Policy and Innovation in Low-Carbon Energy Technologies

by

Gregory Frank Nemet

A.B. (Dartmouth College) 1995
M.A. (University of California, Berkeley) 2004

A dissertation submitted in partial satisfaction
of the requirements for the degree of

Doctor of Philosophy

in

Energy and Resources

in the

GRADUATE DIVISION

of the

UNIVERSITY OF CALIFORNIA, BERKELEY

Committee in charge:

Professor Daniel Kammen, Chair
Professor Severin Borenstein
Professor Margaret Taylor

Spring 2007

UMI Number: 3279632

ProQuest Information and Learning Company
300 North Zeeb Road
P.O. Box 1346
Ann Arbor, MI 48106-1346

Policy and Innovation in Low-Carbon Energy Technologies

by

Gregory Frank Nemet

Abstract

Policy and Innovation in Low-Carbon Energy Technologies

by

Gregory Frank Nemet

Doctor of Philosophy in Energy and Resources

University of California, Berkeley

Professor Daniel Kammen, Chair

Reducing greenhouse gas (GhG) emissions by several gigatons of CO_2-equivalents per year, while affordably meeting the world's growing demand for energy, will require the deployment of tens of terawatts of low-carbon energy production and end-use technologies over the next several decades. But improvements are needed because existing technologies are expensive, limited in availability, or not sufficiently reliable for deployment at that scale. At the same time, the presence of multiple market failures implies that private actors will under-invest in climate-related innovation without government intervention. To help resolve this impasse, policy makers will need to select from a vast set of policy instruments that may stimulate innovation in, and adoption of, these technologies.

In this thesis, four studies are used to contribute to understanding the characteristics of the innovation process—and its interactions with policy—for low-carbon energy technologies. These include analyses of: (1) the trends and future prospects for U.S. energy R&D investment, (2) the effectiveness of demand-pull for wind power in California, (3) the sources of cost reductions in photovoltaics (PV), and (4) the effect of widespread deployment of PV on the earth's albedo. When considering these studies together, the uncertainty in expectations about future policies that increases the risk for investments in innovation emerges as a central problem. As observed in multiple instances in this thesis, the lags between investments in innovation and the payoffs for private actors can last several years. These distant payoffs rely heavily on the status of future government policies because externalities are pervasive for the development of climate-relevant technologies. When expectations about the future level—or existence—of these policy instruments are uncertain, then firms discount the value of these future policies and under-invest in innovation.

The diffusion of institutional innovation is a necessary precondition for the technological innovation required to address climate change. If long-term GhG reduction targets are to be relied upon to stimulate innovation in low-carbon energy technologies, then policy makers need to address the competing goals of increasing the time-consistency of policy and retaining the ability to make adjustments that incorporate new information.

Professor Daniel Kammen, Chair Date

Dedicated to Linda, for her love and patience.

Contents

List of Figures

List of Tables

Acknowledgements

It would be difficult to overstate how much I have learned from Dan Kammen, my primary advisor since I started at ERG. I especially appreciate his recurring challenge to be engaged in work that is bold, innovative, and important. Margaret Taylor provided a model for how to conduct policy-oriented empirical research. I am also grateful for the candid discussions on a variety of topics throughout my graduate school career. Severin Borenstein is a tremendous mentor. I deeply value his engagement with my work and his efforts to improve it. Thanks also to Alex Farrell and David Mowery for providing helpful feedback at various stages, and in particular for their advice while serving on my qualifying exam committee. The community of staff, students, and faculty at ERG has provided a continuous source of inspiration and support.

Discussions with Brian Arthur, Arnulf Grübler, and Nebojsa Nakicenovic at the International Institute for Applied Systems Analysis (IIASA) heavily influenced my thinking about technological change and energy systems. All three continue to be important mentors. I thank the other YSSP students as well for making my summer there enjoyable and intellectually challenging.

Comments I received during various workshops have enhanced this work, especially those at the Innovation Seminar at the Haas School of Business, which I now think of as a model for how to improve research in progress. In addition, I received helpful comments at the U.K. Energy Research Center at Oxford and the European Association of Environmental and Resource Economists Summer School for Environmental Economics.

I have also benefitted from discussions with: Max Auffhammer, Ottmar Edenhoffer, Tim Foxon, Reyer Gerlagh, Martin Green, Rob Gross, Bronwyn Hall, Staffan Jacobsson, Jon Koomey, Skip Laitner, Robert Margolis, Paul Maycock, Karsten Neuhoff, Christine Oughton, Tom Rutherford, Alan Sanstad, Bob Schock, Suzanne Scotchmer, Tarja Teppo, Chihiro Watanabe, and Clas-Otto Wene.

I thank the Energy Foundation for supporting my doctoral work and the U.S. National Academies for supporting my time at IIASA.

Finally, I would never have embarked on this intellectual journey without the emotional support of my family. My late grandfather initially inspired me to see research and writing as a worthwhile activity. My parents and my sister have always encouraged me and supported my choices. Foremost, I want to thank my wife, Linda. She provided constant support in innumerable ways. I can neither thank her enough nor fully express my admiration for her.

