelastic and $M_2$ if it is elastic. If $A_2$ is cho­
sen, the result is $N_1$ if demand is inelastic and $N_2$ if it is elastic. The policymaker
will regret choosing $A_1$ if demand is elastic, and $A_2$ if demand is inelastic. The
horizontal dimension of Figure 9.1 measures subjective probabilities, $\pi_2$, of $S_2$
(increasing left to right from 0 to 1)—and this implies $\pi_1$ since $\pi_1 + \pi_2 = 1$. Sup-
pose there is maximum uncertainty about $S_1$ and $S_2$ in the sense that either outcome could occur with equal probability: $\pi_1 = \pi_2 = 0.5$. The expected value of $V$ can then be maximized by choosing policy $A_1$, giving expected $V$ at point $H$.

Now consider the value of a research program to estimate the elasticity of export demand. The research program will conclude that demand is either inelastic or elastic, but it may be incorrect. Suppose the uncertainties are as given by the following probabilities:

```
<table>
<thead>
<tr>
<th>True state</th>
<th>Research findings</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Inelastic</td>
</tr>
<tr>
<td>$S_1$: Inelastic</td>
<td>0.8</td>
</tr>
<tr>
<td>$S_2$: Elastic</td>
<td>0.3</td>
</tr>
</tbody>
</table>
```

If demand is inelastic in reality, there is an 80 percent probability that the research will correctly obtain this finding. But there is a 20 percent probability the research will incorrectly say that demand is elastic. It is slightly harder to detect that the true state is elastic—the research program will give the correct result with a 70 percent probability and the incorrect result with a 30 percent probability. These likelihoods provide us with an operational measure of research quality—the probability that the research findings are accurate.

If Bayes’ theorem is used to calculate the posterior probabilities, and the assumption is made that it would be equally likely beforehand that demand would be elastic or inelastic (that is, probability is 0.5 for each state), the following posterior probabilities would result:

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<table>
<thead>
<tr>
<th>True state</th>
<th>Research findings</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Inelastic</td>
</tr>
<tr>
<td>$S_1$: Inelastic</td>
<td>0.7</td>
</tr>
<tr>
<td>$S_2$: Elastic</td>
<td>0.3</td>
</tr>
</tbody>
</table>
```

1. The calculations are as follows:

```
<table>
<thead>
<tr>
<th>State</th>
<th>Prior probability</th>
<th>Likelihood of state: Given inelastic estimate</th>
<th>Prior probability times likelihood</th>
<th>Posterior probability, given estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Inelastic</td>
<td>0.5</td>
<td>0.8</td>
<td>0.40</td>
<td>0.40/0.55 = 0.727</td>
</tr>
<tr>
<td>Elastic</td>
<td>0.5</td>
<td>0.3</td>
<td>0.15</td>
<td>0.15/0.55 = 0.273</td>
</tr>
<tr>
<td>Given elastic estimate</td>
<td>...</td>
<td>...</td>
<td>0.55</td>
<td>...</td>
</tr>
<tr>
<td>Inelastic</td>
<td>0.5</td>
<td>0.2</td>
<td>0.10</td>
<td>0.10/0.45 = 0.222</td>
</tr>
<tr>
<td>Elastic</td>
<td>0.5</td>
<td>0.7</td>
<td>0.35</td>
<td>0.35/0.45 = 0.778</td>
</tr>
<tr>
<td>Given elastic</td>
<td>...</td>
<td>...</td>
<td>0.45</td>
<td>...</td>
</tr>
</tbody>
</table>
```
In Figure 9.1, if the finding were that demand was inelastic, then \( \pi_2 = 0.3 \), and \( A_1 \) would be chosen with expected benefits at \( R \). If the finding were that demand was elastic, then \( \pi_2 = 0.8 \) and the choice would be \( A_2 \) with expected benefits at point \( T \). So the research makes no difference in the choice in the first case, but it causes a reversal of course in the second.

What is the ex ante value of the research program? Assuming that each finding was equally likely before the research program was undertaken, the expected value of \( V \) is the mean of \( R \) and \( T \), which is plotted at point \( G \). The value of the research program is the distance \( G-H \) (the length of the double-headed arrow).

The two-state Bayesian model is simple but nonetheless helpful in pinpointing the determinants of the value of research. In particular, three elements determine the value of the research. The first is the value of acting on the information the research provides, if the information is correct. (This is \( M_1-N_1 \) if the research finds \( S_1 \), and \( N_2-M_2 \) if the research finds \( S_2 \).) The second is the state of prior knowledge about the subject of the research. (In Figure 9.1, the prior probabilities of 0.5 for each state represent the absence of prior knowledge.) The third is the quality of the research, as measured by the likelihood that research findings are correct. (In Figure 9.1, this is the likelihood matrix that places us at point \( R \) or \( T \).)

The significance of the model is that, first, these three elements are the only aspects of the research situation that matter, and second, the model shows how to put these elements together to calculate a dollar value for the ex ante value of the research. Moreover, the model helps to confirm which common-sense intuitions about the value of research are most likely to make a difference. For example, the following propositions can be obtained straightforwardly from the model:

- If we already know the state of the world, research is valueless. Proof: “Knowledge” means \( \pi = 0 \) or \( \pi = 1 \). Applying the model using these prior probabilities always generates the same posterior probabilities (0 or 1). Therefore, the policy choice cannot be influenced by research and there is no opportunity for social gain.
- The research is valueless if it does not reveal information about the true state of the world. Proof: No information means the likelihood of each of the two states is 0.5, regardless of the research findings. Applying the model with this likelihood matrix means that the posterior probabilities are the same as the prior probabilities, so that \( R \) and \( T \) in Figure 9.1 cannot differ from their preresearch position (someone who started at point \( H \) would end up at point \( H \)).
- The research is valueless if the same policy is preferred for all states of the world the research considers. Proof: In Figure 9.1, this situation would be represented by having both \( N_2 > M_2 \) and \( N_1 > M_1 \) (the inequalities could
be reversed). $A_1$ (or $A_2$) would then be chosen regardless of the location of $H$, $R$, or $T$. So there would be no gain from the research.

- The ex ante value of research is nonnegative. This is a less obvious result. It is true that some findings (or "messages") have no value: In the export demand elasticity example, a finding of inelastic demand would make no difference in any action taken. But even so the research program—the "message service," in Hirshleifer and Riley's terminology—has some value as long as it reduces uncertainty about the world sufficiently. In Figure 9.1, $\pi$ would increase to about 0.6, where $N_1N_2$ and $M_1M_2$ cross. Even uninformative research—research that delivers no news and leaves the posterior probabilities the same as the prior probabilities—has value no less than zero because it makes no difference in action taken.\(^2\)

In contrast to this result, analyses of crop information, as in Lave (1963) or Bullock (1976), show that improved information can make its users (producers) worse off. In the cases used by Lave and Bullock, this occurs because producers react to short-crop information by making prices lower than they would be without the information. No single producer generates these prices, but their joint response does.\(^3\) More generally, a problem is that the direct users of information make up only part of an economy. Bradford and Kelejian (1978) provide a more complete evaluation of the welfare of crop forecasting information with Bayesian market participants. In their model the social value of improved information is nonnegative (though of course the additional value may be less than the cost of providing the additional information).

The approach used by Hirshleifer and Riley (or Bradford and Kelejian) is relevant for an evaluation of the information generated by policy research. They assume that policymakers take into account all the effects of what they do and can use the research or not as they see fit.\(^4\) Nonetheless, this approach, with its

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2. Research can make the user worse off ex post if its findings are wrong: for example, if one is at $T$ but the true state is inelastic, which has a probability of 0.2. Moreover, research can make the user worse off ex ante if he or she places too much confidence in it. One of the beauties of the Bayesian approach is that it takes into account the likelihood that the research gives the wrong answer. However, that approach assumes that the user knows the likelihood of getting the wrong answer. If the research is of lower quality than thought, the posterior probabilities should be closer to the prior probability of 0.5 than is shown in Figure 9.1. For example, if the message that placed the user at $T(\pi = 0.78)$ should really have placed him or her just to the right of $H(\pi = 0.51)$, a decision to switch policies would have been wrong and the ex ante value of the research would have been negative.

3. Lave considered an economywide context and still estimated that better information yielded a net loss, but he assumed that the opportunity cost of resources in production (of raisins, the commodity he analyzed) was zero, and this apparently drove his (informal) economywide assessment.

4. While the approach of Hirshleifer and Riley is identified here because they use a diagram like Figure 9.1 to analyze the value of information, this approach to the graphical analysis of the two-state decision problem under uncertainty goes back to Savage (1972).
ex ante consideration of the research program in the Bayesian context, has demanding requirements for information, as Graham-Tomasi (1984) notes with reference to papers by Antonovitz and Roe (1984) and Chavas and Pope (1984) on producers' use of information under uncertainty. One needs not only the probabilities of true states of the world given researchers' estimates, but also the ex ante probabilities of the accuracy of researchers' findings. It is necessary to assume that policymakers can carry out a professional evaluation of the research findings they use, at least sufficiently to determine their expected value. This is largely what their professional staffs are for.

The Hirshleifer and Riley framework also suggests that research can be valuable simply by reducing the range of uncertainty about the state of the world, no matter what point estimate is discovered. In Figure 9.1, increased evidence moves the user toward one state or the other (the probability of state 2 gets nearer 0 or 1). Whichever way the research program takes the user, its ex ante value increases as points \( R \) and \( T \) are pushed farther apart. This will be true so long as these are policy options better suited to some states of nature than to others. If the policies generated the same utility no matter what the world is like, then there would be no point in using policy research to determine the state of the world, according to the proposition that research is valueless if the same policy is preferred for all states.

**Quantifying the Gains: National Income**

The effect of policy research on political choices will be referred to as the action-difference caused by the research. It is a measure of the output of policy research. The value of this output will often be difficult to measure. The approach used in this chapter is grounded in the idea that the value of the research is the well-being generated by the action-difference. Welfare economics offers a rigorous theoretical basis for some judgments about well-being, using compensation principles. However, this body of theory is not fully applicable even in well-worn areas such as the use of gross domestic product (GDP) as a measure of social well-being. A practical approach frankly must be limited to a partial assessment. The discussion in this chapter will for the most part be limited in precisely the way it would be in a discussion of national income accounting: The value of social science research is taken as its contribution to society’s aggregate real income.

A second limitation is that no attempt is made to measure the total value of policy-oriented social science research. Instead, attention is restricted to marginal changes, or to partial budgeting of social science research projects. That approach places the focus where it really is in practical discussion, on decreases or increases in the amount of social science research.

The focus on the marginal valuation of aggregate effects on real income effects is limiting in several respects. It means, for example, that there is no place for the scientific value of research as opposed to its economic value. It
means there is no place for a purely cultural value of research. This may be more of an issue in the humanities, but it is also pertinent to the noneconomic aspects of social science. Among other considerations, the focus on GDP means that one gives no consideration to whether redistribution of income harms any socioeconomic, ethnic, or occupational group in relation to another. Economics research can and often should try to estimate distributional effects, such as the gains or losses of farmers. Indeed, this information is often sought as a product of research. But these findings add to the returns to research only as they affect an aggregate measure of well-being. Finally, a focus on national GDP omits any gains or losses to foreigners.

Basic and applied social science research can be assessed by using the same criteria, but basic research can be conceived of as a source of two inputs crucial to applied research: raw data and methods for drawing inferences from data. Consequently, the generation of data on agriculture and rural areas comes under the purview of policy-oriented social science research, to the extent that it influences policy actions.

As an example, consider economic assessments of the returns to agricultural production research. An evaluation of this body of work requires estimates of its action-difference, and of the real income the action difference has generated. Neither estimate could be made without a study far larger than is feasible in this chapter. The absence of such estimates in the voluminous literature on agricultural research policy indicates the practical impossibility of producing such estimates with confidence (see reviews in Huffman and Evenson [1993] and Alston and Pardey [1996]). The amounts and trends in public research funding can be observed for nations and subnational areas, which are arguably influenced by economists’ estimates of returns to research, but estimation of the action-difference requires an estimate of the amount that would have been spent had the economists’ estimates not been published. Huffman and Evenson (1993, 238) argue that governments have responded only weakly to economists’ estimates of returns. Their evidence consists of estimates that imply that returns to research have not been equalized over alternative categories of research spending. Interest-group politics appear to be more important in allocating research spending over the alternatives.

Quantifying the Gains: Redistribution

Because so much political action is aimed at a redistribution of well-being, the question remains how to value policy research that influences political choices that involve redistribution. It is possible that research on the elasticity of demand for exports influences policy not so much because the deadweight loss to domestic national income from acreage left idle increases as demand becomes more elastic, but because producer gains, narrowly defined, decrease. A Bergsonian social welfare function can measure gains in such cases by increases in social welfare. A practical approximation would be the much-used weighted
sum of individual gains and losses from a policy in which the weights are indicators of political preference that vary among individuals or groups. The difficulty is in determining what weights to use. There is one situation, however, in which it may be possible to measure gains from policy research without having to assign weights to interest groups. This involves policy research that influences the political actions of interest groups themselves. For example, U.S. wool growers in 1996 voted down a program through which they assessed themselves funds to be used for promoting wool. If that outcome had been the result of social science research that measured the returns of promotion activities to producers, then the claim could justifiably be made that the gains to producers (the difference between the market rate of return and the presumably lower rate of return on funds invested in wool promotion) were returns to social science research.

An important aspect of policy-related social science research, whether used by policymakers or interest groups, is that much of it is oppositional. A research program on the environmental regulation of agriculture, for example, may consist of a range of studies that increase uncertainty about the state of the world. In terms of the Hirshleifer and Riley framework of Figure 9.1, this research program would reduce expected utility. This result does not violate the proposition that new knowledge always has nonnegative value; rather, this is a research program that reduces what appeared to be knowledge. In terms of the earlier, more formal discussion, it is a research program on the probabilities in the likelihood matrix (relating research findings to the true state of the world).

The preceding suggests that the framework constructed here is too narrow, for surely there are situations in which it is better to be ignorant than to believe things that are untrue. In fact, some political actions are constructed to be appropriate when the state of the world simply is not known. These actions are not typically a decision to do nothing.

Consider policy choice in the following situation. A large group of pesticides is considered to be a possible danger to human health. Regulating them would cause substantial economic losses. But it can also be argued that no human health dangers have been established. The situation can be modeled as a revision of Figure 9.1. The world is either in state 1 (pesticides are dangerous) or state 2 (pesticides are benign). If the world is in state 1, policy $A_1$, regulating pesticides, generates good results; but policy $A_2$, laissez-faire, generates severe losses. If the world is in state 2, policy $A_1$ gives rise to large costs, while policy $A_2$ is appropriate. Now consider an oppositional research program that results not in information that increases certainty that either state 1 or state 2 prevails, but instead increases uncertainty about the true state. This reduces the expected value of either policy.

This is not the end of the story. Knowing that little is known, the best course of action would be to devise a policy attuned to this situation. Devising policies to fit a situation, including its uncertainties and conflicts, is indeed a
key skill of the successful policymaker. One might suggest policy $A_3$, a pilot program of limited regulation, along with monitoring of pesticide use and residues, and controlled studies in the field. This policy has nonlinear utility in the probabilities of the states. It is a poor policy if the state of the world is known to be either 1 or 2 but a good policy if the state is uncertain.

Here the following objection may be made. The state of the world really is 1 or 2, even if no one knows which. What is known is that policy $A_3$ will be wrong in either state. Indeed, this line of argument is typically raised against compromise policy proposals that call for a pilot program or for further research. An appropriate mechanism for choosing a policy if the experimental learning approach is inadvisable might be to choose policy $A_1$ or $A_2$ randomly, that is, by flipping a coin. However, the expected value of the random-choice approach, if $\pi = 0.5$, could well be less than the utility of $A_3$ (the experimental policy) at $\pi = 0.5$. What such a case would reflect is that if $\pi = 0.5$, a pilot program or study would help determine whether the true state was state 1 or state 2. What gives $A_3$ its higher utility is the expected value of being able to choose $A_1$ or $A_2$, as appropriate, in subsequent legislation. In this example, policy-oriented social science research is itself a policy option.\(^5\)

When oppositional research leads to greater uncertainty about the state of the world, and information-generating policies such as $A_3$ are not available, the oppositional research program may still prove beneficial. This will occur when the policy choices being determined are those of interest groups themselves, such as a choice between pieces of legislation that a commodity group should lobby for. In this case, with $A_3$ unavailable, the decision may be to lobby for neither $A_1$ nor $A_2$, but rather to withdraw from the political debate. If the policies are alternative schemes for redistribution, this passive outcome will tend to increase national income, because any redistributional scheme will have deadweight costs. For example, the American Farm Bureau Federation has at times held back from lobbying for higher support prices and has instead made a general argument for lower taxes or a balanced budget. This stance probably reflects doubt about how much regulated support prices benefit Farm Bureau members, who are typically the more business-oriented farmers, in the long run. To the extent that these doubts have been engendered or reinforced by agricultural economists who question the income-increasing effects of price supports (for example, see Johnson 1973), the work of such economists has contributed to national income.

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5. The 1990 Farm Act contained more than 100 mandates and authorizations for studies, reports, and pilot programs. They typically reflected stalemate after extended debate on other policy options. The sum of studies became so large that section 2515, “Scarce Federal Resources,” was inserted, stating that the Secretary of Agriculture may “rank by priority the studies or reports authorized by this Act and determine which of those studies or reports shall be completed. The Secretary shall complete at least 12 such studies or reports” (U.S. Code, 104 STAT. 4075).
Case Studies

Returns to Research: The Value of Publicly Provided Information

The literature on the value of information on crop forecasting, which dealt mostly with estimates produced by the U.S. Department of Agriculture (USDA), was discussed earlier as it related to the evaluation of policy-related research. This literature's more direct application is to the debate on public spending for agricultural commodity forecasting. In addition to the papers cited earlier, some notable published research can be found in Doll (1971), Ryan and Perrin (1974), Freebairn (1976), Antonovitz and Roe (1984), and Babcock (1990). Irwin (1996) provides a comprehensive review and assessment of the implications of this literature for the social value of public situation and outlook programs.

As an example of the use of this kind of research in policy discussion, the American Agricultural Economics Association (AAEA) Data Task Force states:

While valuing information presents a complex challenge conceptually and empirically, several efforts have been made to measure the economic value of agricultural statistics. Based on data from the 1960s, Hayami and Peterson (1972) estimated the net benefits of improving the accuracy of NASS (U.S. National Agricultural Statistics Service) (then the Statistical Reporting Service) production estimates for a large number of farm commodities. Even under conservative assumptions, a reduction from 2.5 to 2.0 percent error (which in general is a NASS goal) returns $100 for every dollar invested. A little later Bradford and Kelejian (1978)—using a different model, data from 1955–1975, and a Bayesian rather than a "naive" loss function—provided a different measure. They produced an estimate of $64.29 million (1975 dollars) for the annuitized annual value to the U.S. economy of eliminating sampling errors in NASS's monthly estimates, just for winter wheat production. Antonovitz and Roe calculate the annual social benefit gain of $78 million annually from the adoption of USDA outlook forecasts by U.S. feed cattle producers. (AAEA 1996, 10)

Irwin (1996) develops a further argument that the observed effects of USDA crop reports on market prices are evidence that these prices generally respond to changing conditions more quickly with these reports than they would otherwise, and that this is a significant source of welfare gains.

Suppose that the finding of the literature is correct; that is, the net social benefits of NASS data are large. To be concrete, suppose that a cut of $10 million annually in the NASS commodity data program is being considered—as it was in the early 1990s. Suppose further that the social value of the in-

6. The Farm Act of 1990 required, and the Bush Administration also desired, expansion of USDA’s database on chemical use by farmers. The expense of this could be covered by cutting some
formation lost would be $20 million annually, that is, that the studies are correct about the net social gains from these estimates. This estimate of gain is far less than the $100 gain per $1 spent of Hayami and Peterson, but one has also to give some weight to others’ far lower estimates of net gains, Office of Management and Budget (OMB) assertions that some NASS surveys have costs in excess of their gains, and even the views of some legislators and citizens that NASS estimates as a whole are valueless or worse (for example, see Weber 1997).

To take a more modest assessment, suppose there is a $10 million annual net social gain from maintaining rather than cutting the NASS budget ($20 million marginal information value minus $10 million marginal cost). To place a value on the social science research that generated this estimate, one has to estimate the change in the probability of a budget reduction caused by the research findings. How has research on the value of information actually influenced the policy process that determines the NASS budget? The key places to look are decisions by the OMB and USDA’s arguments to OMB in the executive branch and the appropriations committees in Congress along with the authorizations made by agriculture committees concerning agricultural data. While commodity groups dominate agricultural policy, their voice is usually muted and the messages somewhat mixed with respect to NASS and data issues. This means that the influence of agricultural interests in maintaining NASS barely outweighs the generic arguments of budget cutters for reducing NASS appropriations. In this context, the value-of-information literature does make a difference. It makes a difference not because congressional appropriators or their staffs read this literature, but because they listen to experts from the administration and nongovernmental institutions, on whom they rely for substantive judgment. Experts in the administration and elsewhere do not typically rely on particular studies cited, but on the general climate of opinion among professional economists—who are well represented both on the staffs of congressional committees and in the executive branch (and not just in USDA). And it is this climate, expressed in the AAEA Task Force report quoted above, that provides the favorable reception to additional spending and resistance to cuts in NASS.

In short, it is not extravagantly optimistic, and indeed it is fairly conservative, to suppose that the body of social science research on agricultural commodity data bears half the responsibility for the decision not to cut $10 million from NASS in FY 1991. Given the earlier estimate of a $10 million net gain for this decision ($20 million marginal social benefit minus $10 million marginal
cost), the expected value of the social science research would be $5 million annually, for as long as its influence lasts.

This preceding computation is a marginal calculation, related to a $10 million change in the NASS budget. The corresponding total assessment would involve an estimate of total federal expenditures on agricultural statistics (by NASS, parts of the Economic Research Service, the Agricultural Marketing Service, and in the agricultural censuses) in the absence of research findings on the value of all publicly generated information in agriculture. The literature contains no estimate of that value, or even a wild guess. But again, the policy influence is through a general sense that the public information enterprise generates net social gains. That general sense would, however, probably exist among economists even in the absence of research on the specific value-of-information issues cited earlier. Hence, even if economics as a field of expertise is responsible for the size of the agricultural statistics budget (though it is too small, according to the predominant estimates), it would not be correct to attribute the gains to specific empirical studies. Nonetheless, these studies have strengthened the general view that favors the provision of data by public agencies.

**Rate of Return to Agricultural Research**

A related, but larger and more widely known body of social science research is on the benefits of research that results in technological improvements in agricultural production. The value of that social science research can be assessed in a way that parallels the discussion of agricultural statistics. Again, prevailing estimates are that the social rate of return is quite high, well above going rates of interest, and even above the arguably more meaningfully comparable rates of return to private entrepreneurial investment before taxes. Estimates that public investment yields net social gains prevail among economists, even with the complications that research is undertaken privately as well as publicly and that there are a variety of technical reasons why many studies overestimate the returns relevant at the current margin (see Alston and Pardey 1996, Chapters 6 and 7).

Thus there is once again the question of the role that research on the rate of return has played in determining the amount of the public spending on research. In this case, however, none of the many authors who have worked on returns to research have published an assessment of the difference this body of work has made in policy choice. Huffman and Evenson supply evidence, as noted earlier, that public decisions on the allocation of research funds do not follow exactly the suggestions made in rate-of-return studies. But that does not mean that policymakers are uninfluenced by those studies, or that policymakers use their findings suboptimally. One of the services of the Bayesian valuation model of Figure 9.1 is to show how research findings can be valuable to rational decisionmakers even though decisionmakers do not gain certainty from the findings of researchers.