Chapter 1

Introduction: Innovation and Climate Policy

1.1 The Climate Innovation Policy Problem

Selecting, timing, and implementing government actions to induce innovation in the energy system are central challenges for stabilizing the climate. Reducing greenhouse gas emissions equivalent to hundreds of gigatons of CO_2, while affordably meeting the world's growing demand for energy, will require the deployment of tens of terawatts of low-carbon energy production and end-use technologies over the next fifty years (Edmonds and Smith, 2006). But existing low-carbon energy technologies are either expensive, limited in availability, or unproven in reliability for deployment at that scale. And the presence of multiple market failures implies that private actors will under-invest in carbon-related technology improvements without government intervention. To help resolve this impasse, policy makers will need to select from a vast set of policy instruments that may stimulate improvements in, and adoption of, low-carbon energy technologies.

1.1.1 The need for non-incremental emissions reductions

Limiting greenhouse gas (GhG) concentrations to double pre-industrial levels would require cutting future emissions by approximately 60% from 'business-as-usual' levels by mid-century (Wigley *et al.*, 1996; Pacala and Socolow, 2004; Conover, 2005; Stern, 2006) (Fig. 1.1). Cost-reducing and performance-enhancing improvements in carbon-free energy technologies are essential for reductions of this scale (Edmonds *et al.*, 2004). While the switch toward less carbon intensive fuels over the past 150 years has gradually reduced the carbon intensity of the world economy (Grübler, 1998), this background "decarbonization" is far from adequate to achieve the carbon reductions necessary to stabilize the climate (Fig. 1.2). This gradual fuel switching, as well as incremental improvements to existing technologies at their historical rates, are insufficient; larger changes to the energy system will be needed. Despite the centuries-long time horizon of the climate change problem, urgency surrounds policy decisions because the roughly 100-year residence time of CO_2 in the atmosphere (Jacob, 1999) and the the 30 to 50-year lifetime of capital stock in the energy industry (Knapp, 1999) are, to a great extent, outside of our control. The

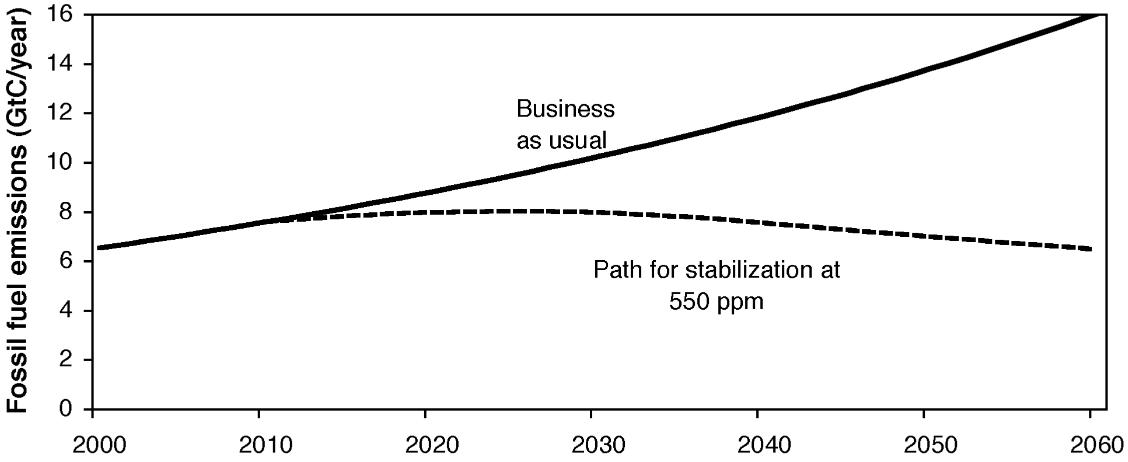

Figure 1.1: Scenarios of fossil fuel emissions. Data: Wigley *et al.* (1996).

benefits from such investments in low-carbon innovation may be large. In assessing the cost of climate policy, Richels *et al.* (2007) found that "Investments in climate friendly technologies can reduce GDP losses to the US by a factor of two or more." Bernstein *et al.* (2006) used simulation results to show that the effect on national energy expenditures of high penetration rates of renewables depends primarily on the relative technological change of renewables versus fossil fuels. But improvements to technologies require investment (Grubb, 2001).

1.1.2 Market failures and the need for a government role

Accounting for two-thirds of R&D investment in the U.S. (Wolfe, 2004; Nemet and Kammen, 2007), the private sector is likely to continue to play a central role in investing in

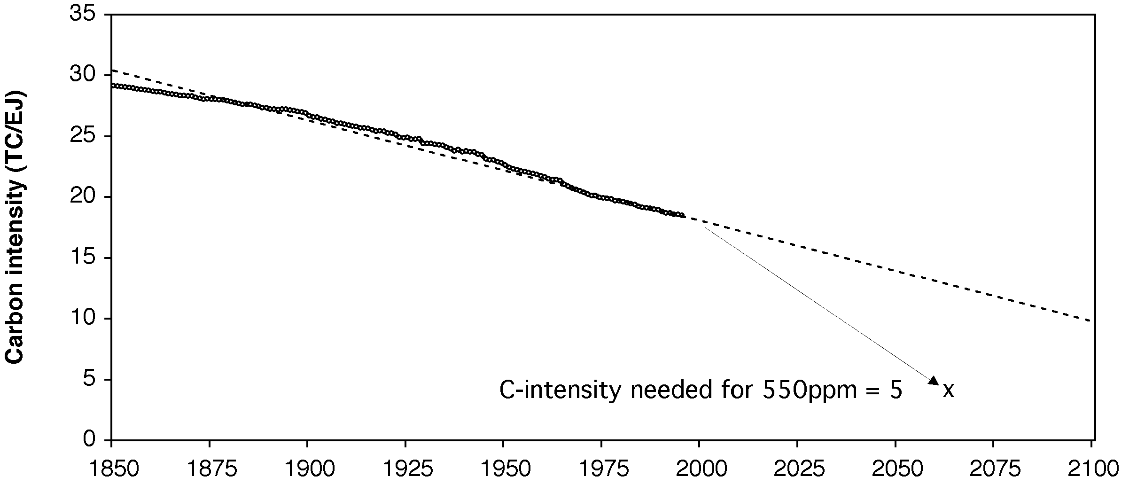

Figure 1.2: Carbon intensity of world energy use (tons of carbon emitted per exajoule of energy generated). Data adapted from Grübler (1998).

innovation and making the outcomes of the innovation process commercially available. However, because of market failures profit maximizing firms will not invest adequately without changes to the business environment in which they operate. When market failures are present, achieving an efficient outcome requires government intervention. In the case of low-carbon innovation, at least two well-documented market failures exist (Jaffe *et al.*, 2005). The first is that the *pollution externality* of GhG emissions is an unpriced negative externality. Human societies have developed institutions, infrastructures, values, and patterns in daily life that are based on a climate that varies within an acceptable range, and only slowly.[1] Access to this stable climate is a public good. Because GhG emissions destabilize the climate from historical norms and thus create future damages (Hayhoe *et al.*, 2004;

---

[1] "Climate" here is defined as conditions of the atmosphere averaged over 30 years (Houghton *et al.*, 2001).

Emanuel, 2005), the values of which are not reflected in prices, offsetting GhG emissions is only weakly valued by private actors. Estimates of future damages vary widely, but are potentially large.[2] For example, Tol (2002) found that, depending on how consumption across countries is compared, a 1°C warming would affect world product by between +2 and -3%. More recently, a report by Stern (2006) taking into account larger temperature changes, feedbacks in the climate system, and the impacts of extreme weather events found that future damages are likely to be higher, reducing world consumption per capita by "At least 5%." This report referred to climate change as "The greatest and widest-ranging market failure ever seen."

The second market failure, *knowledge spillovers*, arises because firms under-invest relative to the socially optimal level of R&D (Nelson, 1959; Arrow, 1962b; Teece, 1986). Firms are unable to capture the full value of their investments in R&D because a portion of the outcomes of R&D efforts "spills over" to other parties as freely available knowledge, e.g. other firms can reverse engineer new products (Griliches, 1992). Jones and Williams (1998) found that the social rate of return to R&D is four times larger than the private rate of return. Okubo *et al.* (2006), in an effort to estimate the macro-economic asset value of R&D expenditure, surveyed previous work comparing the social and private rates of return to R&D. In Fig. 1.3, I display the data in the surveyed studies—Terleckyj (1974); Mansfield *et al.* (1977); Sveikauskas (1981); Scherer (1982b); Bernstein and Nadiri (1988); Goto

---

[2] Estimation of future damages lies at the far end of the chain of causation in the global warming problem and consequently it is the area where uncertainty is highest.

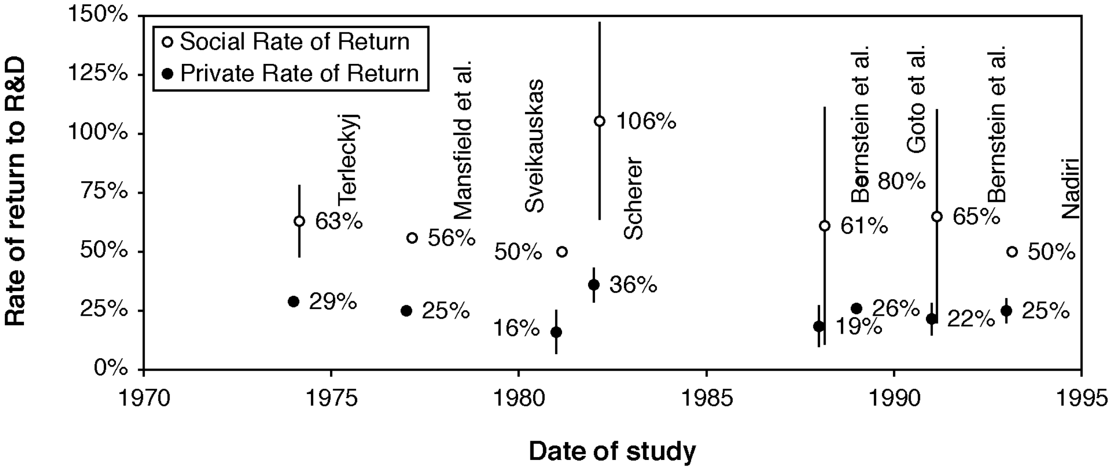

Figure 1.3: Private and social rates of return to R&D surveyed by Okubo *et al.* (2006). Ranges of values shown where available. Data from Terleckyj (1974); Mansfield *et al.* (1977); Sveikauskas (1981); Scherer (1982b); Bernstein and Nadiri (1988); Goto and Suzuki (1989); Bernstein and Nadiri (1991); Nadiri (1993).

and Suzuki (1989); Bernstein and Nadiri (1991); Nadiri (1993)—to show that the public rate of return consistently exceeds the private rate of return and to show the dispersion in estimates. The average private return to R&D across these studies is 25% whereas the public return is 66%. While spillovers, per se, are beneficial since they expand access to the outcomes of R&D efforts, inappropriability prevents firms from receiving the full incentive to innovate and thus discourages them from investing as much in R&D as they otherwise would.