Although a monetary assessment of the value of rate-of-return research cannot be provided here, it is worth noting that the dollar-value stakes for research on agricultural production are much higher than for statistical information. Instead of perhaps $150 million in annual public outlays on U.S. agricultural data, federal and state spending on agricultural production research totaled about $1.5 billion in the mid-1990s. If rate-of-return research has caused this spending to be 10 percent higher than it would have been otherwise, and if that marginal spending generated a social rate of return 10 percent above the relevant opportunity return, then the rate-of-return research generates a social gain of $15 million annually ($1,500 million × 0.1 × 0.1).

Trade Liberalization Studies

In the past 20 years, extensive social science research, both positive and normative, has been undertaken on the consequences of the international trade policies of individual countries and on the benefits of both regional and global trade liberalization. What is the value of that research? In the 1990s a series of policy changes occurred that make it plausible that the value of this research has been substantial. The most significant movement toward global liberalization of trade in agricultural products since World War II was the agriculture agreement reached in the Uruguay Round of the General Agreement on Tariffs and Trade negotiations, concluded in 1994 and phased in over six years. In addition to relaxing some long-standing nontariff barriers to trade, the Uruguay Round agreement set up arrangements under the new World Trade Organization that should help minimize the use of health, safety, or other quality-control measures as disguised measures of protection for domestic industries. The agreement also established important groundwork for further liberalization through negotiations to expand on the Uruguay Round agreement at a later date. Moreover, beyond global liberalization, important movements toward liberalizing agricultural trade within regions have taken place in the North American Free Trade Agreement (NAFTA); the Mercosur agreement among Argentina, Brazil, Paraguay, and Uruguay; and the negotiations to bring Central European countries into the European Union (EU) and increase the access of the newly independent states of the former Soviet Union to EU markets. In all these developments, economists were important not only in developing the public-interest rationale for liberalization, but also in providing technical analysis and advice to both private-sector interests and to governments.

In assessing the value of these activities, one may reasonably invoke what can be called the “bootstrap” approach suggested by Harberger (1954). He balanced the costs of antimonopoly efforts of economists against his estimate that there would be an annual gain in welfare of more than $300 million if monopolies were replaced by competition in the U.S. economy. Analogously, the

7. The “bootstrap” label indicates that the measure of value of the research depends on the economic values that the research itself measures.
estimates of the World Bank (1986), Tyers and Anderson (1992), and others summarized in Blandford (1990) can be used to show that the complete global liberalization of trade in agricultural commodities would bring net social benefits of $30 to $40 billion annually. So if trade were liberalized, and if that liberalization were attributable in part to assessments by economists, the social value of those assessments could be estimated. (But the work of economists who have argued against trade liberalization must also be included on the cost side.)

Now that the Uruguay Round, NAFTA, and Mercosur are in effect, this issue can be posed in a more practical context. The difficulty is that these agreements appear to have had only small effects so far. But if even 2 percent of the gains from complete liberalization have been achieved and economists can claim 25 percent of the responsibility for this, then the worldwide benefit from the trade policy research of agricultural economists would be $150 to $200 million annually (0.02 x 0.25 of $30 to $40 billion).

Analysis of National Commodity Market Intervention

Many countries have changed their national policies in the past decade, usually in the direction of free-market reforms. New Zealand led the industrial countries by making such changes in 1984–86. The reform of the Common Agricultural Policy of the European Union in the 1990s was a paler version of the New Zealand reforms, but it had larger deadweight losses to begin with. Many developing countries, notably Chile, Argentina, and China, have implemented limited market-oriented reforms. Other countries, such as India and Egypt, seem to be starting out on similar paths. The former Soviet sphere is rebuilding its agricultural policies as part of its post-Communist evolution. Agricultural economists have taken part in analysis and debate in all these countries. In New Zealand, Hungary, Poland, Latvia, Ukraine, India, and Egypt, where the author has been able to observe these debates, there is evidence that contributions of economists have made a difference in the form and content of agricultural policies. But their impact appears marginal, and the reforms have typically not been extensive enough to reduce economic waste and inefficiency.

It would be going too far even to guess the value of the economists' contributions by attempting to quantify the real-income effect of their advice and the extent to which policy decisions have been influenced by that advice. However, there exists a more direct quasi-market indication of economists' value in this sphere. This is the willingness to pay for policy research. It is reasonable to use the willingness of the public to pay to place a provisional lower bound on the value of analytical services provided by social scientists employed by government agencies. The lower bound is provisional because two further considera-

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tions may weaken the case. First, some theories about bureaucracy have suggested that employees of government agencies may be paid more than the value of their output (see the parallel arguments on agricultural production research in Pasour and Johnson [1982]). Second, some public research and analysis is conducted largely if not entirely to respond to or refute economic assessments by private-sector interests or foreign governments. To this extent, national income might remain unchanged if both sides reduce their efforts. (A similar point could be made concerning lawyers' services, brand advertising, and military preparedness.)

This approach suggests valuing the output by looking at costs, as is often done in measuring the services of lawyers, accountants, entertainers, and other providers of intangible services. However, because of arguments such as those referred to above, it is useful to consider attempts to measure the value of the output independently.

In the United States, the deadweight loss from farm programs can be estimated to have been $4–5 billion annually in 1985–87. This was due mostly to productive cropland left idle (Gardner 1990, 59). In 1990, policy reforms were undertaken that reduced the deadweight losses substantially, arguably by three-fourths or more, principally by eliminating annual acreage-idling programs. This was done more dramatically following enactment of the 1996 Farm Act. The author has argued that agricultural economists made a significant contribution to these reforms (Gardner 1996). The influence did not stem from quantitative estimates of deadweight loss, but from work on a variety of topics that showed that the commodity programs cost taxpayers and consumers many billions of dollars, but accomplished much less for farmers. These findings did not influence legislators directly, but through newspaper editorialists, government experts in both the executive and legislative branches, and commodity group representatives.

It has to be recognized that economists have not spoken with one voice on agricultural policy. Pasour (1988) emphasizes the role played by agricultural economists in putting U.S. commodity programs into place in the 1920s and 1930s. However, it would be wrong to say that because agricultural economists argue both sides of policy issues, or first one side and then the other of the same issue, these efforts largely cancel each other out. There appears to have been a consensus in favor of government action in commodity markets before World War II that evolved into a consensus opposing such actions in the 1980s and 1990s.\footnote{The evidence for this consensus in the 1990s is the near unanimity of agricultural economists' general views in Congressional testimony on the 1990 and 1995 farm bills, and in expressions of net social costs of existing programs in outlets such as \textit{Choices} (see Gardner 1996).} It could well be that the consensus was correct in both cases—that commodity policies had net social benefits in the 1930s, as argued for example by Cochrane (1993) and Clarke (1994), but that the policies generated net social costs after World War II.
The key point is that these policy issues involve difficult scientific questions as well as potent political forces. In this case, the extent to which policy research has reduced the range of uncertainty about the effects of public choices is an appropriate indicator of the ex ante gains that should be attributed to the research.

Beyond the more sweeping work on agricultural policy, a larger body of economics research has been devoted to particular questions raised in the implementation of farm programs. Examples are the amount of acreage that should be placed in set-aside programs each year, the size of sugar import quotas needed to achieve legislated price targets, selection criteria for land offered by farmers for inclusion in the Conservation Reserve Program, purchase prices for butter and nonfat dry milk needed to reach the legislated farm price of milk, the size of the export bonuses offered under the Export Enhancement Program, and the rules needed to implement pilot programs in revenue and crop insurance. The provision of alternative answers for these and a hundred similar questions can reduce the efficiency loss from commodity programs by many millions of dollars. The net benefits are not quantifiable, but one can have greater confidence in an appeal to the quasi-market mechanism through which government agencies hire researchers because they believe the benefits from the findings justify the staff costs. The author had several years' experience in negotiations on the budget of the Economic Research Service (ERS), a part of USDA. These took place within USDA, between USDA and the OMB, and between the Bush (senior) Administration and the appropriations committees of the House and Senate. In these discussions, OMB made a serious attempt to weigh the value of services provided by ERS against the agency's costs. It asked why agriculture was endowed with so many more government economists, given the size of agriculture relative to other sectors of the U.S. economy. OMB ended up accepting an annual budget for ERS of more than $50 million principally because of its acceptance of the ex ante value of the product in policy formulation.

Research on the Effects of Advertising and Promotion

A narrower and more precisely defined area of inquiry is the effectiveness of generic product promotion, generic meaning not specific to a product identified by a brand name. Such promotion is typically funded largely by producer groups, and the basic issue is whether funding promotion is profitable for the industry.

One recent example is provided by Blisard, Blaylock, and Smallwood (1996). They estimate that over a 10-year period, 1984–94, generic advertising raised fluid milk sales by 5.6 percent, or 12.8 billion pounds. Fluid milk advertising expenditures were $296 million over this period. Of these, $110 million were attributed to a $0.15 assessment on each per hundredweight of milk sold, mandated by the Dairy and Tobacco Adjustment Act of 1983. Blisard and his
colleagues estimate the "gain per act-increased advertising dollar" to be 117 pounds of additional milk consumption.

Should milk producers, and U.S. citizens at large, rejoice in this finding, assuming that they can believe it? To give a rough answer for milk producers, suppose that the elasticity of demand, \( \eta \), is between \(-0.5\) and \(-1.0\), and that the elasticity of supply, \( e \), is between 0.5 and 1.5 (over a 10-year period of adjustment). Then the price effect of a 5.6 percent demand shift \( (0.056/[e - \eta]) \) is between 0.022 and 0.056 for the range of elasticities considered. Using an average farm price of $11 per hundredweight, this implies a price increase for farmers of between $0.24 and $0.62 per hundredweight. With 2.3 billion hundredweight sold \( (12.8/0.056/100) \), the gain to producers, measured by producers' surplus, is between $550 million and $1,400 million. Because this gain was achieved by spending $110 million, the implication of the research finding (although Blisard et al. do not put it this way) is that the fluid milk promotion program is a good investment for farmers. Indeed they ought to spend more on promotion.

The gain to society as a whole must also consider the well-being of milk consumers. There is no standard method for determining this in welfare economics. If advertising changed tastes, an underlying assumption behind estimates of consumer benefit has been violated. If advertising conveyed information, and the increased willingness of consumers to pay for milk measures the value of this information to each consumer, then consumers are no worse off as a result of the promotion program. The producers' net gain is a rough lower-bound measure of society's net gain.\(^{10}\)

For this chapter, the issue is how to value the social science research that led to the net-gain estimate, that is, to assign a value to studies such as those of Blisard, Blaylock, and Smallwood (1996). Many such studies have been carried out, and they tend to show that promotion programs have substantial net benefits, at least to producers.\(^{11}\) But the accuracy of the findings is open to question, probably more so than the studies of gains to agricultural research. (With reference to export promotion programs, see Sumner [1995, 106–109], and works cited therein.) One of the policy actions often taken in promotion programs is a vote by producers on whether to assess themselves. In 1996 the sheep

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10. The main possibility that would render this argument invalid would be if not all consumers were influenced by the advertising. Then the additional consumption of some consumers would drive up that price for all consumers, and the added costs to the uninfluenced consumers should be counted as a loss attributable to the program.

11. Some studies have analyzed the consequences of promotion programs financed by taxpayer funds, notably the Market Promotion Program (MPP, formerly Targeted Export Assistance) which makes grants for the promotion of U.S. agricultural products abroad. Cost-benefit analysis of the MPP has been required by OMB. Such analysis has estimated that the program has had substantial net benefits.
producers, in a close vote, rejected their promotion program, despite the support of the leadership of their main producer organization.

In the two-state, two-policy version of the Hirshleifer-Riley model of valuation, research on the consequences of generic advertising may have little effect on the action chosen (impose an assessment on producers \([A_1]\), as opposed to not imposing an assessment \([A_2]\)). This would be true even when the advertising delivers a message of support for its message (advertising pays \([S_1]\)), as against advertising does not pay \([S_2]\)). The reason is that the perceived quality of the research findings is low in the sense that the likelihood of \(S_2\) is not much different whether the research estimates the true state to be \(S_1\) or \(S_2\). In this case the posterior probabilities will be so close to the prior probabilities of \(S_1\) and \(S_2\) that the ex ante gain from research will also be low. Figure 9.2 illustrates the gain in such a case as \(G-H\), the distance of the double-headed arrow. The ex ante gain may well be so low that it is not worth paying for the research, which is possibly true for research on the value of generic commodity promotion.
In the case of promotion programs and other policy-related social science research, there is another aspect to the issue in that private-sector enterprises or organizations themselves fund the research. For example, the citrus industry has commissioned studies of the value of generic promotion of orange juice. So instead of relying on public cost-benefit analysis, there is a market test for the ex ante value of social science research. Even if such market decisions are questionable, and they have been questioned, for example, when people pay for stock market advice, one should not be too quick to write off the research as not worth the amount paid for it. The payment, after all, is voluntarily made by people who economists normally assume are rational.

Costs of Agricultural Policy Research

The costs of agricultural policy research are mainly for personnel salaries. The directory of the American Agricultural Economics Association (AAEA) lists about 730 people as specialists in agricultural policy (category S840). Many members list more than one specialization, however. The directory lists about 3,400 members, and 4,800 reported specialties. This suggests that 520 full-time equivalent AAEA members spend their research time on agricultural policy (3,400 \times 730/4,800). Assuming that an average of 40 percent of the average academic working year is spent on research (as opposed to teaching and extension), this implies that each year these members spent the equivalent of 210 years conducting agricultural policy research. At an average cost of $60,000 plus 50 percent of that in benefits and overhead support, this amounts to $19 million annually ($90,000 \times 210). However, some agricultural policy research is done by specialists in subjects other than agricultural policy, resource economics, for example, and some agricultural policy researchers are not members of the AAEA. To roughly accommodate these omissions, one can double the cost and round up to $40 million annually.

The relevant question about the benefits of U.S. policy-related economics research in agriculture is, then, whether they add up to $40 million annually. Without wishing to claim much for the rather sketchy case studies, earlier discussions suggested annual returns of approximately $5 million from value-of-information research and $15 million for returns-to-research studies. Assuming that the United States reaps 20 percent of the gains from trade liberalization, research output in that area would be valued at about $40 million annually. Assuming that one-third of the budget of the ERS is spent on implementation research whose value is what is paid for it, the annual benefit would be $20 million. The value of broader agricultural policy research is impossible to estimate with any pretense of precision. To have a figure to work with, however, assume that 1996 Farm Act reforms eliminate one-half of the $4 billion in deadweight losses of pre-reform policies, and that 5 percent of the credit for this achievement
goes to agricultural policy research. This implies a $100 million annual benefit from this research.

Summing up, the U.S. economy receives benefits equal to $180 million ($5 + $15 + $40 + $20 + $100) from the $40 million spent on agricultural policy research. Taking into account remaining costs and benefits left out of these numbers; policy issues not covered in this analysis, such as soil conservation, environmental and food safety regulation, and domestic marketing policy; the lag between research findings and policy results; and the uncertainty in all the benefit values given, one might double the costs and place a doubling and halving range around the benefits. One could then say that benefits of between $90 million and $350 million resulted from costs of $40 to $80 million. Even with the most pessimistic view of annual benefits ($90 million) and costs ($80 million), U.S. agricultural policy-oriented economics research pays.

Conclusions

This chapter analyzes policy research as a source of information, primarily for policymakers, who are Bayesian decisionmakers. The value of their decisions depends on the state of the world, and different policy choices are optimal depending on that state. The true state, however, is unknown, and the output of research is a reduction in uncertainty about that state. The value of proposed research is the ex ante addition to the expected value of the social objective function. This value is nonnegative for decisionmakers who can accurately assess the likelihood that a research program will reveal the true state. But the value of the research may not justify the costs, especially the costs of research that is not expected to contribute significantly to the identification of the true state.

The case studies of agricultural policy research considered in this chapter suggest that there are substantial net gains to the ongoing policy research agenda. However, the quantification of these gains is highly conjectural and narrowly focused on agricultural economics in the United States. Nonetheless, the conceptual framework and empirical evidence given provide reasonable grounds to believe that policy-related research has generated benefits that more than cover the costs.

One line of skepticism about this conclusion as a justification for maintaining or increasing public support for social science research is that even if the arguments of this chapter are accepted in the aggregate, policy-related social science research in agriculture could be managed more efficiently. Many policy-related studies and publications, even the entire output of some researchers, could have been omitted without loss. So only a part, and perhaps even a small part, of the research need have been funded. With better management, less policy research can be funded in the future without a loss of expected future net benefits.
The flaw in that conclusion is its presumption that better management is feasible, in the sense that winners can be picked in advance. It is likely that, as with drilling for oil, recruiting football players, or breeding racehorses, one has to back the low-return efforts to get the successes. As a practical matter, that is, if the policy research budget were to be cut, it is likely that almost as many productive projects would be cut as unproductive ones.\textsuperscript{12}

\textbf{References}


\textsuperscript{12} To this point it is natural to append the standard researchers' refrain: It might be worthwhile to fund additional research on how to "pick winners" and in other ways manage policy research better.


PART IV

Estimating the Value of Economics Research
Economists have made important contributions to our understanding of the impacts of natural science and technology-oriented agricultural research. They have made fewer contributions to our understanding of the benefits of social science research, especially policy research. The need to evaluate policy research arises from two primary sources. First, as budgets tighten, public decision-makers request more evidence of the impacts of research, including the impacts of policy research. Second, assessments of the value of policy research programs may help guide the allocation of resources to programs with the highest expected payoffs.

This chapter suggests practical methods for assessing policy research programs, both ex post and ex ante. It reviews the nature of the evaluation problem, draws lessons from previous attempts to assess social science research, and suggests an approach to evaluation, applying it to two examples of policy research. It stresses the importance of identifying the counterfactual, that is, what would have happened or will happen without the policy research, and addressing the uncertain nature of key parameters.

The Nature of the Problem

Conceptually, the output of policy research is information. That information is usually imbedded in institutions. Many policy research benefits result from reductions in the costs of welfare-improving institutional change. In measuring the contribution of research to changes in policy, several issues are crucial. These include how to apportion credit among the many factors that affect a particular policy change, how to assess the causality between research and the implementation of a policy, and how to measure impacts that may not be reflected in the marketplace. Apportioning credit can be difficult because several pieces of research may contribute to a single policy change. Moreover, basic research on theory and methods contributes to the success of policy research and is part of the research cost, although it represents a fixed cost that is difficult to apportion among applied policy research programs. The more basic research is available,
the lower the cost of designing better policies and the more likely it is that policy prescriptions will be correct.

Causality is an issue because it is difficult to ascertain whether an institutional change is due to social science research or to some other source. In other words, the question arises: What would have occurred without the policy research? It is easier to link a change in yield to plant-breeding research than it is to link an institutional change to policy research.

The market fails to value the results of certain types of research aimed at technical change, but this problem may affect a higher proportion of the benefits of policy research. However, discovering ways to use markets to value as many of the results of policy research as possible is one of the keys to evaluating that research. Nonmarket assessment tools can be used, but only when the market value of the research cannot be determined.

The timing of policy research and the quality of data must also be considered. Timing is important because of the time value of money; advice offered after a decision is made can be worth little, and timeliness influences the likelihood that recommended changes will be adopted.

**Demand for Policy Change**

The demand for policy research is derived from the demand for institutional change (Ruttan 1984), even if that demand comes from groups motivated by self-interest rather than a desire to improve social welfare. Understanding the demand for a change of policy is particularly important when evaluating policy research ex ante. While one can observe whether policies have been adopted and measure their effects ex post, ex ante evaluations require estimation of the demand for a policy change and the likelihood that a new policy will be adopted. Key determinants of the demand for a policy change are the growth or decline of per capita income; changes in product and factor markets, including disequilibria in those markets; constraints on institutional change imposed by ideology, religion, and tradition; short-run political changes and budgeting pressures; and transaction costs and collective action. These issues must be considered case by case. For example, growth in per capita income can increase the demand for environmental amenities, but the value that a particular society places on those amenities needs to be measured empirically. As the demand for environmental amenities grows, so does the payoff to policy research that can influence them.

Changes or disequilibria in product and factor markets that result from technical changes, growth in population or income, or other sources can be major influences on the demand for institutional change (Schultz 1975; Ruttan 1984). Policies are changed to accommodate institutional stresses and strains brought about by disequilibria or secular changes in these markets. Policy research and extension can speed up institutional changes to accommodate such disequilibria. For example, as an economy develops, labor migrates from agri-
cultural to nonagricultural employment. The result is that problems with income
and adjustments in agriculture persist until late in the development process.
These problems often create a demand for policies that address rural poverty
but keep the price of food low to the growing urban population.

Ideology, religion, and tradition may reduce the demand for policy change
(Ruttan 1984), while a change in political climate can stimulate it. The result is
that the demand for policy research can be either constrained for long periods
of time or ratcheted up rapidly.

Transaction costs, combined with the possibility for collective action, im­
ply that the political power of different interest groups influences the direction
of policy change and the likelihood that a policy will be adopted. Transaction
costs and collective action therefore influence both the distribution and the ef­
ciency of policies. This influence makes an assessment of transaction costs and
the strength of interest groups relevant to ex ante assessment of policy research.

The interaction among factors that influence the supply of and demand
for institutional change determines the potential value of policy research. As
market disequilibria, a growing divergence between private and social costs,
and other factors increase the demand for research, the returns to such research
also increase. An examination of factors contributing to the shifts provides clues
about these returns. It is necessary to make such an examination in an ex ante
evaluation. The presence of multiple social objectives also affects how policies
are valued.

Multiple Objectives

Social science research can be designed either to influence the decisions of
policymakers or to measure or change the behavior of economic agents such as
producers and consumers. Policy-oriented social science research is clearly
aimed at policymakers, whose decisions in turn influence the behavior of indi­
vidual economic agents. Public officials use policy levers to address a variety of
social objectives. Policy research may be needed to identify both the levers that
can be used to address specific objectives and the advantages that particular
policies may have over others.

A primary objective of agricultural research in most societies is to increase
economic growth by improving agricultural productivity and efficiency. Policy
research can contribute to this objective by improving allocative efficiency, re­
ducing externalities that cause social costs to diverge from private costs, and
reducing transaction costs that constrain economic growth. A second objective
in many societies is to influence the income distribution to favor a particular
group or factor of production. Policies can be potent tools for addressing such
an objective, much more so than research that produces new technologies. Pol­
icy research can measure effects on distribution and provide information about
how best to achieve such distributional goals. A third social objective is to reduce
risk to health and safety, food security, prices, production, or income. Policy
research has been aimed at the development of institutional mechanisms to efficiently reduce risk.

The presence of multiple objectives for policy increases the diversity in types of research conducted and complicates evaluation. It implies that it will be difficult to assess the aggregate benefits of policy research without identifying the types of policy research conducted or proposed and without explicitly identifying the objectives that the policies are designed to achieve. This identification is also needed if the evaluation is to estimate the tradeoffs involved when particular objectives are targeted by policy research.

**Lessons from Previous Evaluations of Social Science Research**

The few previous quantitative evaluations of social science research have assessed the contributions of marketing, price analysis and outlook, or management research to increases in efficiency. The methods used have included economic surplus and econometric and decision theory approaches, sometimes in combination. Some studies have focused on how publicly provided information alters the behavior of producers. None has attempted to evaluate policy research, but some offer guidance about how one might approach such an evaluation.

**Economic Surplus**

The economic surplus approach has been used to evaluate the net benefits of more accurate outlook and price information (see, among others, Hayami and Peterson 1972; Bullock 1976; Freebairn 1976a, 1976b; Bradford and Kelejian 1977, 1978; Norton and Schuh 1981). When producers estimate a commodity price to be above or below the equilibrium price, they produce a quantity that is larger or smaller than the equilibrium quantity. Research that leads to price forecasts closer to equilibrium reduces net social losses. Previous studies have derived expressions for calculating expected welfare effects on producers, consumers, and society as a whole. Freebairn (1976a, 1976b), for example, assumes that prices of agricultural commodities are formed rationally and compares perfect forecast prices to rational (but inaccurate) forecast prices in decision-making about supply.