Others have pointed out additional market failures. Fischer (2004) proposed that an interaction effect between the two market failures above might exist such that the simultaneous presence of both market failures creates a further disincentive for investment. The portion of the value of a new innovation that spills over to other firms *and* has climate benefits may not overlap completely with the value contained in the individual market failure components, exacerbating the level of under-investment. Earlier work by Kaldor (1972) and Arthur (1989) identified another market failure; the increasing returns that accrue to technologies already in the market—for example, through network effects, learning-by-doing, and economies of scale—may discourage the development of technologies that are not yet commercially available, but which may ultimately be necessary (Driessen, 2003; Sanden and Azar, 2005).

1.1.3 The policy challenge

The need for technological innovation in a business environment characterized by multiple market failures creates a challenge for technology policy: how should public resources be allocated across a diverse set of policy instruments, for an unknown number of technological options, over a multiple-decade time scale? Setting greenhouse gas emissions targets has so far proven a popular first step. Currently implemented climate policy targets, such as the Kyoto Protocol and the current U.S. federal target, demand only modest reductions (Fig. 1.4)(EPA, 2005; Kammen, 2006). In fact, meeting the Bush Administration's climate intensity target for 2012 (Conover, 2005) would involve an *increase* in emissions of 15% above the levels in 2002, when the target was set.

Recently announced targets reveal shifts to both a longer term outlook and substantially

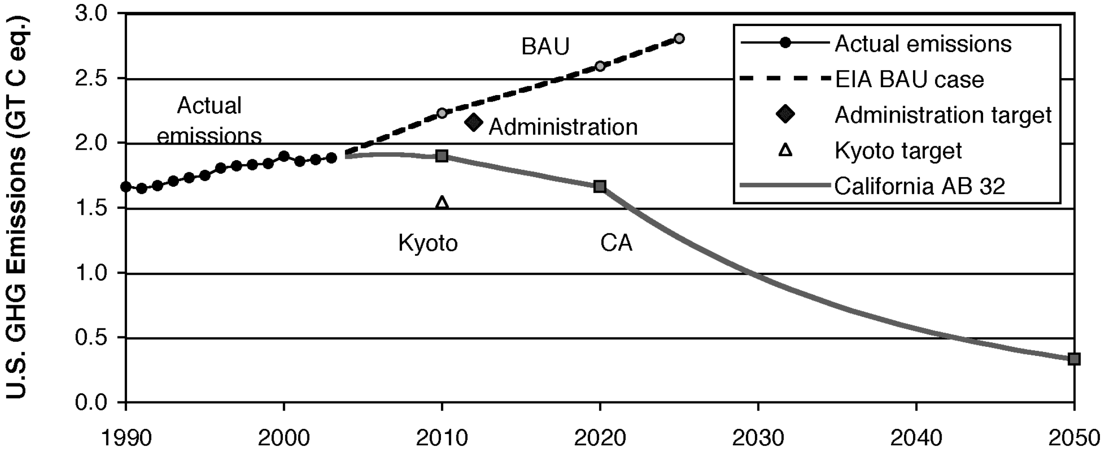

Figure 1.4: Historical U.S. GhG emissions and targets. Data from EPA (2005).

more ambitious reductions. California, with its recently signed targets for 2010 (reduce emissions to 2000 levels), 2020 (1990 levels), and 2050 (80% below 1990 levels), calls for much more substantial reductions (Nunez, 2006). The European Union recently agreed to an even stronger target, which would cut 2020 emissions to 20% below 1990 levels (Ames *et al.*, 2007). Finally, the draft “Climate Change Bill” recently introduced by the U.K. Environment Ministry would cut emissions by 26 to 32% from 1990 levels by 2020 and by 60% from 1990 levels by 2050 (Hall, 2007).

The targets in the Kyoto Protocol and the Bush Administration’s plan are modest enough that small changes to the tax regime, or even encouraging voluntary measures may be sufficient to meet the targets (Pizer, 2005). The California, E.U., and U.K. targets, on the other hand, require a more comprehensive set of implementation policies that will likely impact a much broader swath of the populations and economies of those policies. Consequently, the discussion about the best way to meet them is more contentious.

The extent to which government actions can be relied upon to induce innovation in environmental technologies is crucial for decisions about future energy supply (Holdren, 2006; Gallagher *et al.*, 2006). Yet, a recent debate on the need for developing radically new systems versus incrementally improving existing energy technologies exemplifies, at a high level, the divergence of positions in policy prescriptions. Some groups, such as Hoffert *et al.* (2002), advocate “Revolutionary changes in the technology of energy production” because currently available technologies have “severe deficiencies that limit their ability to stabilize global climate.” Others, such as Pacala and Socolow (2004), argue that “Humanity can solve the carbon and climate problem in the first half of this century simply by scaling up what we already know how to do.” The limited empirical basis for understanding the effects of government actions explains why these studies diverge so extremely in their conclusions; each study makes strong assumptions about the ability of public policy to influence the rate of invention, commercialization, and adoption of low- and zero-emission energy technologies.

In a continuation of the debate, O’Neill *et al.* (2003) argue that “Fundamentally new technologies” will arrive too late because “the process from invention, to demonstration projects, to significant market shares typically takes between five and seven decades.” In contrast, Hoffert et al. hold that “Market penetration rates of new technologies are not physical constants. They can be strongly impacted by targeted research and development, by ideology, and by economic incentives" (Hoffert *et al.*, 2003). This debate shows that decisions related to future energy technologies depend heavily on the extent to which policy can be relied upon to drive innovation. They also reveal a rather weak empirical basis for making assumptions about the ability of government actions to affect the rate of innovation (Edenhofer *et al.*, 2006; Pizer and Popp, 2007).

This prevalence of strong assumptions is particularly troubling because the economic stakes associated with these decisions are large. Worldwide sales in the energy sector add up to $2 trillion per year (Vaitheeswaran, 2003). Studies that attempt to gauge the magnitude of the costs of climate change mitigation, and of the expected damages, typically speak in terms of low single digits of world GDP (Tol, 2002; Stern, 2006), which currently amounts to several hundred billion dollars per year. Governments are already devoting large sums of public funds. In the U.S., the value of total public funds devoted to energy technology subsidies from 1950 to 2005 was two-thirds of a trillion dollars (Bezdek and Wendling, 2006). California alone operates individual programs worth billions in incentives, such as the $3 billion California Solar Initiative (Peevey and Malcolm, 2006). The scale of the energy industry, and, consequently, the efforts to change it, mean that good decisions can have large favorable impacts—and mistakes are unlikely to be forgotten.

Given the permutations of policy choices available and the magnitude of the stakes associated with their results, guiding principles about the effect of policy on innovation are scarce and valuable. An earlier debate within the economics of innovation literature provides a helpful framework for informing these decisions.

1.2 Demand-pull and Technology-push

Within the substantial body of work in the economics of innovation on understanding the drivers of technological change, a prominent cluster distinguishes between forces that affect the *demand* for innovation and those that influence the *supply* of new knowledge. Although that debate within economics subsided as a consensus emerged on the need for both, this debate is revisited here because (1) the arguments on each side highlight important attributes of the innovation process, (2) subsequent policy-oriented research has employed this distinction to inform policy design, (3) these terms are frequently used in energy policy debates themselves, and (4) the terminology used in the earlier debate has in many cases been more precise than that used in current policy debates.

1.2.1 Economic debates on the sources of innovation

Following the widespread recognition of the role that technology plays in economic growth (Solow, 1956) and early work characterizing the process of innovation (Schumpeter, 1947; Usher, 1954), a debate emerged in the 1960s and 1970s about whether the rate and direction of technological change has been more heavily influenced by changes in market demand or by advances in science and technology.

One pair of studies, from the 1960s, clearly portrays the vigorous debate between the two views, in this case, between the Department of Defense and the National Science Foundation. In Project "HINDSIGHT", the Department of Defense presented a historical analysis about the importance of "need" in the development of 710 key military innovations, or what they referred to as "Events", for example, satellites, aircraft, and missile systems (Sherwin and Isenson, 1966; Greenberg, 1966):

> "Nearly 95 percent of all Events were motivated by a recognized defense need. Only 0.3 percent came from undirected science" (Sherwin and Isenson, 1967).

Their explicit conclusion was that defense procurement was critical to innovation. Project "TRACES" (Technology in Retrospect and Critical Events in Science), which was sponsored by the National Science Foundation and was a response to HINDSIGHT, identified the role of basic research in 341 "research events." The study focused on their impact on five important new technologies: magnetic ferrites, the video tape recorder, oral contraceptives, the electron microscope, and matrix isolation (IIT, 1968). They emphasized that the effects of basic research become dominant once a sufficient time frame for analysis is used, i.e. thirty years. Similarly to contemporary debates about technology policy, federal budget appropriation considerations may have promoted the adoption of strong language and positions. But the polarization of the debate was also emblematic of academic debates at the time, which tended to see the two explanations of technical progress as mutually exclusive.

Science and technology push

The core of the science and technology-push argument is that advances in scientific understanding determine the rate and direction of innovation. Immediately after the success of the Manhattan Project, Bush (1945b,a) articulated a highly influential version of this argument in what became known as the "post-war paradigm," and later more derisively as the "linear model." He envisioned a model of technology transfer based on a progression of knowledge from basic science to applied research to product development to commercial products. Dosi (1982) later attributed the prominence of this line of reasoning to several "established" aspects of the innovation process: the increasing importance of science in the innovation process, increasing complexity which necessitated a long-term view, apparently strong correlations between R&D and innovative output, and the inherent uncertainty of the innovation process.

One central critique of the technology push argument is that it ignores prices and other changes in economic conditions that affect the profitability of innovations. Another is that the emphasis on a unidirectional progression within the stages of the innovation process was incompatible with subsequent work that emphasized feedbacks, interactions, and networks (Kline and Rosenberg, 1986; Freeman, 1994; Freeman and Louca, 2001).