One attraction of the economic surplus approach is that it generates measures of economic benefits that are directly comparable to the measures of benefit that have traditionally been generated for research on production. The approach may also have the benefit that the policy research itself may have calculated the potential surplus gains. A key question, however, is how to operationalize the surplus approach when addressing both uncertainty in the knowledge and beliefs of decisionmakers (producers, consumers, or public officials) and uncertainty found in additional parameters (such as causality or likelihood of policy adoption) required for the evaluation. Bayesian decision theory provides a possible method for placing a value on the information available to decisionmakers under these conditions.
Decision Theory

In Bayesian decision theory, learning takes place and modifies the probability distributions with which a decisionmaker starts. Such prior distributions can be assigned to all parameters wherever they appear in the model. Bradford and Kelejian (1977, 1978) and Norton and Schuh (1981) use Bayesian decision theory to evaluate information on the outlook and price of wheat and soybeans. They argue that the expected price is arrived at through Bayesian learning, but also that learning results not only from observing market behavior but from information on the production outlook and prices. This information causes speculative inventory holders to revise their prior probability distributions. It also affects the commodity price distribution. These changes are evaluated using the economic surplus approach. The value of the information is the difference between maximum utility with outlook information and maximum utility without it. Bradford and Kelejian (1978) used their model to forecast wheat crops in the United States. Norton and Schuh evaluate the soybean outlook information provided each year by economists from the University of Minnesota.

Uncertainty is summarized by the dispersion of the subjective probability distributions of individuals over possible states of the world (Hirschleifer 1973). Information consists of events tending to change these probability distributions. One challenge in using decision theory is estimating the subjective probabilities before and after the new information is received. Norton and Schuh assumed that subjective prior distributions were based on historical probabilities of price movements for the preceding 15 years. Conditional probabilities were determined by comparing past outlook projections with the actual states that occurred. These probabilities were then used to calculate posterior probabilities using Bayes’ formula.

Lindner (1987) suggests that Bayesian decision theory should be applicable to policy evaluation as well. However, one problem in using it to evaluate policy research is the difficulty of obtaining estimates of priors from policymakers for the various states of nature. Unlike the situation in which the decisionmakers are individual economic agents such as producers or consumers,

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1. The decision theory approach can be summarized as follows: A variety of actions are open to the decisionmaker, \( a_1, a_2, \ldots, a_m \). Several states of nature \( S_1, S_2, \ldots, S_n \) are also possible and the decisionmaker has some knowledge of the likelihood (prior probability) of such states occurring, \( P(S) \). With a given amount of knowledge, the decisionmaker will choose the action \( a_j \) which maximizes his or her expected utility. The expected utility of the \( j \)th action is \( \Sigma u(a_j|S_j)P(S) \). Now, if additional information, \( Z_1, Z_2, \ldots, Z_m \) becomes available to the decisionmaker and he or she has knowledge of the probability of the information coming true, that is, \( P(Z|S) \), by Bayes’ theorem, \( P(S|Z) = P(S)P(Z|S) / \Sigma P(S)P(Z|S) \). The revised expected value of \( a_j \) is now \( \Sigma u(a_j|S_j)P(S)P(S|Z) \). The value of information is the difference between the maximum utility with and without the information and this can be compared with the cost of obtaining the information.

2. They also assumed that the utility function was linear so that maximizing expected profits was equivalent to maximizing expected utility.
market data cannot be used to estimate these prior distributions. Therefore subjective estimates would have to be elicited from the individual policy decision-makers. Although such elicitation is not impossible, it is clearly difficult, more so than eliciting prior distributions from producers for such variables as market prices. A second problem with this approach is in defining the states of nature for which prior expectations exist. It appears that direct application of Bayes' theorem to calculate the value of policy information will usually be difficult.

**Econometric Approaches**

Alternative procedures for valuing information that do not rely on Bayes' theorem are suggested by Antonovitz and Roe (1985) for price uncertainty and by Roe and Nygaard (1980) for situations in which the parameters of the underlying technology are not known with certainty. These procedures rely on the notion that producers base their allocation of resources on subjective estimates of prices and the underlying technology, and that these estimates are not entirely accurate. Information can be valued using "subjective" rather than "actual" or "more informed" production or profit functions. As with the decision theory approach, the value of information generated for an individual firm can be translated into a measure of the value of information to society using the economic surplus approach. The procedures suggested by Antonovitz and Roe were used by Norton (1987) to evaluate research and extension in farm management and marketing. However, the approach would be difficult to use to evaluate policy research because reductions in deadweight losses and success in meeting objectives other than increasing efficiency would be missed.

Simpler econometric approaches have also been used to evaluate farm management research. For example, farm management expenditures have been included as a separate variable, along with other research and extension variables, directly in a countrywide production or profit function model (see Evenson 1978). One can envision including a similar expenditure or publication-count variable (that is, the number of policy articles published) in such a model, but there would be many drawbacks to the approach.

First, policies are of many different types and are aimed at multiple objectives, so it is unlikely that using a policy variable in an aggregate profit function would produce a significant result. Estimates of production functions are not likely to be useful because policy research does little to shift the production function, except perhaps by influencing the rates at which technology is adopted. Policy research often results in movement along a given supply function; it does not shift the function itself. Also, one would like to measure the impacts of research on the achievement of each objective; a production or profit function approach usually addresses only the efficiency objective.

Second, many of the reductions in deadweight losses that come from reductions in policy distortions and externalities would be missed in such an approach, and reduction in allocative errors are obtained by factors other than
policy research. Third, it would be difficult to use cross-sectional data because most policies spill across geographic areas. Time-series data are limited in many developing countries. Fourth, it would be difficult to specify what should be included in a policy variable. For example, should macroeconomic policies be included? If one included a broad set of policies in a publication count, using, say, a cross-country data set, the analysis would miss key costs involved in a policy dialogue, and the variable would not provide a link to the cost of the research. In other words, even if one did estimate a cross-country production function and picked up a significant effect of a policy variable, it would be difficult to link the effect back to research and there would be legitimate concern that the significance would be only spurious. In addition, such an aggregate analysis would not help an evaluation of specific policy research programs or the allocation of resources among types of policy research. The preceding points do not argue against using econometrics to measure parameters affecting changes in surplus. They do suggest that an aggregate econometric analysis of the benefits of policy research may be uninformative.

Previous social science evaluation efforts show that an aggregate econometric approach is not likely to help much, and direct application of Bayesian decision theory to the evaluation of policy is difficult. The latter is particularly hampered by the difficulty of estimating the prior distributions of the decision-makers directly influenced by policy research, public officials, and bureaucrats. However, the decision theory approach highlights how important it is to incorporate uncertainty in the evaluation. The multiplicity of policy objectives and types of policies dictate a disaggregated assessment of individual policies or classes of policies. Disaggregation of the impacts of policies by societal objectives may be necessary as well. Such a need to disaggregate suggests that it would be appropriate to adopt a case study approach using economic surplus as a measure of welfare changes.

The economic surplus approach, despite its well-known deficiencies, is likely to be the starting point for policy research evaluation, as it can incorporate subjective estimates of parameters and generates results that can be compared both with each other and with results from evaluations of production-oriented research. Such an approach allows for ex ante or ex post evaluations of a portfolio of research projects. The section that follows presents a possible framework.

**A Framework for Measuring the Benefits of Policy Research**

The evaluation of policy research requires an assessment of the value of policy changes. It also requires, for ex post assessment, a determination of the contribution of policy research to those changes and, for ex ante assessment, an estimation of the likelihood that a proposed policy change will be adopted. Several steps must be undertaken in the evaluation, whether it is done ex post or ex ante. First, the problem must be defined in terms of the objectives of the evaluation,
the policy research programs to be evaluated, objectives for the policies, and the path followed from the research to the change in policy (actual or projected). Formulae must be defined for measuring economic benefits. An illustrative list of programs, a policy path, and examples of how benefits might be measured for various types of policy programs such as marketing, credit, labor, and research are provided in Norton and Alwang (1997).

Second, market data (such as prices, quantities, and elasticities) and other data needed for research must be compiled. Estimates must be made of potential changes in costs, the likelihood that a policy will be implemented, lags in research and implementation, and so on. For ex post evaluation, the policy research itself may have already supplied some of this information. If not, it may be necessary to elicit people's subjective estimates. Such subjective estimates are needed for all ex ante evaluations.

For ex post analysis, an attempt can be made to talk to people involved in the policy decision to determine the influence of the research. There is likely to be a trade-off between the time and cost spent locating and interviewing people and the quality of the evaluation. For ex ante analysis, it may be possible to assess historical probabilities for factors such as the adoption of a policy in order to place rough bounds on future probabilities. For example, the probabilities that results of research will be adopted may be lower when they call for land reform than when they call for changes in price policy. It may be useful to ask policymakers or staffers how large the total benefits must be before the opposition of interest groups can be overridden.

Uncertainty of parameter estimates argues that information should be gathered in a way that will allow for a distribution around those estimates. For simplicity, a triangular distribution can be used for key parameters such as the probability of a policy being adopted or the probability that research contributed to an observed policy change (Anderson, Dillon, and Hardaker 1977; Alston, Norton, and Pardey 1995).

The third task is to analyze the data by combining the data and the formulae for economic surplus derived to assess specific policy research, applying capital budgeting methods to the streams of benefits and costs, and then using the results to help justify programs or choose among alternatives.

The fourth task is to use and interpret the results. If internal rates of return (IRRs) are calculated for the policy research program, they can be used to assess the merits of the investments and to compare them with alternative public investments. The net present values can be used to create rankings to help with decisions about resource allocation.

Agricultural policies often have distributional objectives. If the policies evaluated have such objectives, it can be useful to assess whether they are being met and to determine the opportunity cost in terms of the total benefits of the policy research program that are sacrificed if benefits are skewed toward specified groups.
The two examples given below help to illustrate the feasibility of the method. The first example, policy research on deforestation in the Brazilian Amazon was chosen because the problem affects the entire world and the information is in the public record. The second example, tax and exchange rate policies influencing pesticide prices in the Philippines, is an example of an evaluation of research with benefits that must be projected into the future. Both examples are merely illustrative, as key parameters were obtained from fewer sources than one would normally use for such analyses.

**Measuring Benefits from Policy Research:**

**Policies Associated with Deforestation in the Brazilian Amazon**

The accelerated deforestation that has occurred in the Brazilian Amazon represents an important area of policy research that began to be addressed in the early 1980s. LANDSAT satellite imagery that became widely available at that time showed that deforestation began in the 1960s following construction of highways such as the road that runs from Brasilia to Belém. This deforestation accelerated sharply in the 1970s, raising concern about its impacts on the local and global environment. Anecdotal evidence indicated that part of the deforestation stemmed from policies that encouraged the expansion of the agricultural frontier, and research began looking at the links between policy and deforestation.

Studies by Browder (1985), Mahar (1989), Binswanger (1991), and researchers at the Brazilian agriculture research agency Embrapa found that agricultural and economic policies created distortions that led to the inefficient expansion of agriculture and uneconomic logging. Binswanger and Mahar categorized some of the policies that created these distortions.

Agricultural income was largely exempt from federal taxation, which created incentives to engage in agriculture as a means of sheltering other income. Policies for land allocation and titling ensured that squatters were granted titles. Titles were normally given for up to three times the amount of the land that was cleared. Land taxes declined with the intensity of use of the land, and forested land was considered to be unused. These tax rules created incentives for uneconomic forest clearing as pasture was considered an intensive land use; by converting forest land to pasture, tax liabilities declined. Agricultural credit policies were linked to deforestation through the need for land title to secure loans. Titling was made conditional on “land improvements.” In practice this meant clearing the land. These tax and credit policies created indirect incentives for additional land clearing and increased the demand for farmland (Mahar 1989; Binswanger 1991). Pan-territorial pricing of inputs and fuel subsidies contributed to inefficient expansion of agriculture to remote areas. The pricing policies also increased the demand for farmland.

In addition to indirect incentives, direct tax incentives were provided for livestock development projects by the Superintendência do Desenvolvimento...
da Amazônia (SUDAM), the regional development agency for the Amazon. The incentives were originally designed to support industrial development, but eligibility was expanded to include agricultural projects. By 1985, SUDAM had approved 950 projects for tax credits; 631 of these were for livestock development (Mahar 1989). Several authors have noted that without tax credits, livestock development had a negative real rate of return (Browder 1985; Binswanger 1991) and concluded that the SUDAM incentives were directly responsible for as much as 10 percent of total deforestation in the Brazilian Amazon. Other tax credits encouraged deforestation; Binswanger and Mahar examined the effects of these credits and their impacts on deforestation in detail. This completed policy research can be used as an example of how to evaluate policy research aimed at an environmental issue with global implications.

**Defining the Problem**

The objective for this evaluation of policy research is to determine, ex post, the benefits from the research on policies affecting deforestation of the Brazilian Amazon. The objective of the original research was to determine how the policies contributed to the misallocation of resources by creating incentives for uneconomic deforestation. The researchers also suggested ways to reform the policies. The policies themselves had multiple objectives. These included increasing settlement in outlying areas to relieve population pressures in other parts of the country and expanding the agricultural frontier.

The value of research can be considered by tracing the effects of research findings on policy decisions: How were policies changed as a result of the research and what was the value of these changes? To do this, evidence is needed on how deforestation was affected by the policy changes and how much of the policy change was itself due to the research findings.

The path the policy research followed began with a public perception of a disequilibrium and led to the ultimate outcome, a change in the offensive policies (Figure 10.1). The critical steps in conducting the evaluation are, first, to measure the benefits of the change and, second, to determine what portion of the policy change is attributable to the research.

**Defining How the Benefits Will Be Measured**

There are at least two alternative markets where benefits from the research could be measured: the product market (mostly beef, but other alternatives such as lumber exist) and the factor market (land). Nonmarket valuation techniques can also be used. The studies suggest that deforestation was largely a by-product of the demand for farmland and pasture, and that this demand was stimulated by

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3. Superintendência do Desenvolvimento da Amazônia, the regional development agency for the Amazon.
the policies. It thus becomes convenient to measure the effects of the policies through their effect on the market for land. The policy-induced distortions affect the land market by shifting the demand for and supply of farmland away from their social optima.

The socially optimal demand schedule for cleared land in Brazil reflects the marginal social benefits associated with an increase in farmland. The "private" or market demand (reflecting the marginal private benefit schedule) diverges from the social benefit schedule because of policies such as direct subsidies, tax credits, and income tax exemptions. These policies increase the demand for cleared land above its optimum for society, raising the equilibrium price and quantity. By raising the equilibrium price, the policies are said to be capitalized into the price of land. Such capitalization leads to some of the equity effects discussed by Binswanger. The effects of the policies are shown in Figure 10.2. Private demand \( (D_p) \) is found to the right of socially optimal demand \( (D_s) \). \( Q_0 \) \( P_0 \) would have prevailed in the market, but the policies lead to a greater equilibrium quantity \( Q_p \) of cleared land and price \( P_p \) of land. The loss to society resulting from the policy is represented by the triangle \( abc \); this triangle is a deadweight loss incurred by taxpayers. This deadweight loss can be thought of as the "cost" to society of achieving other objectives, such as slower population growth in cities. To measure the amount of the deadweight loss, equilibrium quantities and prices need to be known, as must the elasticities for land supply and demand and the amount of the shift in demand induced by policy.

Measurement of the deadweight loss from the policies is complicated by the external costs associated with deforestation. Most of the policy analyses examined implied that these costs were significant, but did not measure them explicitly. These costs include off-farm costs from soil erosion and siltation,
on-farm costs incurred because of reductions in soil quality that result from this erosion, carbon loading in the atmosphere owing to burning of the felled forest, a loss of biodiversity, and the option value associated with the loss of rainforest. These externalities cause the marginal social cost associated with the supply of cleared farmland to diverge from the private cost curve. In Figure 10.3, the private equilibrium (which is observed in the market) occurs at point $c$, with quantity $Q_p$ and price $P_p$. The social optimum occurs at point $f(Q_s, P_s)$. The deadweight loss associated with the policy distortions given the externalities would be the triangle $ebf$. When the policies are removed, the equilibrium price and quantity will be found at point $a(P_0, Q_0)$, since social costs still diverge from private costs. The surplus change from removal of the subsidy is area $abeg$. This is the measure of benefits from policy research.

The divergence between social and private marginal costs must be considered in calculating benefits, because even though a policy does not shift the marginal social cost curve, the amount of the benefits from the policy change will be affected by these externalities (see Figure 10.3). When such externalities are present, the measured benefits of policy research depend on the focus of analysis. If, for example, the focus is on the benefits to residents of Brazil, external costs would be measured only as they are incurred by Brazilians. The share borne by Brazilians of the total global costs of carbon loading in the

\[4.\text{This cost can be considered an externality because of poor information on the part of the soil user/owner of the land.}\]
Measuring the Benefits

atmosphere, biodiversity preservation, and maintenance of rain forest for option or use will be substantially lower than global social costs.

The formula for measuring the change in surplus is as follows.\(^5\) Referring to Figure 10.3, define \(K = (c - b)/P_p\), and \(K' = (e - c)/P_p\). The total change in economic surplus would then be \(CTS = 0.5KP_p(Q_p - Q_0) + K'P_p(Q_p - Q_0)\). This formula is used in the analysis that follows.

Compiling the Data

The data are taken from a number of sources. The equilibrium price of land, taken from Ozorio de Almeida and Campari (1995), was US$219.\(^6\) This price is from sample surveys and is representative of average land prices in the legal Amazon (Schneider 1995).

A minimum estimate of the divergence between marginal private costs and marginal social costs of deforestation is used (this is the distance \(ce\) in Figure 10.3). The minimum value of carbon sequestration per hectare of Amazonian rainforest is estimated to be $272,\(^7\) and the minimum value of the cost associated

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5. In many cases, it is necessary to derive the formula in terms of one existing quantity (say \(Q_p\)). In the present case, estimates have been made of \(Q_p - Q_0\) in the policy analysis, and this can be used in the analysis, removing the need for elasticities.

6. All values are in 1991 U.S. dollars.

7. This value is based on Fearnside’s (1992) estimate of 16 tons of carbon for the average hectare in the legal Amazon, and Fankhauser’s (1994) low estimate of $20 per ton global warming
with maintenance of biodiversity is estimated to be $20 per hectare (this is a lower-bound estimate obtained by synthesizing information found in Pearce and Moran 1994). Without including local external costs associated with loss of soil quality, siltation, and so forth, the minimum estimated social cost is $292 per hectare. This yields an external cost of $73 per hectare ($292 - $219).

The distance be in Figure 10.3 is computed using historical data on subsidies and deforestation. Mahar cites LANDSAT data showing that between 1970 and 1988, 569,000 square kilometers (km$^2$) had been deforested. Between 1987 and 1988, approximately 23,100 km$^2$ were deforested. This gives a total of 545,900 km$^2$ deforested from 1970 to 1987 (World Resources Institute 1994). During this time, approximately $3,704.3 million in subsidies was provided to approved projects and farmers in the region.\(^8\) The subsidy for land clearing was thus $67.86 per hectare ($3,704.30 ÷ $54.59).

**The Effects of Research on Policy Change**

The effects of research on Brazilian policy cannot be understood in isolation. During the late 1980s, there was considerable public pressure for changes in policy to protect the environment, and stronger pressure for a more open political process. The military dictatorship ended with the formation of the New Republic in 1985, and José Sarney, the president until 1990, began institutional changes that were, on the surface, friendly to the environment. The 1989 constitution contained an entire chapter dedicated to the environment. It has been called the most advanced text for environmental protection in the world. Included in the constitution is a declaration that the legal Amazon (which includes approximately 445 million hectares) is an area of national heritage. Policies associated with deforestation began to be examined as the public increased pressure for environmental protection. Many of the offending policies have since been changed.

Three researchers—Browder, Binswanger, and Mahar—were interviewed to gain insights into the effects of the research on policy changes. Three basic questions were asked; the responses are summarized below.

The first question was: “How have policies related to inefficient land clearing for cattle ranching been changed?” The respondents, and other sources, indicated that SUDAM began to enforce a moratorium on subsidies to cattle ranches by the late 1980s, and that all direct fiscal subsidies for cattle ranching were eliminated by 1992. National tax laws were reformed in the early 1990s, clos-

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\(^8\) Data are taken from Schneider (1995, Table 1.3) assuming 4 percent of livestock credit went to the Northern Region and a subsidy of $364.4 million for 1970. Dollar figures are inflated from 1990 to 1991 using a 3 percent inflation rate.
ing the loophole that allowed agricultural income to be exempted from federal taxes. Credit programs have been reformed to a lesser extent, but their contribution to total demand for ranch land is probably now negligible. Input pricing reforms were undertaken.

The second question was: "In your estimation, how much deforestation would have occurred had the policies not changed?" There was a consensus that deforestation has continued. Recent evidence is that cattle ranching may not be as uneconomic as early authors concluded. Schneider (1995) shows that much of the Amazon ranch population consists of smaller establishments that probably did not take advantage of the direct subsidies. Hecht (1993) notes that the current high rates of deforestation are largely associated with a dynamic that may have started with the subsidies for large ranches, but has now taken on a life of its own. Deforestation may have been jump-started by policy errors, but its solution is now more complicated. Estimates are that removing the policies have reduced deforestation about 15 percent.

The third question was: "How much influence did the research have on the policy change? That is, had the policy research not occurred, what is the likelihood that the policy change would have occurred anyway?" Browder was uncertain about the role of policy and suggested referring to the minutes of the Deliberative Council of SUDAM. Binswanger noted that his research was widely disseminated in 1987. For instance, his paper was reprinted in the Jornal do Brasil. He told the authors that "it was deeply influential in the World Bank as well, and the emphasis on policy reform to reduce deforestation was integrated into Bank doctrine." None of the researchers was willing to attach a quantitative estimate to the proportional influence of the research on changes in policy. Secondary sources suggest that policy research made up about 10 percent of the influence in the decisions to change the policies. This is a rough estimate, however, and is subjected to sensitivity analysis in the calculations below.

**Analyzing the Data**

It is estimated that 0.6 percent of the Amazon area is being deforested annually (World Resources Institute 1994). The total estimated area of the Brazilian Amazon is 445 million hectares (Schneider 1995). Deforestation, therefore, consumes about 2.31 million hectares per year. If removal of the policy reduced deforestation about 15 percent per year, this change would preserve about 346,000 hectares of forested land. Therefore, \( Q_p - Q_0 = 346,000 \) in Figure 10.3. This number can be combined with external costs (ec) ($73 per hectare), the per-unit subsidy (bc) ($67.86 per hectare), the estimate for the policy contribution (10 percent), and the economic surplus formula to calculate the benefits of the policy research. The loss in surplus avoided because of the policy change can be estimated for the 15-year period between 1992, by which time most of the policy change had occurred, and 2007. A 5 percent discount rate is used and the net present value of benefits calculated.
By these calculations, the total discounted surplus loss that would be avoided because of policy research is estimated to be approximately $42 million. The external costs associated with deforestation account for about 68 percent of the surplus loss; deadweight losses (excluding social costs) from the policies average about $11.7 million per year. The social losses are incurred by all citizens of the world (recall that the costs associated with atmospheric carbon loading are used here to measure the social costs). The $11.7 million per year are losses to Brazilians alone. Because no information was gathered on the costs of the research, no rate of return can be calculated.

Sensitivity analysis can be performed to test the importance of the 10 percent assumption for the contribution of the research to the policy decision. Because the nondiscounted benefits are equal each year, the variables in the spreadsheet are related multiplicatively, and research costs are not considered, the benefits would simply be halved if the 10 percent assumption were halved. This would put them at $21 million over 15 years at the 5 percent discount rate.