Later theories on technology push offered a less deterministic version of the argument; they emphasized the role of science and technology while accepting that demand matters. For example, some argued that the existence of exploitable "technological opportunities" plays a role in determining the rate and direction of innovation, and that these may depend on the "strength of science"; in other words, the pace of advances in science and technology in each industry (Rosenberg, 1974; Nelson and Winter, 1977). Klevorick *et al.* (1995) pointed out that, even though demand plays a role, variations in "technological opportunity," the ease with which a technical advance can be made, are critical in explaining the rapid technological advance in some sectors of the economy relative to others.[3] "Capabilities push," idiosyncratic firm-level competencies, emphasized changes in a firm's ability to pursue particular technology paths (Freeman, 1974). One implication is that firms must invest in scientific knowledge to develop their "capacity to absorb" knowledge and exploit opportunities emerging from the state-of-the-art elsewhere (Mowery, 1983; Rosenberg, 1990; Cohen and Levinthal, 1990). Also, strategic decisions and idiosyncratic capabilities within firms are an important driver of the aggregate direction of innovation (Dosi *et al.*, 2006). Another strand raised the issue of the inter-relatedness of the technological system (Frankel, 1955); flows of knowledge between sectors could be important (Rosenberg, 1994) and bottlenecks in the system raised "technological imperatives" that needed to be overcome (Rosenberg, 1969). Rosenberg (1994) also argued that an important determinant of the rate and direction of technological change is the transfer of concepts from one scientific specialty to another. In this same vein, there was a recognition that the activities within R&D are heterogeneous and that the interaction of various actors within the inno-

---

[3] Their study used survey data on 130 industries (4-digit SIC codes) to construct quantitative measures of technological opportunity and found significant and positive correlations between the variables they constructed and several measures of R&D intensity and performance.

vation supply chain is important. Finally, rejoinders to the critiques of the 'linear' aspect of the model defended the "sequential" character of science and technology push: even though,

> "The overall pattern of the innovation process can be thought of as a complex net of communication paths," it is "still essentially a sequential process... with feedback loops" (Rothwell, 2002).

The concept of science and technology push that emerged was multi-dimensional and acknowledged some of the nuances of the innovation process that the strictly 'linear' model ignored. It also differed from earlier versions of the concept in that the abandonment of the language of mutual-exclusivity meant that technology-push could be considered a complement to alternative hypotheses, such as demand-pull.

Demand-pull

Studies in the 1950s and 60s argued that economic factors drive the rate and direction of innovation. Changes in market demand create opportunities for firms to invest in innovation to satisfy unmet needs. Demand "steers" firms to work on certain problems (Rosenberg, 1969). Shifts in relative factor prices (Hicks, 1932)[4], geographic variation in demand

---

[4] Hicks (1932) devised a theory of "induced innovation", in which he related changing factor prices and natural resource availability to the rate and direction of inventive activity. While it pre-dated the technology-push/demand-pull debate, Hicks' induced innovation hypothesis fits into the set of arguments emphasizing the importance of the payoff to innovation. So even though it focused on the supply side of the firm, it had to deal with the demand for innovations that could conserve on a newly scarce input. The theory was later criticized for confusing innovation with factor substitution (Rosenberg, 1969).

(Griliches, 1957)[5], as well as the identification of "latent demand" (Schmookler, 1962, 1966)[6] and potential new markets (Vernon, 1966)[7], all affect the size of the payoff to successful investments in innovation. In the specific case of energy technologies, changes in the prices of conventional sources of energy affect the demand for innovation both within existing processes (Lichtenberg, 1986) and for alternative devices (Popp, 2002).

Dosi (1982) outlines five assumptions required for the demand-pull position: (1) the set of available goods in the market satisfy different needs by consumers, (2) consumers express their preferences for particular features of different goods through demand, (3) increasing incomes lead to the emergence of new preferences, (4) producers understand the "revealed needs" of consumers, and (5) they engage in innovative activities that eventually enable them to bring improved goods to market. Critiques of the theory tend to focus on these assumptions.

Critics of the demand-pull argument attacked it on three grounds. Methodologically, the definition of "demand" in empirical studies had been inconsistent and overall, was considered too broad a concept to be useful (Mowery and Rosenberg, 1979; Scherer, 1982a;

---

[5] Griliches (1957) found that differences in the timing and spatial distribution of the adoption of hybrid corn seeds were influenced by the profitability of the new invention in each location. It was the farmers themselves, and their heterogeneity, that determined the pattern of adoption of this new technology.

[6] Schmookler (1962, 1966) argued that market mechanisms and "latent demand" drove the direction of technological change. Using over a hundred years of patent data on a variety of sectors, he found that patenting activity was correlated with output. He not only emphasized the role of demand, but also rejected the idea that technical progress played a role. The latter claim rests mainly on his observation of a lag between output and patenting.

[7] Vernon (1966) highlighted that the differences in rates of innovation across developed countries were not due to access to scientific knowledge, which he assumed to be equal, but to the activities of entrepreneurs in identifying potential markets for new consumer products.

Kleinknecht and Verspagen, 1990; Chidamber and Kon, 1994). A second line of criticism was that demand explains incremental technological change far better than it does discontinuous change, so it fails to account for the most important innovations (Mowery and Rosenberg, 1979; Walsh, 1984).[8] A third angle addresses the arguments' assumptions concerning firm capabilities, expressing skepticism about (1) how effectively firms can identify "unrevealed needs" from an almost infinite set of possible human needs[9], (2) the extent to which firms in general have access to a large enough stock of techniques to address the variety of needs that could be expected to emerge[10], and (3) how far firms might vary from existing "routines" in order to satisfy unmet demands (Simon, 1959).[11]

Neither sufficient, both necessary. . . simultaneously

Science and technology push fails to account for economic factors, while demand-pull ignores technological capabilities. Following the critical responses to both arguments, weaker, and more nuanced, versions of each were used to support the claim that *both* supply and demand side factors are necessary to explain innovation. But it is not simply that

---

[8] Walsh (1984) used an approach similar to that of Schmookler (1962, 1966) to look at the chemical industry and found results consistent with his once a technology had matured; but in the early stages of development, scientific and technological breakthroughs appeared to be more important.