**Interpreting the Results**

Policy research on Amazon deforestation appears to have earned high returns with major international spillovers even if the research had only a small effect on a decision to change policy. This example shows that many environmental policies are like public goods, clearly justifying the research support provided by the World Bank and the World Resources Institute. The example illustrates how returns to policy research can differ sharply, depending on whose perspective is used to perform the analysis. The appropriate costs to include in assessing the benefit of the policy research will depend on this perspective as well.

**Measuring Benefits from Policy Research:**

**Pesticide Policies in the Philippines**

Several studies in recent years have indicated that pesticides are overused in Philippine rice and vegetable production (for example, see Rola and Pingali 1993; Antle and Pingali 1994; Lazaro et al. 1995; Pingali and Roger 1995). To remedy this situation, the Philippine government has (1) empowered its Fertilizer and Pesticide Authority (FPA) to ban or otherwise regulate pesticides; (2) set up an advisory committee to the FPA to make recommendations on pesticide policy; and (3) established integrated pest management (IPM) programs in rice and vegetables. IPM programs appear to hold the greatest long-term promise for reducing pesticide use while increasing producer profits, but the adoption of IPM practices depend in part on their profitability compared to the use of pesticides.

Pesticide prices in the Philippines are influenced by tax and subsidy policies. Therefore a vegetable IPM project in the Philippines recently conducted an analysis of the direct and indirect pricing policies that affect pesticides used on vegetables to predict whether current policies constrain the adoption of IPM
practices, and, if they do, to provide information to the FPA and its advisory committee as they make and implement pesticide policies. Evaluating this completed research project provides an example of how policy research on pricing and environmental policies can be assessed. Each of the components of evaluation identified in the conceptual framework presented above is included in this example.

**Defining the Problem**

The objective of the evaluation is to estimate the net present value of the vegetable-pesticide policy research project, undertaken by Tjornhom (1995) and others, in order to demonstrate the feasibility of such an estimation. The research to be evaluated was focused, but included price analysis, macroeconomic analysis, and environmental policy analysis. The major direct policies that the pesticide policy project assessed were import tariffs and value-added taxes. The Philippine government used both of these to raise revenues and reduce incentives for pesticide use. The major indirect policy assessed was an overvalued exchange rate. This policy put downward pressure on inflation and subsidized both producers who used imported inputs and consumers. It also inadvertently and indirectly subsidized an input (pesticides) that creates an environmental externality. The policy research led by Tjornhom was designed to reduce any policy-induced environmental externality.

The pesticide policy research drew upon previous economic studies such as Intal and Power (1991), who calculated the amount of the subsidies or taxes on several agricultural commodities in the Philippines during the 1980s. The research benefited from discussions with officials of the FPA, pesticide company representatives, Department of Agriculture personnel, and economists on the agribusiness support project funded by USAID and from data they provided. The pesticide policy research itself was funded under the USAID-supported IPM Collaborative Research Support Program (IPM CRSP), which supports collaborative research among the Philippine Rice Research Institute, U.S. universities, the University of the Philippines at Los Baños, and the International Rice Research Institute (IRRI). It took 18 months to produce the findings of the research. Researchers have since exchanged views about policy with a member of the pesticide advisory committee, FPA, and others (Figure 10.4). The policy recommendations derived from the research came close on the heels of a separate research project on rice that was aimed at assessing the effects of pesticides on the health of farmers and the implications those effects had for policy.

The vegetable-pesticide policy project found that the nine primary pesticides used on vegetables in the Philippines received subsidies of approximately 6 percent based on a net direct tax of about 12 percent and an indirect subsidy due to the overvaluation of the exchange rate of 18 percent. The project recommended that direct pesticide taxes (tariff and value-added taxes) be maintained and that the government follow policies that reduce the overvaluation of
FIGURE 10.4 Path for pesticide policy research
the Philippine peso. No decisions about the recommended policies have yet been made. This evaluation of the policy research, therefore, is partly ex post and partly ex ante.

**Defining How the Benefits Will Be Measured**

The vegetable-pesticide policy research could show several net benefits. Environmental externalities might be lower than they would be if the direct taxes on pesticides were reduced or removed. There could be a gain in efficiency and reduced environmental damage if the exchange rate became less overvalued. But losses in efficiency might also occur because of the tax. More IPM practices could be adopted, lowering the marginal social cost curve associated with vegetable production.

Figure 10.5 presents a model of the retail pesticide market for the nine pesticides monitored in the analysis. The marginal private cost of supplying the pesticides, not including any taxes or exchange rate overvaluation that subsidize the imported active ingredients in the pesticides, is represented by $MPC_0$. The marginal social cost curve with no taxes or exchange rate effects is $MSC_0$. This lies above $MPC_0$ because of the social (environmental and health) costs associated with the pesticides. The effect of the overvalued exchange rate is to increase the social costs, shown by the shift from $MPC_0$ to $MPC_e$. The tariff and value-added tax on pesticides (and active ingredients) reduce the social costs, in effect shifting $MPC_e$ back to $MPC_T$. The research on pesticide policy estimated the vertical distance between $MPC_e$ and $MPC_T$ to be about 12 percent of the retail price ($P_T$).

It appears that the policy research is not likely to influence the overvaluation of the exchange rate, although it recommended that the overvaluation be reduced. The actual exchange rate and the equilibrium free market rate differ because of a broad set of policy distortions rather than a single overt policy action. It can be assumed, therefore, that the research can influence only direct tax policy. The net social benefits from maintaining the current tax policy, as recommended by the research, rather than reducing the taxes on pesticides can be measured as area $abc - ade = dbce$ minus the efficiency loss due to the policy distortion ($efc$). The net benefits, considering both the environmental and efficiency effects, would be $abc - ade - efc = dhfe$.

In the long run, the development and adoption of IPM practices would reduce the marginal social cost of vegetable production by reducing the use of pesticides. This effect could be modeled either as a downward shift of the marginal social cost curve of vegetable production or as a reduction in the demand.

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9. The market can be modeled as closed economy because almost all of the pesticide imports come in as active ingredients that are then formulated into pesticides for use in the country, but are not traded.
for pesticides. Because the net effect of current tax policy on IPM adoption is likely to be small, it will be ignored in the present evaluation. Therefore the pesticide policy evaluation will focus on measuring $dbfe$, which can be measured by the formula:

$$CTS = P_T Q_T Z n K',$$

where $CTS = \text{change in total economic surplus}$, $Q_T = \text{pesticide quantity with an overvalued exchange rate and taxes in place}$, $P_T = \text{pesticide price with an overvalued exchange rate and taxes in place}$, $K' = \text{per unit marginal social cost of pesticides as a proportion of } P_T$, $Z = Ke/(e + n) = \text{proportionate reduction in price from } P_T \text{ to } P_0$, $n = \text{absolute value of the price elasticity of demand for the pesticides}$, and $e = \text{price elasticity of supply for the pesticides}$. Derivation of this formula makes use of the fact that $Q_0 = Q_T (1 + Zn)$.

**Compiling the Data**

Data on quantities and prices for the nine pesticides analyzed in the policy research are presented in Tjornhom (1995). For the evaluation, the data on total...
quantity and weighted average price can be used. These are calculated to be 254,291 kilos and 253 pesos, or $10.50, per kilo. Given the current absence of alternative pest management practices, the demand for pesticides is likely to be inelastic. Supply, on the other hand, is likely to be elastic as more active ingredients can be imported as needed, subject to short-run constraints on capacity in the Philippine plants that formulate the pesticides. Tjornhom (1995) estimated the demand elasticity to be –0.5 and the supply elasticity to be around 1.0. These estimates may underestimate the elasticity of supply but is used in the evaluation below. The research estimates the proportionate direct tax to be 12 percent of $P_T$.

The difference between the marginal social cost associated with pesticide use and the marginal private cost may be the most difficult item to estimate. Such an estimate will have to be made using secondary sources of information because it would take a separate research project to get it from primary sources such as contingent valuation surveys or hedonic methods. Expert opinion is also a questionable source. Pingali, Marquez, and Palis (1994) assessed the effects of insecticide use on health costs and found that those costs increased by roughly a half percent for each percent increase in the dosage of insecticide applied. That was enough to offset any profits earned by the farmer in applying the pesticide to rice. This figure does not include the costs of chronic health problems or other environmental effects. While it is more profitable to use pesticides on vegetables than on rice, it is difficult to estimate the marginal social cost of applying the pesticides. It seems reasonable to begin with a conservative estimate of the marginal social cost. Alternative assumptions about that cost can be introduced later. The differences in the returns to policy research that they produce can be assessed using sensitivity analysis. For the analysis below, the marginal social cost of pesticides is assumed to be 20 percent of the average price of the pesticides. The effect of reducing this assumption to 10 percent will be examined later.

Because the research has just been completed and the results not yet adopted, it is necessary to gather expert opinion on the likelihood that its recommendations will be implemented and about what the time lag in that implementation might be. If possible, three or more people knowledgeable about the policy issue should be interviewed. For this example only two people were interviewed. They were asked four sets of questions.

The first was: “What is your most likely estimate (percent probability) that the direct taxes on pesticides will be maintained by the Philippine government decisionmakers, as recommended in the pesticide policy report. What is your lowest likely estimate? Highest probability estimate? Why?”

The respondents gave 70 percent as the most likely estimate, 20 percent as the lowest, and 100 percent as the highest. The most likely estimate was based on the assumption that an economist on the pesticide advisory committee might carry the recommendation to the government. Also, there is increasing public
pressure to reduce the health effects of pesticides. While the pesticide industry lobbies to remove the taxes, farmers usually do not recognize that the tax is included in the pesticide price, so they have not protested the tax. Also, the tax generates revenue for the government.

To the second question, "How long is it likely to take for adoption (an explicit policy decision) to occur?" the respondents gave an answer of two years. The third question was: "What is your most likely estimate (percent probability) that the tax policy would have been maintained irrespective of the research? Lowest likely estimate? Highest probability estimate? Why?"

The answers of the respondents were 60 percent for the most likely estimate, 20 percent for the lowest, and 70 percent for the highest. Pressure is already on the government to reduce the environmental and health effects of pesticides, and, again, the tax generates revenue.

Lastly, the respondents were asked, "When do you believe that sufficient IPM practices will be available for and adopted on vegetables so that use of the identified pesticides will decline, irrespective of tax/subsidy policy?" They gave 10 years as their answer.

Data on policy research costs are also needed, as is a discount rate to use in capital budgeting. Research costs in this example are easy to estimate because the project is focused on particular policies; the costs of research do not have to be apportioned over multiple objectives. Approximately $40,000 was spent over 18 months on personnel, travel, data collection, supplies, communications, administration, and so forth. An additional $5,000 is likely to be spent on fostering a dialogue about the policies at issue. The real discount rate is estimated to be 5 percent.

Analyzing the Data

The data generated as described above and the formula used to calculate the economic surplus can be incorporated in a spreadsheet to calculate the net benefits from the policy research project. The spreadsheet includes a triangular probability distribution showing the probability that a policy will be adopted. A Monte Carlo simulation is used to generate a distribution of values for economic surplus, net present value, and IRR. Incorporating the uncertainty of the parameters in this way provides a confidence interval around the expected value. A simpler approach to the evaluation would be to average the low, medium, and high values for the parameter for policy adoption in order to obtain its expected value for use in the analysis. That approach could be used if a large number of programs were being evaluated and an analysis were needed more quickly.

For the current study, the triangular distribution approach was used as described by Anderson, Dillon, and Hardaker (1977, 268–269). Two hundred uniform variates were generated and included in the equations. These variates were used to generate the triangular distribution for the policy adoption variable (see
Norton and Alwang 1977 for details). The 200 estimates of the probability of policy adoption were then included in the economic surplus equation, so that 200 estimates of net present value and IRR were calculated. The mean for net present value was $134,203 with a 95 percent confidence interval of $11,017.\textsuperscript{10} The mean value of the IRR was 29 percent with a 95 percent confidence interval of 1.5 percent.

The 29 percent rate of return is at the low end of the range often cited in studies of research evaluation. However, the assumptions used in the present analysis may be conservative, especially the assumption that tax policy affects only the nine most important pesticides used on vegetables. Rates of return for specific projects can be expected to vary greatly with few projects and programs having high payoffs and several that yield little. One purpose of ex ante evaluation is to increase the proportion of high returns in the policy research portfolio.

Because of the uncertainty about the 20 percent assumption for the marginal social cost of pesticides, the benefits were recalculated using a 10 percent assumption. This change reduced the estimated mean net present value to $48,726 with a 95 percent confidence interval of $5,509 and an estimated IRR of 16 percent.

*Interpreting the Results*

Although it is only a simplified example, the pesticide policy research evaluation illustrates the feasibility of evaluating policy research projects using the economic surplus approach. A similar approach could be used for a portfolio of possible policy projects or programs. It becomes even more important for such an evaluation to incorporate the risk component in various parameters and to conduct the evaluation assuming two or three alternative levels of funding for each program as discussed above. The pesticide policy research evaluation did not calculate the effects of the policy recommendations on the distribution of the benefits, but it would not be difficult to do so.

*Conclusions and Implications*

Measuring the benefits of policy-oriented social science research is difficult for a number of reasons. The diversity of types and objectives of agricultural policies requires, at a minimum, an analysis that is disaggregated by major type of policy. Causality between completed policy research and changes in policy is nearly always uncertain. Predicting the adoption of policy recommendations is highly uncertain in the ex ante evaluation of policy research. The complexity

\textsuperscript{10} This confidence interval assumes that only the probability of adoption is uncertain. Of course, this procedure could have been followed with triangular distributions around other parameters as well, and a joint probability distribution and confidence interval developed.
of the effects of policy usually implies that evaluation is forced to use approximate measures lest the evaluation of each policy research program or project become a major policy research project itself. Lastly, certain types of policy research generate benefits that are not priced in the market.

These difficulties render any aggregate econometric analysis of the benefit of policy research highly suspect, but do not preclude the use of economic surplus analysis. Bayesian decision theory may be a useful approach for valuing information provided by certain types of social science research programs, especially where they affect the decisions of individual economic agents whose prior distributions can be estimated using market data. However, decision theory is difficult to apply in policy analysis aimed at government decisionmakers because of the difficulty of assessing the prior distributions of policymakers.

Economic surplus analysis has the advantage that it can facilitate evaluation of diverse types of policies, assess the distributional effects of policy research, generate results that are directly comparable to evaluations of technology-oriented research, calculate ex post or ex ante research benefits, and provide an assessment that is consistent with economic theory. In some cases, the surplus measures may be rough, but the more such evaluations are completed, the shorter will be the learning curves for those attempting them in the future.

The uncertain nature of many of the parameters used in an evaluation of policy research dictates a need for carefully structured questions posed face-to-face with those most knowledgeable about the policy process and the proposed or completed research. Ex post analysis or historical documents can provide benchmarks or guideposts for certain parameters in some cases, just as it does (or should) in evaluations of technology-oriented research. However, the inevitable uncertainty of the parameters used in policy-oriented social science research makes it especially important to use distributions around those parameters and to conduct analysis that generates confidence intervals. Sensitivity analysis for key parameters is also recommended.

For ex ante analysis, learning something about the severity of disequilibria in the affected markets and the political costs of policy changes can help to predict whether recommended policies will be adopted and the value of the research. The greater the disequilibria in resource use, the greater the potential gains from relieving the policy or other institutional constraints causing the disequilibria. An excellent example is the large efficiency gains in Chinese agriculture that followed the change in its property rights system and other changes in policies after 1978. It is no coincidence that the fastest growing economies in the world are poor. Lower-income countries are farther from reaching their economic potential than wealthier countries. Thus, when a poor country can reduce institutional constraints that cause economic disequilibria, its growth can take off (Olson 1996).

The political costs of making a decision can greatly affect the odds that policy advice will be followed. Those costs are influenced by the political power
of interest groups. This, in turn, is influenced by the cost of collective action. The latter depends in part on the size and homogeneity of interests of the groups, with smaller homogeneous groups often exercising substantial power. Also, the larger the potential total benefits associated with a policy change, the greater the likelihood that it will be adopted. Understanding why a society adopts its policies is crucial for predicting whether a proposed piece of policy research will change them. Fortunately, a large body of literature has developed on why policy change occurs in agriculture (for example, see Anderson and Hayami 1986 and Roe and Pardey 1991). However, there should be no illusions about the difficulty of making a quantitative evaluation of policy research ex ante. The uncertainty surrounding the estimated benefits of such an evaluation will inevitably be high.

It is clear that policy changes almost always depend on multiple factors, including multiple policy studies. Apportioning credit among the factors is difficult, but not impossible in most cases. For an ex post analysis, it can be helpful to examine the extent to which the policy research was fed into a policy dialogue with decision-makers. Even good applied policy research often ends up only in journals, with little or no influence unless the policy dialogue was explicitly pushed.

Modeling the market effects of policies graphically and in formulae for economic surpluses is important. But the most difficult aspect of an ex post evaluation of policy research is gathering information that allows a careful assessment of possible counterfactual outcomes (what would have happened to the supply and demand curves without the research?). The most difficult aspect for an ex ante evaluation is assessing the probability that policy recommendations will be adopted. Despite these difficulties, it appears that quantitative evaluations of policy research are feasible using carefully structured economic surplus analysis if the methods include means for considering the uncertainty of the parameters.

References


Norton, G. W., and J. Alwang. 1997. Policy for plenty: Measuring the benefits of policy-oriented social research. Staff Paper SP-97-6, Department of Agricultural and
Applied Economics, Virginia Polytechnic Institute and State University, Blacksburg, Va., U.S.A.


Economists have been at the forefront of efforts to develop methodologies to assess the economic benefits from biological and physical research and empirical techniques to quantify these social benefits. Almost 300 empirical studies are now available, containing more than 1800 estimates of rates of return (Alston et al. 2000). It is ironic that economists have only recently begun to explore methodologies that might be employed to articulate and measure the payoffs to policy-oriented economics research. With increasing demands for accountability in the use of public funds, there is now a stronger imperative to examine economics research from the viewpoint of its economic benefits.

The difficulty in evaluating economics research is that outputs or outcomes do not involve products and technical processes, unlike most technological innovations resulting from, for example, applied agricultural research and development (R&D) on crop diseases, crop varieties, fertilizers, or pesticides. Hence, determining the effects of economics research is difficult. Economists have begun to investigate this issue only relatively recently.

In this chapter, I evaluate estimates of the benefits of a specific policy-oriented economics research program. The case study concerns the impacts of rice marketing and trade policy research in Vietnam conducted by the International Food Policy Research Institute (IFPRI) on the government of Vietnam’s decision in 1996 to liberalize trade policy by removing rice export quotas.

This is an edited and abridged version of the article “Assessing the impact of food policy research: Rice trade policies in Vietnam,” published in Food Policy 27 (1): 1–29, copyright 2002, reprinted with permission from Elsevier.

1. Dollar and Pritchett (1998), for example, provide empirical support for the hypothesis that improved macroeconomic and trade policies in developing countries can substantially increase economic growth and reduce poverty, but they do not consider how economics research contributed to these impacts. See Krueger, Chapter 8 in this volume.

2. See, for example, Hayami and Peterson (1972), Freebairn (1976), and Bradford and Keljian (1978) for studies on the value of economic information, and Lindner (1987), Schimmelpfennig and Norton (2003), and Schimmelpfennig, O'Donnell, and Norton (2004) for discussions and applications of the role of Bayesian decision theory in evaluating the effects of policy-oriented economics research.
The approach to estimating the economic benefits of IFPRI's rice policy research program in Vietnam has two components. First, I developed a partial equilibrium model and used it to estimate the potential domestic and international economic benefits of changes to the commodity trade policies of a country. Then, I use interviews with decisionmakers who implemented or participated in the policy process leading to these changes to assess the degree to which the resultant benefits can be attributed to the economics research program. In this case, the benefits of the economics research program are shown to accrue because the program accelerated the implementation of beneficial policy changes.

The chapter is organized as follows. First, a standard analytical model of the gains in economic welfare from trade liberalization is presented. Next, the policy environment in Vietnam is described during the period in which the economics research was undertaken and changes in rice export policies implemented. Then, empirical estimates of the benefits from the economics research are presented. In the conclusion, the key findings of the study are summarized.

A Framework for Valuing Policy Research, Information, and Advice

In 1996, the Government of Vietnam relaxed rice export controls. Figure 11.1 depicts domestic and international benefits that accrue from such a policy initiative. The excess demand (ED) and excess supply (ES) curves facing the domestic country are presented in Figure 11.1b. These are constructed from the rest of the world and domestic (Vietnam) market supply and demand curves presented in Figures 11.1a,b. Under the export quota regime, the domestic country's excess supply curve is $ES_o$, the domestic price is $P_o$, the world price is $P^w$, and imports by the rest of the world are defined by line segment $ef$. When the export quota is removed, the new excess supply curve for the domestic country is $ES_1$, and the new world and domestic price are identical and equal to $P^w$.

The welfare effects of the policy change are as follows. Initially, the State Trading Enterprise that operates the export quota receives rents equal to area $A$ in Figure 11.1a. These rents disappear when the export quota is removed. One part (area $ghij$) is transferred to the rest of the world, and the other part (area $jiba$) is transferred to domestic rice producers. The net gain in economic surplus to Vietnam from the policy change is area $B$ plus area $C$ less $ghij$. The reduction in the world price means that, as illustrated in Figure 11.1c, foreign producers lose economic surplus equal to area $D$ and foreign consumers gain consumer surplus equal to $D$ plus area $E$. Part of $E$, the net gain to foreigners, is a transfer from the domestic State Trading Enterprise resulting from the elimination of dead-weight losses. Given estimates of the model's parameters, quantitative estimates of these welfare effects can be obtained. The issue is then whether—and to what extent—these welfare effects can be attributed to the impact of policy-oriented economics research.
As Bayesian decision analysis suggests, economics research that informs the policy process with relevant and convincing information may lead to quicker decisions than would otherwise be the case. This possibility is illustrated in Figure 11.2 where, in the upper-right quadrant, the conceptual relationship between the duration of the research project and the output of knowledge/information exhibits a phase of increasing and then decreasing rates of return. Moving counterclockwise from the upper-right quadrant, the knowledge/information variable is shown to affect the time needed to implement policy change in the upper-left quadrant.3

The time saved in policy implementation is then transformed, in the lower-left quadrant, into improvements in economic welfare. These are measured as the discounted stream of income gains that accrue sooner rather than later, as a result of more timely policy innovation. When these discounted welfare benefits are mapped in research time space, the relationship depicted in the lower-

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3. This relationship could take many forms other than the reverse L-shape depicted in Figure 11.2. For example, it could be a steplike function, beginning with a vertical phase on the left, a horizontal phase in the middle, and a vertical phase on the right.
right quadrant is obtained. If an economics research program is completed in period $OA$, then the length of $AB$ represents the value of the research.

The framework described in this section is used to evaluate the impact of the economics research undertaken in the mid-1990s by IFPRI in collaboration with Vietnam’s Ministry of Agriculture and Rural Development (MARD) on domestic and international rice trade policies. The next section describes the economic and policy environment in which the research took place and the sequence of policy changes that followed.

4. The policy change could always be reversed at some future time, when the political environment changes. In this event, the benefits of the research may be short lived. Impact evaluations should account for the possibility of such policy reversals.

5. Traxler and Byerlee (1992) have posited a similar relationship for crop management research, which they viewed as generating information that changes farming practices involving the combination of inputs. Babu and Mthindi (1995) suggest that policy research benefits can be separated into pre- and post-decisionmaking benefits, where the former involves capacity building and institutional strengthening. In contrast, Garrett and Islam (1998) suggest that social science research evaluation should only look at outputs, processes, and potential outcomes, rather than focusing on actual policy outcomes. This seems implausible. Although evaluating the quality of the research output and the processes by which a research institute carries out and communicates its research findings is necessary, it is not a sufficient condition for assessing research program impacts.
Vietnam's Economic and Policy Environment

Rice is the major agricultural commodity produced in Vietnam. During the mid-1990s, it typically accounted for about half the country's agricultural production, almost 70 percent of annual cropland, 90 percent of staple food production, and over 10 percent of Vietnam's exports. Rice also accounts for 75 percent of the caloric intake of Vietnamese households and almost 30 percent of all household consumption expenditures.

During the French colonial period in the nineteenth and twentieth centuries, rice exports rose, reaching 2 million tonnes in 1928. The magnitude of rice exports was considered to have contributed to the famine that occurred in the country at this time. Minot and Goletti (2000) argue that this and later famines after World War II contributed to policymakers' concerns about the effects of rice exports on the well-being of the poor. When the French left in 1954, rice exports had declined to 0.15 million tonnes.

In 1989, Vietnam again began to increase rice exports, after importing 1 million tonnes in each of the years 1987 and 1988 (Table 11.1). This followed the liberalization program, which commenced in 1981, when a contract production system replaced collectivization of agriculture. Farmers were able to cultivate individual plots and sell above-quota surpluses on the free market. Then followed the doi moi (renovation) policy in 1986, in which the government announced its intention to encourage the private sector; to give greater priority to agriculture, exports, and consumer goods; to reduce inflation by correcting budget deficits; and to promote international trade.

Specific Politbureau resolutions to encourage agriculture were initiated in 1988, and the household was identified as the basic agricultural production unit. Farmers were allowed to buy, own, and sell agricultural inputs, such as machines, buffaloes, and tools, and cooperative land was assigned to farming households for 10–15 years. In 1989, subsidies and price controls were eliminated, fiscal policy was tightened, gold trading was legalized, positive real interest rates were established, a unified and devalued exchange rate was put in place, and international trade was liberalized. In 1991, an export duty on rice was reduced from 10 percent to 1 percent, imported inputs used to produce exports were exempted from duties, and the Agriculture Bank of Vietnam was allowed to lend to households. Individual property rights were further strengthened in 1993, allowing farmers to exchange, transfer, lease, inherit, and mortgage land. Rice remained the only commodity subject to export quotas. All other quotas were removed in 1995, although, as discussed later, rice export quotas were substantially increased beginning in 1996.

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6. Minot and Goletti (2000) report that the correlation between per capita consumption of rice and exports was $-0.48$ between 1912 and 1944.
### TABLE 11.1 Rice production, trade, and market trends in Vietnam

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</thead>
<tbody>
<tr>
<td>Paddy production (million tonnes)</td>
<td>19.0</td>
<td>19.2</td>
<td>19.6</td>
<td>21.6</td>
<td>22.8</td>
<td>23.5</td>
<td>25.0</td>
<td>26.3</td>
<td>27.6</td>
<td>28.3</td>
</tr>
<tr>
<td>Actual export quota (million tonnes)</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>1.9</td>
<td>1.6</td>
<td>1.9</td>
<td>2.0</td>
<td>2.9</td>
<td>3.6</td>
<td>4.0</td>
</tr>
<tr>
<td>Rice exports (million tonnes)</td>
<td>1.4</td>
<td>1.5</td>
<td>1.0</td>
<td>2.0</td>
<td>1.7</td>
<td>2.0</td>
<td>2.0</td>
<td>3.0</td>
<td>3.6</td>
<td>4.0</td>
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<tr>
<td>Domestic wholesale rice prices</td>
<td>143</td>
<td>135</td>
<td>162</td>
<td>155</td>
<td>159</td>
<td>163</td>
<td>200</td>
<td>207</td>
<td>201</td>
<td>195</td>
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<tr>
<td>(Mekong River Delta)</td>
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<tr>
<td>(U.S.S per tonne nominal)</td>
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<td></td>
<td></td>
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<tr>
<td>Domestic real paddy prices</td>
<td>480</td>
<td>600</td>
<td>490</td>
<td>370</td>
<td>330</td>
<td>320</td>
<td>370</td>
<td>400</td>
<td>...</td>
<td>...</td>
</tr>
<tr>
<td>(Dong per kilogram)</td>
<td>107</td>
<td>87</td>
<td>53</td>
<td>33</td>
<td>29</td>
<td>29</td>
<td>34</td>
<td>36</td>
<td>...</td>
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<tr>
<td>(U.S.S per tonne)</td>
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<tr>
<td>Vietnam rice export price</td>
<td>194</td>
<td>170</td>
<td>226</td>
<td>207</td>
<td>203</td>
<td>218</td>
<td>266</td>
<td>285</td>
<td>247</td>
<td>260</td>
</tr>
</tbody>
</table>

**Sources:** Data for production, export quotas, and exports are from IFPRI (1996, 238) and personal communication with Francesco Goletti; those for wholesale and real paddy prices are from Goletti and Minot (1997, 5–33).
These and other policy changes led to an annual average increase in rice production of 5 percent over the period 1985–95. Yield increases accounted for about 57 percent of increased rice production, improvements in cropping intensity for 38 percent, and their interaction for 8 percent. However, the area planted in rice fell, and contributed to a 4 percent decline in rice production.

Between 1989 and 1995, real prices for paddy and rice declined annually by 4.3 percent, but price variability also decreased. The decline in real prices was primarily the result of high domestic inflation rates and an annual average decrease of 12.5 percent in the real exchange rate. Rice exports nevertheless increased at an annual rate of 8.4 percent during the same period (Table 11.1). The IFPRI research team estimated that, had the real exchange rate been stable, price incentives for rice producers would have increased 7 percent per year instead of declining. The IFPRI research team conveyed this finding to MARD to assist it in its dialogs with the Ministry of Finance (MOF) and the Ministry of Trade (MOT) regarding the pervasive effect of macroeconomic adjustments on the rice sector.

The overall socioeconomic environment in Vietnam in the years preceding the mid-1990s can be characterized as dynamic. The rice sector was flourishing in terms of production and productivity, in spite of unfavorable price trends. Incentives for private enterprise in agriculture were having significant effects on rice production and trade. Thus, the IFPRI research program was operating in an emerging or transition economy rather than in a command economy, and policymakers were receptive to research insights that addressed the policy environment surrounding a strategically and economically important food crop.

The government of Vietnam had clearly initiated a rice export strategy well before the economics research program on rice markets was initiated by IFPRI. Yet there was concern about continuing declines in paddy prices for farmers, poverty and food insecurity, interregional and rural-urban disparities, and linkages with rice exports (especially in the context of food security concerns).

Research Process and Policy Responses

In 1995, the Asian Development Bank (ADB) funded a study of rice market monitoring and policy options to be implemented by IFPRI. The project formally ended in March 1997. The objectives of the project were:

- To undertake an in-depth investigation of rice marketing, processing, storage, and trade.
- To analyze the structure of incentives, including the impact of existing interventions.
- To assess the impact of reforms on farmers, processors, traders, exporters, and consumers.
- To prepare rice policy options for implementation by the government.
- To develop a database on key rice market indicators.
• To provide training to the staff of relevant government agencies in statistical sampling, survey design, methods, data processing, and economic policy analysis.

The project’s overriding goal was to develop an understanding of the operations of both the domestic and international rice markets in Vietnam among policymakers. Little research had been done on Vietnam’s rice markets prior to the IFPRI study, and the government was eager to examine policy options related to such issues as decentralization, infrastructure, marketing costs, deregulation, credit, technology, stocks, price stabilization, and input markets. A major concern was to assist the government in making the transition from direct quantitative or fiscal interventions in the rice market to a more market-oriented profile and to further facilitate the policy dialogue between the ADB and the government of Vietnam. MARD and IFPRI were the primary collaborators.

The project implemented a “structure, conduct, and performance” analysis of the rice market and the findings informed the policy process in two ways. The first was to use data collected in an extensive national survey to describe current rice marketing channels, their costs, and constraints. The second was to construct a Vietnam agricultural spatial equilibrium model (VASEM) to examine policy options that would improve the functioning of the rice market and increase economic welfare.

An extensive series of seminars, workshops, training courses, working papers, and research reports were planned at the outset to ensure that information would find its way to the intended audiences. In late 1995, the economics research team began to communicate initial survey and analytical findings. Between October 1996 and April 1997, three workshops were held in Vietnam involving 144 participants. Two were on methodology and one on policy. Ten project-related seminars, attended by 331 participants, were also held in Vietnam for individuals from government ministries, research institutions, and universities.

More than 20 reports, papers, and training manuals relating directly to the rice policy project were prepared. One of the most comprehensive was the final report to the ADB (IFPRI 1996). This report described the methodology; the background to the study; the structure, conduct, and performance of the internal and external rice markets; domestic rice production and postharvest trends; rice competitiveness and food security; and the likely effects of various policy options on social welfare. It was translated into Vietnamese in March 1997 to ensure that the partners in the project received full benefit from the work.

The project resulted in the following 13 specific policy recommendations (IFPRI 1996):

1. Progressively increase the rice export quotas until they are no longer binding.
2. Substitute current quotas with export taxes.
3. Give private sector access to rice exports.
4. Dismantle internal policy restrictions on rice movement and freely allow internal trade.
5. Promote rice exports with measures to improve rice quality, reduce shipping costs, and improve Vietnam’s reputation among foreign buyers.
6. Provide access to credit to marketing agents to facilitate procurement operations, storage activities, and investments in processing and transport.
7. Provide access to information on prices, food production, international markets, and the marketing system to a variety of marketing agents, both public and private.
8. Provide a stable and credible policy environment.
9. Monitor macro policies to ensure that exchange rate depreciation does not penalize farmers.
10. Target food security stocks and distribution to food-insecure households.
11. Target investments in agricultural research to increased yields.
12. Target agricultural research to improve rice quality.
13. Invest in postharvest technology.

The chronology of major rice policy changes in Vietnam since 1992 (when IFPRI first initiated contact) and the activities of IFPRI are juxtaposed in Table 11.2.

Although Vietnam had embarked on a policy of increased rice exports prior to the IFPRI involvement, a figure of 2 million tonnes appears to have been viewed as an upper limit on exports, given policymakers’ concerns about food security. In late 1996, when IFPRI began to communicate the results of its rice trade policy research, Vietnam was experiencing a period of falling rice prices that led to farmer agitation in the south. As a result, intense discussions took place within and among ministries and the Government Office about price policy, exports, and internal trade. In the discussions, MARD was a major protagonist for liberalization, and it was IFPRI’s formal collaborator in the rice policy project. MARD made direct use of the emerging results of the joint study, especially those that indicated that Vietnam could export up to 5 million tonnes of rice without impairing food security or exacerbating poverty and with considerable benefit to farmers.

Rice exports increased to 2.9 million tonnes in 1996, despite a de jure quota of 2 million tonnes at the beginning of the year (Table 11.1). No crisis resulted and, in early 1997, the prime minister raised the quota to 3.5 million tonnes, removed the state trading enterprise’s rice export monopoly, lifted internal trade restrictions on rice, dropped licenses and controls on transport, and removed wholesale taxes on food. MOT and MARD were given the joint responsibility to regulate rice exports.

In early 1998, the quota was further raised to 4 million tonnes, and private-sector participation in rice exports was allowed. In June 1998, the government
TABLE 11.2 Chronology of rice policy decisions by the government of Vietnam and IFPRI involvement

<table>
<thead>
<tr>
<th>Year</th>
<th>IFPRI activities</th>
<th>Government decisions</th>
</tr>
</thead>
<tbody>
<tr>
<td>1992</td>
<td>Initial contact with VASI director in France; suggestion that IFPRI undertake research collaboration; planning for IFPRI senior staff visit to Vietnam</td>
<td>Rice exports at 1.9 million tonnes</td>
</tr>
<tr>
<td>1993</td>
<td>IFPRI's MSSD director visits Vietnam; establishes formal links with government; recommends visit by director general</td>
<td>Land Reform Resolution No. 5 (five rights) promulgated Rice exports at 1.6 million tonnes</td>
</tr>
<tr>
<td>1994</td>
<td>IFPRI director general leads delegation to Vietnam; introduces IFPRI's program to ministries and research organizations; explores scope for collaboration</td>
<td>Passive government response to IFPRI visit Rice exports at 1.9 million tonnes</td>
</tr>
<tr>
<td>1995</td>
<td>IFPRI commences study on property rights to land with VASI under multicountry study; IFPRI/DAI submit proposal for rice market monitoring and policy options study to ADB in May, following invitation to submit via competitive process; funding approved in July, project commences in September, surveys begin in December</td>
<td>Rice exports at 2.0 million tonnes High world prices for rice; rice trade very active; government imposes controls on domestic trade because of concern about food security in the north; illegal rice flows to China</td>
</tr>
<tr>
<td>1996</td>
<td>Surveys continue to June; training courses conducted in January–March; study tour to Thailand undertaken in June; analytical work undertaken in July–September; presentation of results at workshop in October; seminars around country before and after completion of the analysis</td>
<td>Intense discussions in many government forums, stimulated by the IFPRI studies, led to reevaluation of rice policies Crisis in May–June, after main April harvest led to significant price falls; farmers complain to provincial leaders in the south; minister of agriculture and rural development visits the south to review the situation, promising farmers price support; intense discussions in December involving MARD, Government Office, SOE, MOT, MPI, MOF, Government Price Committee, and provincial</td>
</tr>
</tbody>
</table>

(continued)
### TABLE 11.2 Continued

<table>
<thead>
<tr>
<th>Year</th>
<th>IFPRI activities</th>
<th>Government decisions</th>
</tr>
</thead>
<tbody>
<tr>
<td>1998</td>
<td>New study on starch industry development with PHTI and CIAT, funded by IDRC</td>
<td>leaders over rice price policy and exports; MARD was the main protagonist for liberalization, using IFPRI study as key input</td>
</tr>
<tr>
<td></td>
<td>UNDP project on diversification and poverty mapping completed and seminar held in March</td>
<td>Exports reach 2.9 million tonnes without a crisis</td>
</tr>
<tr>
<td></td>
<td>Decision for IFPRI Board of Trustees meeting to be held in Hanoi, February 1999, and symposium on food policy in Indochina jointly with MARD</td>
<td>New decree by prime minister, raising rice export quota to 4.0 million tonnes and providing for private-sector participation</td>
</tr>
<tr>
<td></td>
<td>Food processing study conducted in April for IDO, involving rice, coffee, seafood, fruits, and vegetables</td>
<td>Four private companies apply for export licenses in March; no licenses yet issued</td>
</tr>
<tr>
<td></td>
<td>Trade incentives and constraints study conducted for World Bank in May</td>
<td>Government curbs further exports in June after rate exceeds expectation at 2.5 million tonnes; domestic prices very high</td>
</tr>
<tr>
<td></td>
<td>Impact study conducted in July-September.</td>
<td>Rice exports expected to reach 3.8 million tonnes</td>
</tr>
</tbody>
</table>

**NOTES:** ADB, Asian Development Bank; CIAT, Centro Internacional de Agricultura Tropical; DAI, Development Alternatives Inc.; IDRC, International Development Research Centre; MARD, Ministry of Agriculture and Rural Development; MOF, Ministry of Finance, MOT, Ministry of Trade; MPI, Ministry of Planning and Investment; MSSD, Markets and Structural Studies Division of IFPRI; PHTI, Post-Harvest Technology Institute; SOE, State-Owned Enterprise; UNDP, United Nations Development Programme; UNIDO, United Nations Industrial Development Organisation; VASI, Vietnam Agricultural Science Institute.

curtailed further exports, because there was concern that they were likely to exceed the announced annual quota. In subsequent years, the government has announced the annual quotas in February or March, but it can modify them for food-security reasons in response to emerging trends. In 1998, a temporary correction was implemented in June, partly because of a drought in the north.

Currently, no trade is allowed in the export quotas, and the rice export tax is zero. However, MOF reserves the right to impose export taxes, depending on demand and supply. The creation of a market-monitoring system in MARD is consistent with the finding in the IFPRI study that price discovery by market
participants is rudimentary. This system tracks domestic, border, and international markets and prices for 10 agricultural commodities.

**Perceived Influence of the Economics Research**

Several conclusions and recommendations from the economics research appear to have been noted by policymakers. However, it would be heroic to assert that actual policy changes were the direct result of the IFPRI study’s influence. Other players were involved in the policy process. These included the Centre for International Economics, Canberra; the Centre for International Economic Studies, Adelaide; the Harvard Institute for International Development, Cambridge; Lincoln International New Zealand, Canterbury; and the National Centre for Development Studies, Canberra.

To relate the IFPRI team’s work to the policy changes that took place requires direct reference to partners and stakeholders. Their perceptions about the policy environment are critical in assessing whether the policy-oriented economics research made a difference in the policy process. In August–September 1998, I conducted a series of interviews with 35 persons who were either partners in the research endeavor (13 individuals) or stakeholders in the outcomes (22). The economics research team and other actors in the policy environment compiled the initial list of interviewees. In some cases, interviewees themselves suggested further names, which were added to the list. I conducted most interviews in person, but a few were conducted by phone.

The interviews were guided by a list of 38 potential questions drafted after review of the documentation of the study and discussions with the primary research project staff. The questions covered capacity building and training, policy environment, demand for IFPRI involvement, communication of results, policymaking impact, and new information and insights.

At the start of each interview conducted in Vietnam, an IFPRI staff member introduced me to the interviewee and gave a brief background of the reasons for the impact study, to validate my credentials. The staff member then left the room and the interview was conducted in confidence. An interpreter was present in 23 interviews of Vietnam officials. The interpreter was from the Department of Science and Technology of MARD. While this facilitated discussions, the interpreter may have exerted unintentional influence on the interviewees. No information is available on biases engendered by these potential effects.

The evidence from the surveys indicates that the IFPRI study is perceived as providing original insights into Vietnam’s rice sector. Prior to the study, there was neither a detailed understanding of the sector, nor basic information on such aspects as trade flows, marketing channels and margins, costs of production, price differentials within and outside the country, and transport costs. By combining primary and secondary data in standard “structure, conduct, and performance” analysis, the economics research team was able to illuminate the
policy environment. This apparently set the stage for an audience receptive to the study's policy results from the later modeling work; that is, in Bayesian fashion, it changed the policymakers' priors about the potential relevance of the research.

Many decisionmakers stressed the importance of the surveys and the primary data analysis that followed in establishing IFPRI's ability to provide credible policy analysis in Vietnam; that is, the study "changed the level of dialog in Vietnam" (see responses to question 1 in Table 11.3). The study's quantitative nature was regarded as "a first" and the use of the spatial equilibrium model VASEM to address key policy issues was viewed as powerful. Key decisionmakers in MARD requested the economics research team to examine many options because the quality of the data and the model gave them confidence. They became advocates for policy recommendations that emerged from the study, well before the publication of the final report in December 1996 (IFPRI 1996).

Within the context of Vietnam, the study's originality also derived from its use of competitive market economics to address policies. Many senior policy analysts in Vietnam were trained in the former Soviet Union. Younger analysts, who are now returning to Vietnam with master's and doctoral degrees from the United States, Europe, and Australia, viewed the IFPRI study as validation of their newly acquired skills.

Respondents strongly agreed that the IFPRI study influenced decisions about relaxing rice export quotas, involving the private sector in exporting, and removing internal trade restrictions (Table 11.3). No one claimed that IFPRI was the sole influence on these policy changes; rather, they saw the study as a key strategic input into a policy process with many actors and vested interests. The workshops and seminars conducted by IFPRI were regarded as crucial in building the consensus required before policy changes could be effected. No one policymaker or institution makes decisions in the government of Vietnam. Policy change is rather a consequence of a diffuse mechanism. IFPRI's independence, the quality of its research, and its extensive communications facilitated the achievement of consensus on these policy issues. In addition, the advice was timely. Many respondents indicated that the changes would not have occurred as quickly in the absence of the economics research.

Several interviewees indicated that the choice of MARD as the project partner facilitated any subsequent influence on policy processes and policy formulation. One alternative could have been for IFPRI to collaborate more directly with such research institutions as the Vietnam Agricultural Science Institute, the Institute of Agricultural Economics, or the National Economics University. This might have enabled the research and databases to be better institutionalized than was apparently the case. However, these research institutions are not explicitly integrated into the decisionmaking process, and any such gains might have occurred at the expense of direct policy impacts.
TABLE 11.3 Summary of partner and stakeholder interview responses

<table>
<thead>
<tr>
<th>Question or issue related to IFPRI study</th>
<th>Number of responses</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Positive</td>
</tr>
<tr>
<td>1. Did aspects of study surprise or give original insights?</td>
<td>17</td>
</tr>
<tr>
<td>2. Policies on rice export quotas and increased private-sector role influenced</td>
<td>16</td>
</tr>
<tr>
<td>3. Policies on rice export quotas and relaxation of internal trade controls occurred earlier than otherwise</td>
<td>14</td>
</tr>
<tr>
<td>4. Training was effective</td>
<td>10</td>
</tr>
<tr>
<td>5. Overall assessment of study excellent</td>
<td>9</td>
</tr>
<tr>
<td>6. Data and recommendations used by government of Vietnam</td>
<td>8</td>
</tr>
<tr>
<td>7. Conclusions on links between rice export quotas and poverty/food security influential</td>
<td>8</td>
</tr>
<tr>
<td>8. Expressed demand for further IFPRI policy research</td>
<td>5</td>
</tr>
<tr>
<td>9. Policy on rice export tax influenced</td>
<td>5</td>
</tr>
<tr>
<td>10. Policy research capacity sustainably strengthened</td>
<td>5</td>
</tr>
<tr>
<td>11. Policies on targeted programs for poor influenced</td>
<td>4</td>
</tr>
<tr>
<td>12. Conclusions on importance of exchange rate and monetary policies on rice competitiveness influential</td>
<td>4</td>
</tr>
<tr>
<td>13. Policy on credit availability influenced</td>
<td>4</td>
</tr>
<tr>
<td>14. Data and recommendations used by stakeholders</td>
<td>2</td>
</tr>
<tr>
<td>15. Is VASEM model still being used?</td>
<td>2</td>
</tr>
<tr>
<td>16. Press coverage good</td>
<td>1</td>
</tr>
<tr>
<td>17. Institutionalization good</td>
<td>1</td>
</tr>
<tr>
<td>18. Conclusions on effects of rice export quotas on domestic price stability influential</td>
<td>0</td>
</tr>
</tbody>
</table>

Note: VASEM, Vietnam agricultural spatial equilibrium model.

Training was regarded as a strong feature of the project (Table 11.3). No one was negative about it, but there was almost universal agreement that much more was required and that it should be conducted continuously. Many respondents indicated that the study was excellent in both content and quality, and that the government was making extensive use of the research.

Respondents indicated that one of the more influential aspects of the study showed the effects of relaxing rice export quotas on poverty and food security (Table 11.3, item 7). Apparently, in addition to changes in export quotas, substantial investments in additional rice storage to accommodate the expected growth in stocks were also being considered at the end of 1996. Indeed, these were even seen to be policy alternatives. At the time, issues related to possible price rises and food-security effects were paramount in MARD discussions.
The IFPRI conclusion—that although price rises were likely to occur if quotas were increased, they would not compromise the food security or welfare of the poor—had a significant effect on the government's policy decisions. Provisions were made to increase the wages of government employees in urban areas if necessary to compensate them for possible increases in the price of rice. To ensure a smooth transition, the decision was taken to relax rice quotas gradually.

Respondents provided only weak support for the hypothesis that evidence from the study about rice export taxes and targeted programs for the poor was influential in effecting change (Table 11.3, items 9 and 11). Rice export taxes have been revised up and down over recent times and currently the rate is zero. There does seem to be greater attention to deficit regions to ensure that rice supplies are in stock and special programs for the poor have been strengthened.

**Decisionmaking Processes**

MARD is the ministry with primary responsibility for rice exports and price policies. However, because rice exports involved economy-wide policy issues, both MOT and MOF are also key actors in determining policy (Figure 11.3). In MARD, the director of the Department of Agricultural and Rural Development Policy was the key advocate for change. MARD and MOF were both concerned about the power wielded and economic rents earned by the state trading enterprise from the rice export monopoly. The Ministry of Planning and Investment (MPI) was also involved in the decisions, as rice is important in the economy. In MPI, the Central Institute of Economic Management was a key player, because if its role in Vietnam's economy-wide reform process.