[9] A key assumption within the "recognition of needs", is that firms are aware of the directions that the market is pulling, even when those unfulfilled needs have not been clearly expressed. Dosi (1982) questioned, in particular how *discontinuous*' technical change could be assumed to occur through the recognition of needs.

[10] This suggests that firms have access to an almost infinite variety of techniques on which they can draw to satisfy consumers' changed preferences, thereby ignoring differences and limitations to "innovative capability" among firms (Dosi, 1982).

[11] Many of these critiques draw on theory developed by Simon (1959), who argued that firms tend not to stray far from "routines"; as a result they can be expected to maximize their future profits only within a narrower range of possibilities than is actually be available. He termed this type of optimizing behavior, "satisficing" and fit it within his broader notion of "bounded rationality."

both factors contribute; they also interact. Demand-pull and technology-push are “Necessary, but not sufficient, for innovation to result; both must exist simultaneously” (Mowery and Rosenberg, 1979). Similarly, Kleinknecht and Verspagen (1990) found statistical anomalies in the work of Schmookler (1962) that led them to a much weaker estimation of the role of demand; they too emphasized the role of the *combination* of demand-pull and technology-push. In a survey of 40 innovations, Freeman (1974) found that successful innovations showed the ability to connect, or “couple” a technical opportunity with a market opportunity. For example, Pavitt (1984) showed that industry specific attributes affect the relative importance of each. Often, adoption of one technology depends upon complementary innovations and the potential of one may stimulate investment in the other (Mowery and Rosenberg, 1989). Under this model, cumulativeness, networks, and feedback effects loom large. With this emphasis on interactions, the reduction of the innovation process to two causal factors proved limiting, and their use in the literature subsequently abated. Yet these terms continue to be invoked in policy debates over the allocation of public funds to stimulate innovation, particularly for energy technologies.

1.2.2 Application to policy

Application of this economic framework to policy decisions creates a taxonomy separating government actions that affect the size of the market for a new technology from those that influence the supply of new knowledge. Government can encourage innovation in two ways: it can implement measures that reduce the private cost of producing innovation, *technology-push*, and it can implement measures that increase the private payoff to successful innovation, *demand-pull*. It is important to consider that the arguments in the economics of innovation debate were descriptive, with a scope that included the whole economy, whereas the policy debates adopt a normative perspective and focus on the role of public sector actions, often for individual technologies. Consequently, the descriptive analysis of the past has focused on the *average* effect of various stimuli to innovation, while the prescriptive orientation of current policy debates is concerned with the *marginal* effects of policies.

Technology-push policy

Examples of policies that reduce the cost to firms of producing innovation include: government sponsored R&D, tax credits for companies to invest in R&D, enhancing the capacity for knowledge exchange, support for education and training, and funding demonstration projects. Critics of such policies note their mixed record of success (Cohen and Noll, 1991)[12], the possibility that public spending crowds-out private investment (Goolsbee, 1998; David *et al.*, 2000)[13], and their tendency to isolate scientific understanding from

---

[12] Cohen and Noll (1991) reviewed six large government R&D programs and found mixed results. The one definitive success was communications satellites. Of the three large energy R&D projects, the success of the photovoltaics program was considered ambiguous while the synfuels programs and breeder reactors programs were considered failures, costing several billion dollars each. They note that rapid program build-ups tended to lead to waste, yet they noted that politically, large R&D projects were “most salable” as a reaction to a near-term crisis.

[13] David *et al.* (2000) point out that the degree to which R&D crowding out is a problem is related to the flexibility of the labor supply of scientific and engineering personnel.

technical knowledge (Stokes, 1997).[14]

Demand-pull policy

Government actions also create incentives for firms to invest in innovation by raising the payoffs for successful innovations. Examples include: intellectual property protection, tax credits and rebates for consumers of new technologies, government procurement, technology mandates, regulatory standards, and taxes on competing technologies. The strongest and most established demand-pull policy instrument in the U.S. is the system of intellectual property protection which has its roots in Section 8 of the U.S. Constitution. It gave Congress the right:

> "To promote the Progress of Science and useful Arts, by securing for limited Times to Authors and Inventors the exclusive Right to their respective Writings and Discoveries" (U.S., 1790).

Here the government supports innovation by granting a temporary monopoly to inventors whose ideas are "novel, non-obvious, and useful" enough to qualify to receive a patent.

The importance of "post-adoption innovation," improvements that occur *after* a technology has entered into use, is often used to justify a demand-pull, rather than a technology-

---

[14] Responding to the 'linear' model embodied by Bush (1945b), Stokes (1997) rejected the basic research/applied research dichotomy put forth by Bush and illustrated that important previous innovations do not fit neatly in either category. He argued that the most dynamic areas are where scientific understanding and technical knowledge are linked, what he referred to as "Pasteur's Quadrant." The policy challenge for this model is how to increase investment in "use-inspired basic research".

push approach. Drawing on the work of Arrow (1962a) on learning-by-doing,[15] the claims made are that (1) opportunities to make technical improvements emerge from firms' experiences in manufacturing, (2) such improvements are uniquely available from experience and cannot be substituted for by R&D investments, and (3) these types of incremental improvements are important—they account for a substantial amount of cost-reducing and performance-enhancing improvements in many technologies.[16] For example, in the case of wind turbines, understanding the complex aerodynamics of a curved turbine blade moving through a perpendicularly-flowing medium, the location-specific turbulence that turbines cast on downwind turbines, as well as the longer-term effects of the ambient environment on component materials, have been important for improving the technology and yet have proven extremely difficult to replicate in a laboratory setting. The validity of using this argument to justify demand-pull policies hangs on the extent to which R&D can substitute for learning-by-doing and -using, as well as the productivity of each in stimulating innovation.[17]

---

[15] It is important to note that even though the paper by Arrow (1962a) is frequently cited as a formal claim that learning-by-doing matters, in the same paper he also proposed that learning-by-doing is subject to "sharply diminishing returns."