International stakeholders such as ADB and the World Bank played an indirect role in the processes underlying rice policy. They also made use of the IFPRI study. Whether their leverage was instrumental in directly affecting the nature and timing of policy innovation is unclear.

The Government Price Committee was also an important player, because in the Government Office structure, it provides a conduit between the ministries and the Prime Minister's office. Committee members were informed of the study and participated in the seminars and workshops.

Many other domestic stakeholder institutions, including the rice State Trading Enterprise, were associated with the IFPRI study, either directly, as collaborating partners (for example, the National Institute of Agricultural Planning and Projection), or as peers or mentors (for example, the universities).

All of the stakeholders identified in Figure 11.3 were engaged in the study through the series of seminars and workshops presented by the economics research team as results were emerging. Copies of the final report to the ADB both in English (December 1996) and Vietnamese (March 1997) were provided (IFPRI 1996). Thus, as in other cases (for example, Islam and Garrett 1997), the research had an impact well before formal publications of the study appeared.
FIGURE 11.3 Decision processes in rice policy changes in Vietnam


This section provides estimates of the benefits to Vietnam of the economics research team’s work. The institution responsible for the work, IFPRI, has a reputation for contributing to international social scientific knowledge through quality assurance of independent peer reviews. Much of IFPRI’s output consists of public goods at the basic-strategic end of the research spectrum. Thus, the research team in Vietnam was viewed as a credible source of reliable information, even though, as discussed earlier, other groups also served as suppliers of social science and policy research and advice to Vietnam.

Many of the policymakers interviewed about the process of changing rice policy stated clearly that IFPRI’s economics research facilitated decisionmaking (Table 11.3); some estimated that the time needed to formulate policy changes was reduced by from at least six months to more than two years (see responses to questions 4 and 18, respectively). Unfortunately, only a few respondents were prepared to provide such quantitative estimates of the time saved, so statistical measures of dispersion and central tendency could not be derived. Instead, the range of estimates provided by the interviewees is used to establish conservative (one year) and optimistic (two years) estimates of IFPRI’s impact on speeding up rice export reform in Vietnam. Applying these measures to the estimated increases in Vietnam’s national income from the policy changes (derived from the VASEM spatial equilibrium model of Vietnam’s rice markets) provides measures of the economic benefits from the role played by IFPRI. Note that not all benefits from rice policy reform are attributed to IFPRI; only those that occurred earlier than would have been the case without IFPRI’s research efforts. Note also that it is implicitly assumed that the model simulations correctly portray the efficiency gains and distributional outcomes from policy changes. In truth, this is unlikely to be completely realistic, but substantial efforts were made during the research process (via the workshops and seminars) to assess the validity of the model’s predictions.

Table 11.4 presents empirical estimates of the economic value of the rice policy changes effected by the government of Vietnam itself and the contributions of the economics research to rice policy formulation in Vietnam. The estimates were generated using the VASEM model under the two alternative assumptions about IFPRI’s impact on the timing of the policy change. Row 5 presents the estimated benefits of the export quota changes that actually occurred, calculated as the difference in annual national income between the no policy change/policy change scenarios in Table 11.4. The conservative (one-year) view of the impact of IFPRI on these changes is shown in row 6 as the difference in national income streams between these two scenarios for a one-year period. Similarly, the optimistic (two-year) view is presented in row 7. To each of these benefit streams is added the benefit from relaxation of internal trade
TABLE 11.4 Value to Vietnam of IFPRI research on rice policies

<table>
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<tr>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Without policy change</td>
<td>2.5(^a)</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
</tr>
<tr>
<td>2. At actual levels with IFPRI</td>
<td>2.5</td>
<td>3.0</td>
<td>3.6</td>
<td>3.8</td>
<td>4.0</td>
<td>4.0</td>
<td>4.0</td>
</tr>
<tr>
<td>3. At delayed levels conservative about IFPRI comparative advantage over alternative supplies</td>
<td>2.5</td>
<td>2.5</td>
<td>3.0</td>
<td>3.6</td>
<td>3.8</td>
<td>4.0</td>
<td>4.0</td>
</tr>
<tr>
<td>4. At delayed levels more optimistic about IFPRI comparative advantage over alternative suppliers</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>3.0</td>
<td>3.6</td>
<td>3.8</td>
<td>4.0</td>
</tr>
</tbody>
</table>

Benefits and costs (U.S.$ million)\(^b\)

<table>
<thead>
<tr>
<th>Benefits of policy changes(^c)</th>
<th>0</th>
<th>16</th>
<th>54</th>
<th>60</th>
<th>66</th>
<th>80</th>
<th>80</th>
</tr>
</thead>
<tbody>
<tr>
<td>Conservative value of IFPRI role(^d)</td>
<td>0</td>
<td>16</td>
<td>35</td>
<td>0(^e)</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>More optimistic value of IFPRI role(^f)</td>
<td>0</td>
<td>16</td>
<td>54</td>
<td>36</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Cost of IFPRI research</td>
<td>0.18</td>
<td>30.55</td>
<td>0.13</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
</tbody>
</table>


\(^a\)The de jure quota was set at 2.0 million tonnes. Informal exports to China, however, were estimated at 0.5 million tonnes in 1995; hence the base case was set at 2.5 million tonnes, which amounts to an even more conservative estimate of benefits.

\(^b\)Benefits include those from relaxing both export quotas and internal trade controls on rice; the latter occurred in 1997 in scenario 1 and is lagged one and two years in scenarios 3 and 4, respectively. Only benefits accruing to Vietnam are shown; those to the rest of the world are excluded.

\(^c\)Measured as the difference between national income estimates generated from VASEM under scenario 2 minus scenario 1.

\(^d\)Measured as the difference between national income estimates generated from VASEM under scenario 2 minus scenario 3.

\(^e\)As actual exports under scenario 2 approach 4.0 million tonnes, the differences in national income estimates between scenario 2, on the one hand, and scenarios 3 and 4, on the other, disappear. This is because under the large-country assumption, as Vietnam approaches the free-market level of exports, the gains from liberalization are offset by the losses from lower world prices. The benefits of export liberalization are underestimated (overestimated) to the extent that the assumed elasticity of export demand \((-12)\) is too small (large) in absolute value.

\(^f\)Measured as the difference between national income estimates generated from VASEM under scenario 2 minus scenario 4.
restrictions, which are estimated to begin in 1997 in the optimistic scenario, in 1998 in the “best guess” scenario, and in 1999 in the conservative scenario.

In the conservative scenario, IFPRI’s contribution to the income gains to Vietnam all occur in 1997. In the optimistic scenario, they occur in 1997 and 1998. The peak annual value is U.S.$54 million in the optimistic scenario and U.S.$35 million in the pessimistic case. The benefits to Vietnam from the two policy changes (export quota reforms and relaxing internal trade restrictions) of course continue for as long as the policies remain in place. These stabilize at U.S.$80 million annually beginning in 1999.

Present values of the benefit streams and benefit-cost ratios attributable to IFPRI’s research under the three scenarios are depicted in Table 11.5. If we truncate these present values in 1997 to reflect only those realized by that date, the most conservative estimate of IFPRI’s contribution to Vietnam has a present value of U.S.$45 million, implying a benefit-cost ratio of 56. If we use the more optimistic assessment of IFPRI’s role and truncate at 1997, we obtain a present value of U.S.$61 million and a benefit-cost ratio of 77. Allowing the benefit streams to play out until 2000 does not increase the conservative present value. For the more optimistic scenario, the present value increases to U.S.$91 million and the benefit-cost ratio increases to 114 when calculated to 2000. The estimated present value to Vietnam of the two policy changes is estimated at U.S.$222 million up to 2000. This estimate increases to almost a billion dollars if the reformed policies remain in place until 2020.

The interviews in Vietnam provided credible evidence that, beginning in 1996, the economics research affected rice export policies. However, because the research project did not formally end until early 1997, the earlier attribution might be viewed as heroic. Although it seems clear that the project had impacts prior to its conclusion, an alternative set of estimates was developed under the assumption that IFPRI did not affect policy changes until 1997. In these scenarios, the present values and benefit-cost ratios fell between 18 and 23 percent.

The benefits presented in Table 11.4 that were derived from VASEM exclude any gains accruing to the rest of the world from increased rice exports from Vietnam. Vietnam is a low-cost producer of rice, and the rest of the world is a net importer from Vietnam. That country now represents some 20 percent of world rice trade, and the current VASEM uses a rest-of-the-world export demand elasticity of −12 for rice from Vietnam. If this estimate is correct, relaxing Vietnam’s rice export quotas would result in net welfare gains in other countries (as illustrated in Figure 11.1). Hence, the values presented in Table 11.4 underestimate the total international benefits from the policy adjustment.

Table 11.5 presents estimates of the present value of the benefits of policy reform and related benefit-cost ratios when net benefits to the rest of the world are included. Depending on the scenario, including international benefits from the policy changes increases these estimates by 34 to 84 percent. Vietnam’s share
TABLE 11.5 Benefits and costs of the IFPRI research on rice policies to Vietnam and the rest of the world

<table>
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<tr>
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<tbody>
<tr>
<td>Present value (1995 U.S.$ million)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Of policy changes</td>
<td>61</td>
<td>98</td>
<td>222</td>
<td>355</td>
</tr>
<tr>
<td>Conservative value of IFPRI role</td>
<td>45</td>
<td>72</td>
<td>45</td>
<td>82</td>
</tr>
<tr>
<td>More optimistic value of IFPRI role</td>
<td>61</td>
<td>82</td>
<td>91</td>
<td>149</td>
</tr>
<tr>
<td>Benefit-cost ratio</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Conservative value of IFPRI role</td>
<td>56</td>
<td>91</td>
<td>56</td>
<td>103</td>
</tr>
<tr>
<td>More optimistic value of IFPRI role</td>
<td>77</td>
<td>103</td>
<td>114</td>
<td>187</td>
</tr>
</tbody>
</table>

NOTES: IFPRI, International Food Policy Research Institute; ROW, rest of the world.

*a* Using a 5 percent discount rate.

*b* Figures in parentheses represent present values with benefit streams from policy changes continued to 2020.

Some 60 percent of Vietnam’s exports of rice are to predominantly low-income countries of Asia and Africa (Goletti and Minot 1997, 7–27). These countries stand to gain directly from the decline in world prices occasioned by expanded exports from Vietnam. However, lower world prices resulting from Vietnam’s expanded role in the export market also provide benefits to countries importing rice from other countries. To the extent that these are also low-income countries, the international benefits from Vietnam’s rice policy changes may also have tended to favor the poor. In 1992 to 1994, low-income countries represented only some 50 percent of total world rice imports (M. W. Rosegrant 2000, personal communication). Hence, even more affluent countries participated significantly in the gains from Vietnam’s increased role in the world rice trade.

The results from the VASEM simulations in Tables 11.4 and 11.5 can be combined with net benefit ratios from the household survey data used in the IFPRI research to assess the impact on poverty in Vietnam. Table 11.6 shows the number of people who move above or below the poverty line set at the income level equivalent to the 25th percentile in 1995. This amounted to U.S.$66 per year in 1992–93 terms. Overall, the two policy changes appear to have marginally increased the number of people falling below the poverty line. However, the increase represents less than 0.1 per cent of the population. The marginal increase in poverty appears to result from the decline in international prices with
TABLE 11.6 Effect of policy changes on number of individuals (thousands) moving above or below the poverty line in Vietnam

<table>
<thead>
<tr>
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<th></th>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Overall effect of policy changes</td>
<td>0</td>
<td>+46</td>
<td>+77</td>
<td>+63</td>
<td>+81</td>
<td>+66</td>
</tr>
<tr>
<td>Conservative estimate of IFPRI role</td>
<td>0</td>
<td>+46</td>
<td>-77</td>
<td>-24</td>
<td>-16</td>
<td>0</td>
</tr>
<tr>
<td>More optimistic estimate of IFPRI role</td>
<td>0</td>
<td>+46</td>
<td>+77</td>
<td>+134</td>
<td>0</td>
<td>+107</td>
</tr>
</tbody>
</table>

NOTES: A positive figure implies an increase in the number of people who fell below the fixed poverty line at the 25th percentile set in 1995. A negative figure implies a reduction in the number of people falling below the poverty line.

Increased exports from Vietnam in the model. This adversely impacts the larger number of poor farm households, offsetting the benefits to the smaller number of urban poor rice consumers. The conservative scenario on IFPRI's role suggests a small reduction in poverty, whereas the more optimistic scenario indicates a marginal worsening of poverty. The latter result is difficult to rationalize. Indeed, these findings differ from those obtained by Goletti and Minot (1997) with earlier versions of VASEM.

The primary implication of this poverty analysis is that, in the short term, the policy interventions by the government of Vietnam may have had a neutral effect on the numbers in absolute poverty, or, at worst, had a very small adverse effect. However, there may be long-term benefits to the poor through the contributions of these policies to economic growth and its well-documented effects on poverty alleviation (Deninger and Squire 1996; Roemer and Gugerty 1997; Bruno, Ravallion, and Squire 1998; Dollar and Pritchett 1998).

Conclusion

Policy-oriented economics research may have important effects on the timing of policy reform. This study has shown that such effects can be quite large and represent substantial returns on resources allocated to policy-oriented social science research. Partial equilibrium models, which provide estimates of the economic gains from policy changes, can be combined with benefit-cost analysis to estimate the economic value of impacts on policy timing. This case study represents one possible methodology for addressing the issue of how to attribute policy-decision outcomes to one specific economics research program. Much more needs to be done on the methods of attribution for all the players influencing policymaking. Conducting extensive interviews with collaborators, stakeholders, and decisionmakers is one way to determine whether and how the economics research influenced policy decisions. Experience with the case study described in this chapter suggests there is scope to improve on the survey and interview techniques to better ensure objectivity.
Finally, note that the estimates of the benefits from the policy changes in Vietnam and the contribution of the economics research are both derived from the same model that was used to provide the advice to Vietnam in the first place. As Krugman (1997) and Timmer (Chapter 6 in this volume) point out, there is a circularity about this type of impact assessment that tends to undermine the credibility of the assessment. However, this sort of tautological dilemma is almost impossible to avoid, especially if the research is done well in the first place.

References


Economists have provided intriguing and useful information on the value of public research in natural science and agriculture (for examples, see Huffman and Evenson 1989; Alston and Pardey 1996). But less is known about the value of economics research in agriculture. This chapter provides a perspective for the study of this topic.

We begin by placing the products of economics research in three groups—new economic information, products contributing to technological change, and products contributing to policy—each of which has its own users, impacts, and benefits. We argue that the effects of research depend on the transmission of results and the capacity of users to take advantage of them. Thus the productivity of economics research depends on the quality of extension and economic literacy. We then develop a quantitative framework for assessing the benefits of an economics research program and key issues associated with implementation. We present alternative approaches to reduce the effort required to assess the impact of research. One way to do this would be to concentrate on a small number of successful programs that capture most of the benefits. Because estimates of the effects of research are shrouded in uncertainty, we suggest methodologies to communicate the magnitude of this uncertainty. The attribution of benefits to specific projects is especially difficult, so it is important to develop sound documentation and testimony to link economics research projects to their alleged effects. Some examples based on case studies illustrate how to overcome the more problematic issues in the empirical evaluation of economics research benefits.

Research on the productivity of social science research in agriculture is in its infancy, so the emphasis in this chapter is not on providing exact formulas or rigorous procedures. Instead we identify the most problematic methodological aspects of the evaluation of economic benefits, and then we suggest how to address them. Economists may use estimates of the productivity of economics research to affect decisions made about the allocation of resources to agricultural economics, but this information should also be used to assess the productivity of various lines of research within agricultural economics and to allocate
resources among them. The productivity of research in the field strongly depends on how the research results are distributed. Efficient strategies for developing research programs may need to be interrelated and to be dependent on investment in education and the dissemination of results. Finally, we highlight the problematic nature of having economists quantify the effects of their own research. That creates problems of moral hazard and credibility. It is not enough to convince economists of the value of economics research; the results of that research must be effectively communicated to noneconomists. Credible analyses of the impact of research must rely on independent sources and objective documentation as well as rigorous methodology.

**Classifying the Effects of Economics Research**

Studies on the productivity of research have emphasized the effects it has through embodied innovations—that is, through new products. But research also leads to disembodied innovations, such as new practices and management strategies. Both embodied and disembodied innovations have "public goods" properties and are likely to be underprovided by the private sector. Thus research supported by the public sector should lead to new knowledge and information and produce disembodied innovations.

Some of the disembodied innovations produced by economics research are institutional innovations that provide new organizational structures to address social, economic, and environmental problems. Some are managerial and decisionmaking innovations that improve the choices made by firms, consumers, and the public sector. The outputs of economics research are diverse; to avoid omission and to develop the appropriate arsenal of tools to assess their benefits, it is useful to classify them. In Table 12.1, the outputs of economics research are divided into three classes and nine categories. The first class, economic information, includes prices and quantities, institutions and policies, and aggregate information. The second class includes three categories of outputs that contribute to technological change: innovations in production and management, product introduction and marketing methods, and management tools for research and development. The third class includes three categories that affect public policies: policy paradigms and institutional innovations, policy analysis tools, and assessments of the impact of research on policy.

This classification scheme shows the diversity of the results of economics research. It suggests that an effort to estimate those benefits correctly must be ambitious. As Table 12.1 demonstrates, the categories of research output vary both in who uses a specific output and in the type of benefits it generates. Many categories may require the development of specific analytical tools to quantify their benefits. The points below provide insight into an evaluation of the benefits obtained from the output of research.
First, note that very few quantitative studies of benefits of economics research exist. The only category of outputs of economics research for which such estimates have been made is data on prices and quantities. The Data Task Force of the American Agricultural Economics Association documented some of the benefits associated with price estimates provided by the U.S. Department of Agriculture (USDA) and argued that the value of these benefits significantly exceeded the cost of generating this information (American Agricultural Economics Association 1996). It is important to develop and apply methodologies to assess the benefits from the other eight categories.

Second, some economics research benefits may be misattributed to biological and physical research in agriculture. There are two possible sources for such misattribution of benefits. Economics research has facilitated the adoption of many innovations by improving the means for identifying niches for new technologies and contributing to the design of effective strategies for marketing and education. In addition, results have been internalized in technologies that are promoted by members of other disciplines. For example, nutritionists promote feed-rationing formulas that may have originated in the optimal-diet studies of Stigler (1945), and research on farm and agribusiness management has resulted in software and other decision tools that have been used by resource and production managers to increase the efficiency of land allocation and food distribution. These contributions have been attributed to other categories.

Economics is also a beneficiary of the results of research in other disciplines (e.g., mathematics, statistics, computers, sociology). This has to be recognized in any assessment of the contributions of economics. However, the presumption here is that agricultural economics has been a net exporter of methods.

Third, an assessment of research benefits is useful in determining research incentives and allocating resources within disciplines, including economics. The work of economists demystified the process of scientific discovery, showed that it is affected by incentives, and led to the introduction of more formal mechanisms for managing research (Binswanger and Ruttan 1978). There is a growing tendency among the organizations that sponsor research (for example, CGIAR, BARD, NSF) to emphasize incentive-based programs, to target research areas with the highest return, and to evaluate research performance periodically using economic criteria.

Furthermore, it can be useful to assess the productivity of different lines of research within disciplines in order to improve research management. In particular, it is important to assess the comparative benefits of various research lines in economics and agricultural economics. We economists have to practice what we preach.

Fourth, rigorous economic modeling has led to many institutional innovations and policy paradigms. Economics is a young discipline, only about 300 years old. For much of its life, the aim of theoretical research was to refine and
<table>
<thead>
<tr>
<th>Category</th>
<th>Prices and quantities</th>
<th>Institutions and policies</th>
<th>Aggregate information</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Class 1: Economic information</strong></td>
<td>Commodity price predictions, output focus</td>
<td>Overview of international agricultural policies</td>
<td>Sectoral productivity estimates, sectoral accounts</td>
</tr>
<tr>
<td><strong>Examples</strong></td>
<td></td>
<td>Agribusiness, investors, exporters, developers</td>
<td>Policymakers, voters</td>
</tr>
<tr>
<td><strong>Users</strong></td>
<td>Traders, farmers, agribusiness</td>
<td>Increased trade and investment, reduced transaction costs, increased efficiency</td>
<td>Timely policy adjustments, informed political choices</td>
</tr>
<tr>
<td><strong>Benefits</strong></td>
<td>Uncertainty reduction, increased efficiency, higher average output, lower prices</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Class 2: Contributions to technological change</strong></td>
<td>Product introduction and marketing methods</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Examples</strong></td>
<td>Feed rationing formulas, resource allocation methods, conjunctive use of water, economic pest thresholds, location and transportation management tools</td>
<td></td>
<td>Competitive grants, research incentives, research assessment tools</td>
</tr>
<tr>
<td><strong>Users</strong></td>
<td>Resource managers, agribusiness, farmers</td>
<td>Agribusiness, investors, developers, farmers</td>
<td>Policymakers, research managers</td>
</tr>
<tr>
<td><strong>Benefits</strong></td>
<td>Improved resource allocation, increased profitability</td>
<td>Increased trade and investment, reduced transactions costs, increased productivity</td>
<td>Improved research productivity, improved research accountability</td>
</tr>
<tr>
<td>Category</td>
<td>Policy paradigms and institutional innovations</td>
<td>Policy analysis tools</td>
<td>Policy impact assessment</td>
</tr>
<tr>
<td>--------------------------</td>
<td>----------------------------------------------------------------------------------------------------------------</td>
<td>--------------------------------------------------------------------------------------</td>
<td>------------------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>Class 3: Contributions to public policy</td>
<td>Market efficiency, gains from trade, externalities, public goods, transferable pollution permits, incentive-comparative policies, privatization, intellectual rights</td>
<td>Cost-benefit analysis, input-output models, CGE models, impact assessment models</td>
<td>Analysis of alternative farm programs, prediction of economic impact of food stamp programs</td>
</tr>
<tr>
<td>Examples</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Users</td>
<td>Government, policymakers, citizens, public organizations</td>
<td>Government agencies, policy analysts</td>
<td>Government agencies, policy analysts</td>
</tr>
<tr>
<td>Benefits</td>
<td>Improved resource allocation, equity, and quality of life</td>
<td>Improved efficiency</td>
<td>Screen out bad policies, improve policy designs, identify unexpected consequences</td>
</tr>
</tbody>
</table>
generalize the basic insights of Adam Smith (on the social desirability of free markets) and David Ricardo (on the gains from trade) using more advanced machinery. Indeed, Arrow and Debreu received the Nobel Prize for identifying conditions under which a competitive equilibrium exists and is socially optimal. But the added rigor and advanced machinery expanded the capabilities of and led to a multitude of policy paradigms and institutions. For example, over the last 40 years, formal economic analysis has advanced the notions of externalities, public good, market failure, incentive-compatible policies, trading in pollution permits, intellectual property rights, and so forth.

Fifth, assessments of the impact of research are preventive and corrective. They are generally routine exercises, but they can require original research on methodology or empirical estimation. Such assessments can be especially valuable if they lead to the selection of superior outcomes. The most important contribution of this research may be preventive: it screens wasteful proposals. For example, the use of formal cost-benefit analysis to assess water projects in the United States has led to a significant reduction in new water projects and has prevented many pork barrel projects (Zilberman et al. 1994a). Assessment research can also identify the unintended consequences of proposed policies, possibly leading to changes in policy. Finally, it provides extra benefits by introducing alternatives for policy that would not have been considered. For example, Sunding, Zilberman, and MacDougall (1997) were asked to evaluate the cost of transferring water away from California agriculture to the environment. They recognized that the cost depended on whether it was permissible to trade in water. That helped to establish water trading as a key ingredient of water reform.

Sixth, timing is everything. Implementation may lag behind the publication of research results. As Rausser and Zusman (1991) suggest, the timing of policy reforms may depend on random events. In particular, policies and institutions may be changed during and after periods of crisis. A body of research could provide an intellectual foundation for new institutions and arrangements might have no observable impact until after a crisis. The major reforms in California’s water laws after the 1987–92 drought were based on concepts and ideas that had long been promoted by economists (Zilberman et al. 1994b).

Seventh, there is a difference between recommending a policy and implementing it. The benefits of may not be fully realized because a policymaker may either act against the advice of economists or ignore it. Even if policymakers do not act in accord with the recommendations of economists, they may claim that they follow economists’ advice or behave according to an economic principle due to concerns for their public relations. In some political situations, the results of potentially valuable policy research may be wasted.

Eighth, economists make mistakes. Incorporating their recommendations into policy does not necessarily ensure that the policy will succeed or that human well-being will improve. Economists have a track record of giving wrong advice. After all, many consider Marx to be an economist. Based on what at the
time were considered sound economic principles, South American economists followed development strategies that emphasized import substitution and led them nowhere (Prebish 1950). In other cases, ignoring or even acting against the recommendations of economists has produced successful outcomes. These cases cannot be ignored in an assessment of the value of economic research. Economists must attempt to identify those situations where economics has failed so that they can learn from these failures.

**Transmitting the Results of Economics Research**

The value of economics research that provides new knowledge to economic agents in either the private or public sectors has to be based on the transmission and interpretation of the research results to end users and the capacity of those users to employ that information. The availability and accessibility of research results to potential users depend on the effort made to transmit the information through extension and the communications media (including books for popular consumption, other print media, radio, and television).

Suppose research results have \( N \) identical potential users. Assume that the gains users receive from research depend on the amount of the research, the extension effort, and the education of the user. Let \( H \) be the amount of research (measured by research expenditure), and let \( X \) denote expenditures on the transmission of information through extension (and other means, which will not be described here in order to simplify the discussion). Let the education of the user be denoted by \( D \). It is reasonable to assume that, in most cases, the more education users have, the more they benefit from the given information. If \( G \) denotes the gain for an individual receiving research results, it can be presented as \( G = g(D, H, X) \), where \( g \) is the functional relationship between the gains from research, research and extension expenditures, and user education. The percentage of the population that receives the results of research is denoted by \( P \), and it is a function of expenditures on extension, \( P = p(X) \), where \( p \) is the functional relationship between the percentage of the population that receives research information and extension expenditures.

The gross benefit from research in this simple model is denoted by \( GB \), and is

\[
GB = N \times P \times G = N \times g(D, H, X) \times p(X).
\]  

Equation (12.1) suggests that society's gains from research increase as the number of potential users of the information expands, the information transmission system becomes more effective, and potential users become more educated and better able to use the information. The formulation in equation (12.1) implies that the information generated by economics research is a public good that can be used by many individuals simultaneously. This assumption is valid in many situations and suggests that the benefits of economics research
depend on the size of the affected industry or economy. The same research ef-
fort, ceteris paribus, may generate a greater benefit if it addresses problems of
the United States as a whole rather than just one state.

The net benefits from research, \(NB\), is equal to gross benefits minus the cost
of research and extension, and \(R\) is research expenditures:

\[
NB = N \times g(D, H, X) \times p(X) - R - X. \tag{12.2}
\]

Expenditures on research and extension (and, within a wider context, edu-
cation) are public policy choices. Strategies that maximize benefits from re-
search and extension allocate resources so that the marginal benefit of research
is equal to the marginal benefit of extension. Thus, there are situations in which
investments in extension and education are essential if society is to gain from
research. In other words, the returns to research are likely to be meager if there
are no effective mechanisms for transmitting results to the end users or if these
users do not have the education to take advantage of the information generated.
Thus, when a research program is established that generates economic informa-
tion, it should have effective mechanisms for disseminating its results and for
producing results that are useful to its designated clientele.

Equation (12.2) also emphasizes the importance of economic literacy. When
the potential users of economics research have strong backgrounds in
economics, they can use more powerful and refined economic concepts, which
may improve the quality of the results. Analysis of the optimal allocation of
resources between research and extension further suggests that increased eco-
nomic literacy is likely to reduce the need for extension and interpretation. Thus
improved economic literacy increases the net benefits from research by both in-
creasing the gross benefits from research and reducing the cost of extension. In
spite of the importance of basic economic concepts in everyday life (such as
discounting, trade-offs, and efficiency), most people do not get a decent eco-
nomic education in their primary and secondary schooling. The lack of economic
literacy may be the biggest obstacle to the productivity of economics research.
Furthermore, most economics courses in universities are designed to produce
economists rather than to provide individuals with a basic knowledge of eco-
nomics. Economists are challenged to develop academic programs that will
appeal to individuals in other disciplines.

Equations (12.1) and (12.2) emphasize the links among the creation, trans-
mission, and use of information and illustrate the interdependency among re-
search, education, and extension. But the model fails to capture the complexities
that affect the productivity of research. Individuals and organizations are het-
erogeneous, and even within one sector economic agents vary drastically in the
potential they have to gain from information, their access to it, and their capacity
to use it. Given the heterogeneity in economic literacy in the population at large
and among policymakers, it may be worthwhile to use several mechanisms to
communicate the results of the same research. To obtain the optimal impact, not
only the effort to transmit research but the formulation of research and the type of answer it provides should be adjusted to the capabilities and needs of the users. The message must be tailored to the client, not the messenger. Thus, broad and sophisticated research efforts may need to be sacrificed if the main objective is to maximize the immediate contribution of research to a real-world situation.

Economic agents use many types of economic information that they obtain from many sources. To assess the benefit of a specific research program, one has to consider it in that context. There are likely to be several providers of economic information, and the products of economics research may be either complements or substitutes. There are externalities between research projects, and the results of one may borrow from the processes and outcome of another. Thus, it may be difficult to pinpoint the impact of a particular research project. In some cases, it would be advisable to assess the benefits of lines of research that may consist of several interdependent studies rather than to attempt to estimate the benefits of each individual research project.

When several projects address the same problem independently, the one with the most significant impact might not be the one with the highest quality results. Instead, it may have the better program for outreach and transmission. The selection of channels for transmitting results depends on the potential users of information, their skills and backgrounds, and specific technological and institutional circumstances. Research results can be transmitted to users through several major mechanisms:

- **Print media.** This includes books and magazines for the general reader, scholars, or professionals and practitioners.
- **Electronic media.** These are radio, television, and the Internet. These media tend to be updated frequently. They may even be interactive.
- **Education.** This includes basic as well as college education, and professional degrees and training.
- **Extension.** This consists of tailor-made outreach programs such as demonstrations and short courses that are targeted and specific. In many cases, extension can include participation in the use of economic information.
- **Consulting.** Consultants are paid to conduct research and give advice on its implementation.

Table 12.2 shows how the classes of economics research results described in Table 12.1 can be transmitted to end users. Table 12.2 presents hypotheses that need to be tested empirically, but it can also be used as an instrument to think about the problem. Several points should be emphasized about transmitting the results of research that the table suggests.

First, the effectiveness of the channels of communication in transmitting research results varies by category. Formal economic education, for example, is probably most effective in communicating new policy paradigms and
<table>
<thead>
<tr>
<th>Classes of research outcomes</th>
<th>Print media</th>
<th>Electronic media</th>
<th>Education</th>
<th>Extension</th>
<th>Consulting</th>
</tr>
</thead>
<tbody>
<tr>
<td>Economic information</td>
<td>Impact</td>
<td>Primary</td>
<td>...</td>
<td>...</td>
<td>Secondary</td>
</tr>
<tr>
<td>Prices and quantities</td>
<td>Primary</td>
<td>Primary</td>
<td>...</td>
<td>...</td>
<td>Secondary</td>
</tr>
<tr>
<td>Institutional policies</td>
<td>Primary</td>
<td>...</td>
<td>Primary</td>
<td>Secondary</td>
<td>...</td>
</tr>
<tr>
<td>Aggregate information</td>
<td>Primary</td>
<td>Secondary</td>
<td>...</td>
<td>...</td>
<td>Secondary</td>
</tr>
<tr>
<td>Information for new technologies</td>
<td>Primary</td>
<td>...</td>
<td>Primary</td>
<td>Primary</td>
<td>Secondary</td>
</tr>
<tr>
<td>Management innovations</td>
<td>Primary</td>
<td>...</td>
<td>Primary</td>
<td>Primary</td>
<td>Secondary</td>
</tr>
<tr>
<td>Adoption research</td>
<td>Primary</td>
<td>...</td>
<td>...</td>
<td>Primary</td>
<td>Secondary</td>
</tr>
<tr>
<td>Research management</td>
<td>Secondary</td>
<td>...</td>
<td>...</td>
<td>...</td>
<td>Primary</td>
</tr>
<tr>
<td>Policy research</td>
<td>Primary</td>
<td>...</td>
<td>Primary</td>
<td>Secondary</td>
<td>Secondary</td>
</tr>
<tr>
<td>Policy paradigms and institutional innovations</td>
<td>Primary</td>
<td>...</td>
<td>Primary</td>
<td>Secondary</td>
<td>Secondary</td>
</tr>
<tr>
<td>Policy analysis tools</td>
<td>Primary</td>
<td>...</td>
<td>Primary</td>
<td>Secondary</td>
<td>Secondary</td>
</tr>
<tr>
<td>Policy impact studies</td>
<td>Primary</td>
<td>Secondary</td>
<td>Secondary</td>
<td>Secondary</td>
<td>Primary</td>
</tr>
</tbody>
</table>
The Value of Economics Research

institutional innovations. In the print media, books and magazines can effectively convey information about new policy paradigms, while technical and statistical bulletins are a major channel for communicating information on prices. The electronic media are becoming a major tool for transferring information on prices and quantities. They may become an important means for disseminating policy impact studies.

Second, economists can popularize economic concepts and paradigms in the print media. Milton Friedman’s articles in Newsweek have done an excellent job of educating the public about the ideas of Adam Smith and David Ricardo. Best-sellers written by distinguished economists such as John Kenneth Galbraith and Paul Krugman have familiarized policymakers with economic thinking. Increased economic literacy will improve the effectiveness of economics research, but effective popular communication by economists can achieve the same end.

Third, the economics profession should encourage a wide array of publications addressing economic issues with different degrees of sophistication. It is most prestigious to publish in technical journals that are accessible only to highly trained economists. But to have optimal impact, the results of economics research should be published in less technical and more policy-oriented journals that are accessible to economists in industry and even to educated laymen. Choices and the Journal of Economic Perspectives fill an important gap, and the introduction of other outlets that appeal to a broader audience will increase the productivity of economics research.

Fourth, the policymaking process determines the best way of communicating the results of policy impact studies. If decisions are made through popular ballots (the proposition to ban chemicals in California, for example), then the electronic media become important. If policy decisions are made in a legislature, where a modest number of decisionmakers interact, then in-depth reporting through print media may become more important. When there is a single decisionmaker, then a consultant may be the most effective way to transmit knowledge.

Fifth, there are both wholesalers and retailers of knowledge and information. Universities and organizations like the Economic Research Service of the USDA can generally be viewed as wholesalers of information, as they address a wide variety of topics from many perspectives. Organizations like consulting firms and specialized newspapers are information retailers; they sort and modify the results generated by wholesalers to fit the needs of specific clients, who are often individual decisionmakers with particular needs, willing to pay for a tailor-made product.

Sixth, the earnings of information retailers capture benefits that actually belong to the wholesalers. Because retailers rely on the information produced by wholesalers, and in many cases pay little for it because such information is a public good, their net earnings capture the benefits of publicly funded research. Thus, one indicator of the value of economics research is implicit in the
income of economic consulting firms (American Agricultural Economics Association 1996). Further research should be devoted to developing a mechanism to quantify this relationship.

Seventh, consulting provides public benefits. Consulting work can increase the productivity of economics researchers, both because it is an effective way to transmit knowledge and because it can add an element of realism to economics research.

Quantifying the Benefits of an Economics Research Program

As economics penetrates to the everyday life of research-funding organizations, there is a growing need to develop quantitative estimates of the benefits of agricultural economics research, especially research that is policy-related. This section develops a simple formula for quantifying the benefits of research and discusses how to address some of the thorny issues in applying it.

Policy generally affects economic systems by influencing a key variable or parameter (such as demand, supply, or the equilibrium of the market). Methodologies to assess the impact of policy change are well documented elsewhere and so will not be presented here.¹ Our emphasis here is on issues associated with evaluating a research program comprising many projects that may affect several policies over a long period of time. There are uncertainties about policy choices in general and the exact contribution of economics research to any change in policy in particular. The analysis and discussion below address these problems of dimensionality and uncertainty.

Assume that the research program of a group—be it USDA, the International Food Policy Research Institute, or a department in a university—consists of \( N \) identifiable research projects. Let \( i \) be an indicator of research projects that can assume values from 1 to \( N \). This program is assumed to affect \( M \) policy issues. Let \( j \) be the indicator of policy problems, so that \( j \) assumes a value from 1 to \( M \). The research project affects a decision about policy by generating information and knowledge that contribute to the selection of the policy chosen over other options. Suppose that in the case of a particular policy, \( j \), the number of policy options considered is \( K_j + 1 \). For the particular policy problem \( j \), let \( k \) assume values from 0 to \( K_j \), where \( k = 0 \) for the option chosen. It assumes values from 1 to \( K_j \) for the other policies considered in addressing policy problem \( j \).

It is unclear which policy option should be adopted to address policy issue \( j \) in the absence of the chosen policy (\( k = 0 \)). Because a policy is likely to

¹ For example, Lichtenberg, Parker, and Zilberman (1988) developed procedures to assess the impact of supply shifts because of policy changes, and Lichtenberg and Zilberman (1986) developed a framework for estimating the effects of supply shifts due to changes in supply in an agricultural industry subject to price supports. Alston and Pardey (1996) present many models and examples to assess the effects of different types of policy changes, so this aspect of the analysis can be taken for granted.
last for several years, let $P_{kjt}$ denote the probability that policy option $k > 0$ would have been implemented to address problem $j$ if the option $k = 0$ was not available. Let the difference of social welfare resulting from the use of policy option $0$ instead of $k$ at year $t$ be denoted by $\Delta B_{kjt}$. This difference in welfare is the aggregated change in the economic surplus associated with switching from policy option $k$ to $0$ at period $t$.

Let the year $0$ be the benchmark period for discounting benefits. In most cases, this will be when the evaluation is being conducted. The assessment of the benefits of research may be restricted only to those benefits that could be seen ex post, partly as a result of the policy research program. In this case, the time horizon for an assessment of benefits will be from $t = -T_p$ to $t = 0$ ($T_p$ denotes the number of past years considered in the analysis). However, the life of the policy option does not end with the assessment of benefits. Thus, it is also useful to consider the future benefits (ex ante) of selecting a policy option. In this case, the time horizon for the assessment may be from $t = -T_p$ to $t = T_f$ ($T_f$ denotes the number of future years of the analysis). One major difference between assessing benefits from the selection of a research option ex post and ex ante is that policy option $0$ was chosen precisely to address policy problem $j$. Thus,

$$\sum_{k=1}^{\infty} P_{kjt} = 1 \quad \text{for} \quad t \leq 0.$$ 

The probability that policy $0$ will not be chosen in the future is

$$1 - \sum_{k=1}^{\infty} P_{kjt} \quad \text{for} \quad t > 0.$$ 

Let $G_j$ denote the expected discounted net benefit of selecting policy option $k = 0$ to address policy issue $j$. Then,

$$G_j = \sum_{t=-T_p}^{T_f} \left( \frac{1}{1 + r} \right)^{t} \sum_{k=1}^{K_j} \Delta B_{kjt} P_{kjt}.$$ 

It is unreasonable to attribute the selection and enactment of a policy option entirely to a research project. Economists may conduct brilliant research and communicate it effectively to policymakers, yet other players in the political system may influence the selection of a recommended policy. Let $0 = S_{ij} < 1$ be the independent share of project $i$ in the solution to policy problem $j$. The economic benefits attributed to project $i$ will then be

$$\sum_{k=1}^{N} S_{ij} G_{ij}.$$ 

---

2. The contribution of several projects to a single policy solution may be dependent, but the constrictions of the shares should aim to avoid "double counting," and the sum of the shares will be equal to the overall shares of the research program. Thus individual shares can be treated as independent.
These are the expected net benefits of the project to all the policies it influences. The net economics of the research program are

\[ \sum_{j=1}^{M} \sum_{i=1}^{N} S_{ij} G_{ij}. \]

The formula for the net benefits of a research program seems straightforward, but obtaining actual estimates is an empirical challenge—for several reasons. Some of these obstacles and our suggestions to overcome them are presented below. They include the dimensions of the research program, uncertainties about the economic impact of individual projects, and difficulties in crediting the benefits of research.

### Dimensionality

The number of projects \((N)\) in a research program may be in the tens or even hundreds, and the number of policies each affects \((M)\) may be in the tens or hundreds as well. These effects may occur in several countries over different time periods. Therefore, quantitative estimates of \(G_{ij}\), the economic impact of one project on one policy, may be both time consuming and prohibitively expensive.

Fortunately, studies on the distribution of benefits and the effects of research programs suggest that these distributions are skewed. A small number of projects may account for most of the effects of a research program. Parker, Zilberman, and Castillo (1998) found that out of several hundred royalty-generating research projects at the University of California, the top two generated 70 percent of the technology transferred in 1994. This suggests that an assessment of the economic impact of a research program should concentrate on identifying the most effective research projects and assessing their benefits. The aggregate benefit of these projects provides a lower bound for the benefits of the program. Thus, let \(\delta_{ij}\) be an indicator, where \(\delta_{ij} = 1\) if the effect of a project on policy issue \(j\) is assessed, and 0 otherwise. A lower bound on the net expected benefits of a research program is

\[ \sum_{j=1}^{M} \sum_{i=1}^{N} \delta_{ij} S_{ij} G_{ij}. \]

A cherry-picking approach to estimating research benefits is to identify a small number of projects and policies that a priori seem to have the highest \(G_{ij} S_{ij}\) and estimate only their benefits. Three possible approaches to identifying productive research projects are presented here.

The first is to screen projects sequentially based on their assessed productivity. This procedure was applied by Just et al. in their study on the economic benefits of the United States–Israel Binational Agricultural Research and Development Fund (BARD) for the United States (Just et al. 1988). This binational fund supported 208 projects at the time of the review. Just and his colleagues used a two-step procedure to obtain a lower-bound estimate of the economic benefits.
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of the fund. First, they asked fund directors and senior staff to identify the projects with commercial potential. They identified 55. Then research proposals and final reports from each project were reviewed. The principal investigators were asked to provide basic information about the economic impact of innovations resulting from their research (which crops were affected and where, estimated yield increases, cost reductions per acre, and so forth) and the names of those who had adopted or were likely to adopt the innovations. This screening identified projects with the highest potential for generating benefits. The expected discounted economic benefits of these projects were estimated to have a value of $521 million, much more than the $90 million the fund distributed at the time of the review. The distribution of benefits was highly skewed even among the top 10 projects: the top two provided 60 percent of the expected benefits.

A second approach is to have individual researchers select a subset of successful projects. A research team would then screen these projects to identify the most productive ones. Their benefits would then be quantified. This procedure was used by Goldman, Shah, and Zilberman (1990) and McWilliams and Zilberman (1996) to evaluate the productivity of extension in two California counties. They asked the project leaders (farm advisors) to provide basic information on two of their projects with the greatest economic impact. This information included descriptions of the projects and their results, quantitative information on regions and populations that would benefit from the discovery, quantitative estimates of per-unit benefits (per acre, animal), and the names of individuals who could verify these claims. After the initial screening, the research team for each study identified fewer than ten projects with significant economic effects, and they quantified the benefits from these.

A third approach would be to make the initial selection based on the volume of service use. Where the amount of time allotted to each project or number of contacts with each client is documented, projects that require the most effort or service can be selected to have their benefits quantified further. This approach can be useful in assessing projects that generate economic information and predictions, as Parker et al. (1996) demonstrated in their study of the benefits of the California Irrigation Management Information System (CIMIS). The CIMIS directors provided a list of extensive users, and the research team interviewed these users to assess both the reductions in costs and the increases in revenue from the weather information provided by the system. Parker et al. estimated that the annual benefits to the subset of interviewed users were up to 26 times higher than the annual costs of running the CIMIS program. Furthermore, they discovered that the system generated unexpected benefits. CIMIS was designed to provide information to help farmers make decisions about irrigation, but some of the biggest users also used this information to make choices about pesticides, manage golf courses, and manage the water supply in urban areas.

A key feature that these three approaches share is the collection of evidence from users of the system. Estimates of benefits provided by project leaders should
be used only for the initial screening of projects and when corroborated by testimony from users or objective experts. In some cases, more than seven people were contacted to assess the benefit of a particular project. One common finding is that researchers were often unaware of the effects of their research. By relying on networks of informants, program assessors will get evaluations that are more complete and consistent.

These three selection procedures underscore how important it is to document research projects, their benefits, and their users. The cost, speed, and accuracy of research evaluation are significantly improved if documentation procedures are established and followed as part of the research effort. Outreach programs can make it possible to document the benefits from research projects after the research has been completed.

**Uncertainty about the Economic Impact of Individual Projects**

The net benefit of a research program is a weighted sum of the expected net benefits of the individual research projects that make up the program. These expected net benefits are not known with certainty. They have to be estimated, and the accuracy of their estimators is highly uncertain. The credibility of the assessment of a research program's expected net benefits is likely to increase if the analysis provides some measurement of the randomness of the $G_{ij}$'s estimates. In other words, it may be more useful to provide a confidence interval than a point estimate of the economic benefit. This approach was used to assess a proposed ban of pesticides in California.

In 1989, Californians voted on a proposition (Big Green) to ban the use of most chemical pesticides in the state. The results of research by agricultural economists from Berkeley were used in portraying the proposition in a bad light, which may have contributed to its defeat. Zilberman et al. (1991) assessed the economic impact of the proposition on five major California crops: almonds, grapes, lettuce, oranges, and strawberries. Several groups of scientists estimated the proposition's impact on the yield and output of different crops. Zilberman et al. (1991) used these numbers to estimate each crop's shift in supply as a result of the proposition. They found that the estimates of the demand and supply elasticities of the five commodities varied widely. They simulated the outcome of the proposition and several hundred likely scenarios. Had the proposition passed, the annual expected reduction in the consumer and producer surpluses of the five commodities was estimated to have been about $509 million. There was a 5 percent probability that the loss in consumer and producer surplus in the five markets would have been greater than $1.75 billion, and a 5 percent probability that the losses in the surpluses would have been smaller than $390 million. The variability of the estimates on the impact for each of the crops was significant. This demonstrates the value of providing more than one number to represent the estimated impact of a policy change.
Sunding, Zilberman, and MacDougall (1997) used a different approach to present the uncertainty of estimates of the effects of policies. Because modelers often disagree about the exact specifications of an economic system, they used three different models to assess the effects of reducing the supply of water to California agriculture. These reductions were associated with versions of the Central Valley Improvement Act of 1992 and water quality regulations for the San Francisco Bay and Delta. One of the three models was a large and detailed quadratic programming model of the water system in the San Joaquin Valley. Another assumed putty-clay technology (where input-output coefficients are fixed in the short run [clay] but may vary in the long run [putty]) and allowed little substitution between crops in adjusting to reductions in the water supply. This model was especially appropriate for assessing short-term income. A third model was less intensive, but allowed more flexibility in adopting technology and in using ground water in response to water supply cuts. In most cases, the putty-clay model provided a higher estimate of the reduction in water supply costs than the other two models. Overall, however, the results were consistent. For example, all the models indicated that the cost of aggregate cuts in the water supply depended on how the cuts were made rather than on their exact volume. So if the costs of supply reductions were shared only among a small group of growers who had junior rights to water, the cost of implementation might be two or three times higher than if the costs were shared among a major group of growers (for example, all the contractors in the Central Valley Project). Providing several estimates of economic effects based on different but plausible models is particularly useful in an ex ante impact assessment, where final outcomes depend on contingencies likely to be determined or observed in the future. This holds true as well for estimates of the effects of economics research, where economists combat credibility issues when testifying about the value of their own product.

**The Attribution of Benefits to Economics Research**

The issue of credibility is especially important when $S_{ij}$, the coefficient of benefits attributed to economics research, is determined. In many cases, economics research is essential in the policymaking process, but the final outcome depends on other actors in the system. Difficulty in determining how the credit for benefits should be shared is not unique to economics research. For example, it is a crucial element in the debate over how to share the profits of a new technology or how to establish royalty rights for that technology. The working hypothesis for technology-transfer negotiations at the University of California is that the net surplus generated by innovations should be distributed equally among innovators, developers, marketers, and producers (Parker, Zilberman, and Castillo 1998). Of course, the share of each contributor is adjusted as part of the negotiation. Universities, for example, will demand higher royalties for allowing the
right to use university-patented innovations that are further along in their development. A similar logic should apply in establishing coefficients that attribute benefits to economics research.

Developing quantitative theories and tools to determine the shares of contributors in the establishment of a particular policy is an important challenge faced by political economists and applied political scientists. Unfortunately, "guesstimates" and ad hoc rules are often used to determine these shares. Because economists face an apparent conflict of interest in determining the value of economics research, it is essential to obtain testimony and evidence from noneconomists about the contributions of their research to policymaking. This may be done through interviews, media coverage, and so forth. To be credible, it is important to use a conservative number. Interviews with individuals involved in policymaking may provide one set of numbers for the contributions of economics research to policy. It would be useful to ask a sample of decisionmakers to rate (on a scale from 0 to 10) how economics research (or, if possible, even research provided by specific projects) contributes to a particular outcome of policy. Obtaining such numbers is desirable but not always feasible, and alternative approaches may have to be pursued.

As argued earlier, new policies may be viewed in the same way as technological innovations. Credit for their establishment should be divided among conceptual and applied economic programs and the political players that contributed to this process. For example, applied researchers, extension specialists, and communicators might describe the basic idea to policymakers and shape it to fit the particular circumstances. Politicians and other actors in the political arena might then vote or lobby for policies and take the credit or blame for their choices. A generic approach for attributing the credit for policy innovations could borrow from offices that engage in technology transfer and assign 25 percent credit to the economics research that identifies the policy and provides the basic idea. Economists would get some of the 25 percent credit for "development," while the actors in the political process would get 50 percent for "marketing and production." This formulation is obviously only a starting point. Because the circumstances that lead to new policies vary significantly, as do the roles of economists and policymakers, the share of the benefits assigned to economics research should also vary. This may be best illustrated by some examples based on the authors' experience.

**Big Green**

Both proponents and opponents of this ballot proposition campaigned actively. Economists at the University of California became involved and may have affected the final outcome. Agricultural economists in Berkeley held a conference and wrote a report stating that the proposition was flawed because pesticides provide benefits, and some use could be justified because the benefits exceeded the costs. A number of politicians pressed university administrators to suppress
this report and punish the scientists. The media picked up the story, and articles about the research made the front page of some major newspapers. Based on this report, the opponents of Big Green adopted the argument that the proposition was bad for economic reasons. The public, which had favored the proposition in earlier polls, rejected it by a two-to-one margin. The proposition might have passed had it not been for the involvement of University of California researchers. To assess the benefits from this use of economics research, conservative assumptions should be adopted. The first is to attribute only 5 percent of the benefits to economics research rather than 25 percent. The second assumption would be to calculate the benefits of the use of pesticides on only the 5 crops mentioned earlier, out of more than 100 on which pesticides are used. This figure is conservatively estimated to be $300 million annually. If 5 percent of that figure can be attributed to the economics research benefits, the net benefit would be $15 million. This is greater than the expenditures on agricultural economists in the University of California.

Water Banks

Water in California and other western states has been allocated by water rights regimes that queue water users and their rights to water. This allows water trading, but it is not a water market system. For 40 years, a large body of literature in economics and agricultural economics had advocated specific proposals to transform this system into a market-oriented one. For years, economists testified at regulatory agency hearings in favor of this transition, but nothing changed until 1991. California was then in its fifth year of drought, and officials at the Department of Water Resources created a water bank.

The water bank bought the rights for water at $125 per acre-foot. This water was moved to the Delta, and farmers south of the Delta were able to buy it for $175 per acre-foot from the water bank. This mechanism facilitated trade between water-rich farmers in the north and water-starved farmers in the south. Some water was left to revitalize the ecosystem of the San Francisco Bay and Delta. A quantitative assessment of the 1992 benefits from this water bank, estimated to be $60 million, is presented in the appendix.

It seems reasonable to attribute 25 percent of the credit for the water bank to research in agricultural economics. Economists have argued in favor of water markets for years, in both professional journals and the popular press. Some senior decisionmakers in the California Department of Water Resources are educated in economics and were familiar with the argument in favor of water markets. Economists and staff members at the University of California and the Water Resources Department have maintained constant contact. Although it is

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difficult to identify the specific research projects that may have contributed to
the establishment of the water bank, the long history of advocacy of the water
bank system by economists justifies a 25 percent share of the $60 million ben-
efit. This $15 million represents a significant benefit from economics research.

**Other Water Stories**

In recent years, economists have been important in fostering a number of
changes in water policy. We examine a few of these cases to highlight the dif-
ficulties associated with quantifying the contributions of economists to the
policy process.

During 1992, the U.S. Congress held a debate about the volume of water
that could be diverted for environmental purposes from federal water projects in
California. One of these proposals, the defeated Johnston Bill, called for diver-
sion of 2.5 million acre-feet from the 8 million acre-feet of the Central Valley
Project. The results of agricultural economics research were used to help defeat
this bill and support the Bradley-Miller Bill that required diverting only 800,000
acre-feet. Had the Johnston Bill passed, it could have increased the costs to
society by $100 million (Zilberman et al. 1994b). But it seems unrealistic to
assign any benefit to the economics research that helped to defeat this bill. Even
if Congress had passed it, the president would have vetoed it. The important
contribution of economics research to the Bradley-Miller Bill was that, in ad-
dition to the diversion of water to environmental purposes, it made it possible
to trade in water. Even though this legislation passed in 1992, it is in the early
stages of implementation and the direct benefits are difficult to trace thus far.

Economists have helped to establish institutions for trading water in Cali-
ifornia. For example, Olmstead et al. (1997) found that an electronic water
market in the Westlands water district had increased annual water trading by
150,000 acre-feet above recorded historical patterns. This water market was
established as a cooperative effort among the water district, the University of
California, environmental groups, and the Bureau of Reclamation, and it is ex-
panding to include other water districts. Because environmentalists are involved
in the project, their political objections to the construction of conveyance facil-
ities for interregional trading may well be reduced. It is likely that there will be
large-scale trade in water in the Central Valley within 5 to 10 years. Such trade
will reform the California water system. Shah and Zilberman (1994) ran simu-
lations showing that up to 25 percent of available water could be saved if the
state made the transition from water rights to water markets. However, this es-
timate does not include the cost of making the transition to the new system,
adopting technology, and so forth. Even using a conservative estimate, and as-
suming a 5 percent savings of surface water, the annual savings would be about
1.6 million acre-feet. Conservatively valuing each acre-foot at $60, the annual
benefit from water markets would be about $100 million. At present, the ben-
efit from the water market operating in Westlands and other marketing arrange-
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Conclusion

Economics research generates a wide array of benefits, including information, technological change, and improved policy. There are few quantitative studies on the benefits of economics research, and some benefits may be misattributed to biological and physical research. The productivity of economics research is determined largely by its transmission and the ability of users to use it. Therefore, investment in extension outreach and economic literacy are important means of increasing the impact of research.

Even casual observation suggests that economics research is valuable, but noneconomists must be convinced of this. Because most benefits are likely to be concentrated in a small number of successful projects, a useful approach to an assessment of benefits would be to identify successful projects and analyze the benefits they provide. The analysis must recognize the uncertainty of such estimates. In addition, the argument behind the estimates should be transparent, relying on documentation and testimony from users, policymakers, and noneconomists. An assessment of the benefits from economics research provides information that can be used to justify continued support for economics research and for allocating monies among lines of research.

One of the most difficult obstacles in preparing an assessment of the benefits of economics research is a lack of evidence. Economists believe that the allocation of funds to disciplines and within them should be based on productivity. Some also advise that a key ingredient of effective management is accountability. As economists, we must practice what we preach and establish an effort to document performance and results. Reductions in the cost of data collection and documentation have lowered the cost of such efforts. One logical follow-up of this approach would be to develop procedures for documenting and accounting for the effects of economics research.

Appendix

The benefits of the California water bank that was created in 1992 are approximated in Figure 12A.1. For simplicity, the numbers are rounded. Farmers sold 600,000 acre-feet to the water bank at $125 per acre-foot. Water use varies in the Delta, so that the marginal benefit of water may be as low as $25 per acre-foot (low-value crops such as rice and pasture generate a marginal benefit of $25 per acre-foot or even less [Sunding, Zilberman, and MacDougall 1997]). In Figure A12.1, a linear supply curve for water is assumed. This intercepts the vertical axis at $25 per acre-foot, and 600,000 acre-feet of water would be supplied when the price is $125 per acre-foot. Farmers in the San Joaquin Valley bought 400,000 acre-feet from the water bank at $175 per acre-foot. Again, a linear demand curve is assumed. It is choked at the price of $325 per acre-foot (high-value crops such as peaches, tomatoes, and oranges have generated ben-
APPENDIX FIGURE 12.1 Assessing the effects of the California water bank in 1992

The benefit of $325 per acre-foot or higher [Sunding, Zilberman, and MacDougall 1997]). The benefit from the water bank includes four elements. First, the benefit to the seller is equal to the area $ABC$ in Figure 12A.1, or $\Delta ABC = (125 - 25)(600,000)/2 =$30 million. Second, the benefit to the buyers is represented by the area of the triangle $EDF$, or $\Delta EDF = (325 - 175)(400,000)/2 =$30 million. Third, the area $CDFG$ represents revenues to the government, half of which may cover the cost of the program, or $\Delta EDFG = (175 - 125)(400,000) =$20 million. The net benefit they generate is about $10 million. Finally, the triangle $FGB$ approximates the value of water left in the Delta, or $\Delta FGB = (175 - 125)(200,000)/2 =$5 million. Assuming a linear demand for water to be used for environmental purposes that corresponds to line $FB$ in Figure 12A.1, the net benefit this water generated would be $5 million.

The net benefit to sellers and buyers of water totals $60 million. The rest of the analysis of benefits is speculative. If there is a $10 million net benefit to the government and a $5 million environment benefit, then the total benefit from the water bank would be $75 million. To be conservative, the $60 million figure will be used as the estimated benefit from the water markets.
References


Sundin, D. 1997. Third annual report for challenge grant program: Collaborative field demonstrations of the efficacy and practicality of financial incentives for agricul-


In this book, we have examined what economists do and how they do it, and have considered the perhaps more vexing question of what it is worth. In these contexts, the following major themes have been addressed by the eleven studies that make up this work:

- The evolution of economics research as an industry and the global distribution of economics research resources.
- The role of economists and economics research in policy processes.
- The contributions of economists and economics research to social welfare: what have they been and do they matter?
- The measurement of the benefits and impacts of economics research on decisionmaking and the challenges that confront economists in estimating those effects and communicating the value of their work to decisionmakers.

**The Evolution of Economics Research**

Regardless of what economics research is actually worth, there is compelling evidence that over the past 120 years, the size of the economics profession and the amount of economics research being done has increased substantially. This growth has taken place, albeit at an uneven pace with some periods of stagnation and others of exceptionally rapid growth, and with persistent differences among regions of the world in the extent of economics research effort (Smith, Pardey, and Chan-Kang 2003, Chapter 2 of this volume). Moreover, the scope and emphasis of economics research have changed over time. Especially since the 1940s, economists have dramatically broadened the scope of their research to address new policy issues, including much of President Lyndon B. Johnson’s Great Society agenda (for example, health care, social security, and education), and, more recently, environmental and resource issues. Even newer concerns and newer areas of study, such as law and economics and the economics of the family, have been added to the more established focal points, such as economic growth, macroeconomic stabilization, and imperfect competition. Furthermore,
many new theoretical and empirical techniques have been developed. In the
process, the economics profession has become somewhat more fractionalized
as subdisciplines become more specialized.

The intellectual and geographic development of economics over the past
100 years has been profound. However, evidence from the evolution and geo­
graphic locations of economics associations and their memberships makes it
clear that economics research expertise is much more heavily concentrated in
developed countries than in developing countries, and that very poor countries
are especially lacking in economics research resources (Smith, Pardey, and
Chang 2003). From the perspective of global economic welfare, the lack of
expert economic policy advice in poor countries may be a significant deterrent
to economic growth. Harberger (Chapter 4, this volume) argues that when econ­
omists are institutionalized in bureaucracies, they play a crucial role in prevent­
ing the introduction of poor policies. As Solow (1997) has put it, reflecting on
his service to President John F. Kennedy’s Council of Economic Advisers, they
serve a critical function as intellectual sanitation workers, cleaning up at least
some of the messes created by advocates of poor policy.

The Role of Economists and Economics Research in the Policy Process

What economists do and how they do it, especially in the context of the policy
process, is an explicit focus in three of the studies in this volume (Harberger,
Chapter 4; Timmer, Chapter 6; Krueger, Chapter 8) and is at least tangentially
addressed in most of the other chapters. Many economists work inside the
policy process in government bureaucracies, playing important roles in the evo­
lution of new policies and regulations. One of their most important functions
is to prevent the introduction of truly adverse programs by pointing out many
of their unintended consequences, as well as mitigating some of their intended
consequences on economic welfare and its distribution among different interest
groups. As Harberger (Chapter 4) emphasized, economists who serve as policy
practitioners often work in highly constrained policy environments, where, from
an economic welfare perspective, the best policies (such as a uniform sales tax)
are simply infeasible and, therefore, second-best policies are the effective options.
However, second-best policies can often be a vast improvement on proposed
initiatives and can contribute to significant welfare improvements.

Keynes famously pointed out that, through their creative independent re­
search, some economists develop new perspectives that eventually become
pervasive among both policymakers and the economists who advise them. It is
in this sense that one of the most important consequences of economics research
is that policymakers eventually become slaves to its findings. This is not to say
that economists always get things right, and Krueger’s essay (Chapter 8) on the
catastrophic catalog of intellectual events surrounding the Prebisch hypothesis
is illuminating in this regard. The promulgation of import substitution as an
effective policy for positive economic growth in developing countries was never plausible from the perspective of Ricardian trade theory, but it was widely advocated by development economists in the 1950s and 1960s and was not widely abandoned by many developing country policymakers until the 1980s and 1990s. In some ways, Kreuger’s study is a seminal account of the appropriate decline and fall of a grossly misguided policy recommendation in response to good empirical and theoretical economics research. As Kreuger emphasizes, however, her tale is also a cautionary one, for the theory of comparative advantage was well developed and widely accepted by economists long before Prebisch and others argued, on the basis of inadequate and faulty data and inappropriate theory that, in contra-distinction, import substitution was essential for industrialization and economic development.

Implicitly, Krueger’s account of the import substitution imbroglio highlights another vital role for economists: As in any science, economists need to examine and question existing theoretical and policy paradigms. Economics research can only become more valuable to the policy process if economists gladly pursue a Hegelian dialectic process, in which existing ideas are challenged by new hypotheses, and data about the competing hypotheses are evaluated. In other words, at least in this context, President Harry Truman was wrong-headed in pleading for a one-handed economics profession.

The Contributions of Economics and Economics Research to Social Welfare

The economics research industry continues to produce a broad portfolio of economics research output. These outputs range from specific products derived from very applied research to new information and insights that flow into the decisionmaking processes of households, firms, and governments (Smith and Freebairn, Chapter 5). Krugman (Chapter 3) provides a broad perspective on substantial historical contributions of economics to our understanding of the economy and economic policy. He argues that major contributions, such as David Ricardo’s theory of comparative advantage and the insights of macroeconomists about the effects of monetary and fiscal policies on economic stabilization and growth, have had substantial impacts on policy choices and the organization of society, and that widespread economic literacy is a precursor for improved public and private decisionmaking. Much, however, remains to be done with respect to economic literacy and policy competency, especially in poor countries, where the general lack of human capital has been debilitating.

Smith and Freebairn (Chapter 5) provide a more detailed account of the range of outputs provided by social science (with an emphasis on economics). Economics research produces new information, including new data on economic performance and new findings about the impacts of economic policies. It also produces new products, such as methods for computing optimal inven-
tory holdings and optimal animal feed mixes, that increase productive efficiency. They suggest that the value of the information provided by policy-oriented economics research may lie in the extent to which it results in what Ruttan (1984, 2003) termed "institutional change"; that is, the research alters the structure and setting of economic policy instruments. One important issue here is that economics research, and perhaps especially policy-oriented economics research, must not only produce results, but, as both Lindner and Gardner (Chapters 7 and 9) point out, those results must be potentially useful.

Essentially, a central conclusion of the studies in this book is that economics research has affected almost all aspects of the daily lives of ordinary people. The reason for this is not hard to find. Almost all aspects of our daily lives require that we make choices about the use of scarce resources, and economics research directly addresses the efficiency with which those choices are made.

The Measurement and Communication of the Consequences of Economics Research

Another core theme of this book is that there is a real need for methodologies to measure and communicate the economic benefits of economics research. There is a general consensus across the 11 studies in Chapters 2 through 12 that changes in economic welfare are what matter, and Smith and Freebairn (Chapter 5) argue that accounting for dynamic effects is critical. However, using techniques that are widely applied to measure the impact of other types of research (such as agricultural research and development) on productivity may be especially problematic with respect to economics research, because of difficulties associated with measuring both research inputs and outputs. For example, many of the outputs from economics research represent new public-good knowledge and nonmarketed services supplied in the public and household sectors.

There is a broad consensus that case-study approaches (in which, ex post, the economic welfare effects of selected research programs are evaluated) could be more productive, but that cherry picking—the selection of highly successful research programs—is inappropriate. Economics research not only produces gushers and dry wells, but also poisoned wells (Smith and Freebairn, Chapter 5; Lindner, Chapter 7; Krueger, Chapter 8); that is, sometimes economics research and policy analysis leads to the adoption of welfare-reducing policies. When the poisoned-well syndrome is possible, cherry-picking approaches are likely to overstate the benefits of economics research. In addition, it must be recognized that some economics research is proprietary or "oppositional" in nature and may be used by individual interest groups for rent-seeking purposes (Harberger, Chapter 4; Smith and Freebairn, Chapter 5; Lindner, Chapter 7; Gardner, Chapter 9). This type of research may well have negative effects on aggregate economic welfare, even though the research redistributes economic benefits to the interest groups who commissioned it.
Gardner (Chapter 9) and, not without some reservations, Lindner (Chapter 7) both recommend that, because policy-oriented economics research most often generates information, Bayesian approaches to estimating the potential value of new economics research projects may be useful for those who allocate research budgets. Both focus on the insights provided by Hirschleifer and Riley (1992) as well as Bradford and Kelejian (1978) about the value of information provided by research. Research is only valuable if it both provides new information and, as noted above, ex ante that information also has some probability of altering the choices selected by decisionmakers. The research is likely to be more valuable if the new information will be used by larger numbers of decisionmakers. However, applying the Bayesian approach to valuing economics research may be difficult, not least because specifying the research funder's likelihood function may not be feasible (Lindner, Chapter 7; Norton and Alwang, Chapter 10).

Research on measuring the economic benefits of policy-oriented research is challenging and in its infancy. Gardner (Chapter 9), Norton and Alwang (Chapter 10), Ryan (Chapter 11), and Zilberman and Heiman (Chapter 12) all provide case studies evaluating the effects of specific policy-oriented economics research programs.

Gardner (Chapter 9) examines the potential benefits of research by agricultural economists in four areas: (1) domestic policy reform in the United States; (2) returns to agricultural science R&D and the allocation of research funds; (3) the value of economic information on market behavior; and (4) international agricultural trade policy reforms under the Uruguay Round of the General Agreement on Tariffs and Trade (GATT). In each case, his "back-of-the-envelope" estimates suggest that benefits from the agricultural economics research programs substantially exceeded any reasonable estimates of their costs.

Norton and Alwang (Chapter 10) investigate the economic benefits from two separate economics research programs assessing (1) the welfare consequences of reducing subsidies that encouraged deforestation in Brazil, and (2) reducing subsidies for the use of pesticides in the Philippines. In both cases, they estimate that the actual or potential net present value of possible policy changes substantially exceeded the costs of the research.

Ryan (Chapter 11) analyzed the payoff from an economics research project that revealed to government policymakers the welfare consequences of liberalizing rice markets in Vietnam. Ryan's findings indicate that the project contributed to economic welfare by speeding up the implementation of new rice export policies that increased economic welfare sufficiently to yield a benefit-cost ratio for the project that is very large.

Zilberman and Heiman (Chapter 12) provide case studies of the impact of economics research on California's Big Green policy proposal to sharply reduce pesticides use and create a water bank. In both cases, the payoffs attributed to the research were large.
Notwithstanding the optimistic perspectives on the value of economics research provided by these case studies, at least two common themes were identified in each case that serve as important caveats and concerns for future research on the benefits of economics research. First, the attribution problem was at best only partially resolved. The attribution problem is as follows: new policies may be implemented for many reasons and the relative contribution of economics research to the policy changes may or may not be substantial. Moreover, whose policy research—past or present—accounts for which particular part of a policy package is difficult to discern. Sorting out this problem is not a simple matter. However, there is a general consensus that reliable and credible procedures for resolving the issue are urgently needed.¹

Second, in each of the case studies included in this book, the authors measured the economic benefits of economics research using either a simple assessment of their impacts on real gross domestic production or applied measures of economic welfare (consumer and producer surpluses and taxpayer cost changes). Thus a circularity problem raised by Krugman (Chapter 3) comes into play. Economists may have no (or relatively few) problems accepting these measures as indicators of economic benefits. However, they do have to persuade noneconomists that such measures are reasonable.

Conclusion

Economics research has made substantial contributions to the well-being of ordinary people in almost all countries of the world. These contributions have pervaded the fabric of private and public decision processes and enhanced our ability to utilize resources efficiently. However, a central conclusion of this volume is that for economics research to be useful, economists must be able to influence policymakers. As with monetary policy, when it comes to obtaining substantial benefits from any economics research, the transmission mechanisms are crucial. Those benefits, which can clearly be substantial, can only accrue if effective economic policy practitioners are in the right place at the right time to provide the right message. In the global community, the governments of almost all the developed and rapidly developing economies have embedded economics researchers in a plethora of departments and agencies. In addition, the public

¹ For example, Norton and Alwang (Chapter 10) interviewed only three researchers when assessing the role of the economics research program in altering Brazil’s subsidy programs that were encouraging deforestation. Thus, they did not have a very precise handle on the proportional impact of the research on the policy decisions. Similarly, although Ryan (Chapter 11) interviewed over 20 decisionmakers in his study of rice policy research in Vietnam, the survey instrument was relatively imprecise and often administered in settings that did not guarantee the respondents’ confidentiality. Gardner simply guesstimated (albeit conservatively) the possible proportion of total economic benefits attributable to economics research on agricultural trade liberalization.
and private sectors have provided resources and infrastructures for economics researchers in research institutes, private consulting firms, and universities, who work outside of government bureaucracies to provide critical inputs to decisionmaking processes. However, as is the case with many components of scientific and technical knowledge, the poorest countries of the world are woefully short and desperately in need of adequate economic expertise.

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