[16] These arguments are rooted in engineering-based studies (Wright, 1936; Alchian, 1963; Rapping, 1965), were formalized by Arrow (1962a) in his conception of "learning-by-doing" and later extended to include "learning-by-using" (Rosenberg, 1982b).

[17] Adopting this parlance, R&D activities are sometimes referred to as 'learning-by-searching' in economics (Malerba, 1992), management (Huber, 1991), as well as in energy policy modeling (Kouvaritakis *et al.*, 2000; Klaassen *et al.*, 2005).

A similar consensus for energy technology policy

Studies concerned with the effectiveness of energy technology policy reach a consensus, similar to that in the economics of innovation literature, that both types of instruments are necessary (Grübler *et al.*, 1999b; Norberg-Bohm, 1999, 2000; Requate, 2005a). Alic *et al.* (2003) explain that both are needed for the climate problem because the complex, uncertain, and iterative nature of the innovation process implies that different policies are needed at different stages of the innovation process. Goulder (2004) argues that economic theory supports using both types of policy strategies in parallel. Resting their arguments mainly on a precise discussion of the sources of market failure, Jaffe *et al.* (2005) also argue that both economic theory and empirical studies support the need for "Broad-based public support of technology innovation and diffusion."[18]

However, this consensus on the need for both types of policies provides only limited practical guidance because it fails to provide a basis for the allocation of public funds between the two. Where claims about allocation are made, studies acknowledge that optimal allocation is highly specific to individual technologies (Sagar and van der Zwaan, 2006) and even firms (Norberg-Bohm, 2000).[19] Attempts to econometrically identify the effects of demand-pull and technology-push, e.g. Kouvaritakis *et al.* (2000); Watanabe *et*

---

[18] Although it is important to consider that demand-pull and technology-push do not necessarily correspond to appropriability and environmental externality market failures, e.g. the reason for the patent system is to address the appropriability of R&D problem, but by granting a temporary monopoly to inventors, it is creating a demand-pull, rather than a technology-push, incentive.

[19] For example, Norberg-Bohm (2000) suggests that allocation between demand-pull and technology push should be based on: the size, strength, and risk of niche markets, industry structure, firm financial capability, firm technological capability, and the sources of innovation.

al. (2000); Miketa and Schrattenholzer (2004); Klaassen *et al.* (2005), have so far provided unsatisfying results because of their sensitivity to assumptions about the depreciation of R&D as a knowledge stock and about the lags between policy signals and decisions to innovate; both of these parameters have proven difficult to estimate empirically. This consensus also ignores the detailed characteristics of policy instruments, which are critical to their effectiveness in inducing innovation (Taylor *et al.*, 2005). In short, there has been substantially more work done on the much narrower problem of allocation *within* the set of demand-pull instruments (Jaffe *et al.*, 2002; Requate, 2005a) and *within* technology-push (Cohen and Noll, 1991; NRC, 2001) than *between* demand pull and technology push. The motivation behind this document is that allocation decisions can be informed through an improved understanding of the mechanisms of influence—both between public policies and innovation, and within the innovation process itself.

1.3 Organization of this Document

This document addresses two basic questions with respect to low-carbon energy technologies: (1) how can policy effectively drive innovation? And (2) more fundamentally, what are the characteristics of the innovation process? For the purpose of situating the work in this document, Fig. 1.5 shows a stylized representation of the innovation process and its interactions with policy.[20] At the core of this framework, based on the work of Schumpeter (1947), is the taxonomy—and sequential arrangement—of invention (the discovery of new scientific and technical knowledge), innovation (the use of that knowledge for practical ends), and diffusion (the widespread adoption of new techniques and devices). The sequential and cumulative aspects of the innovation process are central. And what was once considered a "linear" process—from invention to innovation to diffusion—was later acknowledged to include feedbacks, such that experiences in the later stages of the process, e.g. manufacturing, generate new knowledge that can be incorporated into subsequent products and processes (Rosenberg, 1969; Freeman, 1974). An important feature of these flows and feedbacks is that, especially for the most important new technologies, "lags are long," as new technologies must be adapted to engage with the larger technological system (Fagerberg *et al.*, 2004). Others have shown that very large-scale diffusion of technology often has unanticipated social and environmental impacts (Ruttan, 2001). In some cases, these impacts mobilize societal actors to attempt to alter the rate and direction of subsequent technological change (dotted lines). Following the discussion in Section 1.2, technology-push and demand-pull in Fig. 1.5 are situated adjacent to their most direct effects, e.g. demand-pull has its most immediate effects on the diffusion of technology. But the ultimate effects of policy actions may have pervasive effects on the activities of actors throughout the innovation process. This document is especially concerned with attempting

---

[20] Throughout this document, I adopt the terminology of Rosenberg (1982a) and define innovation as the process of creating cost-reducing and performance-enhancing improvements.

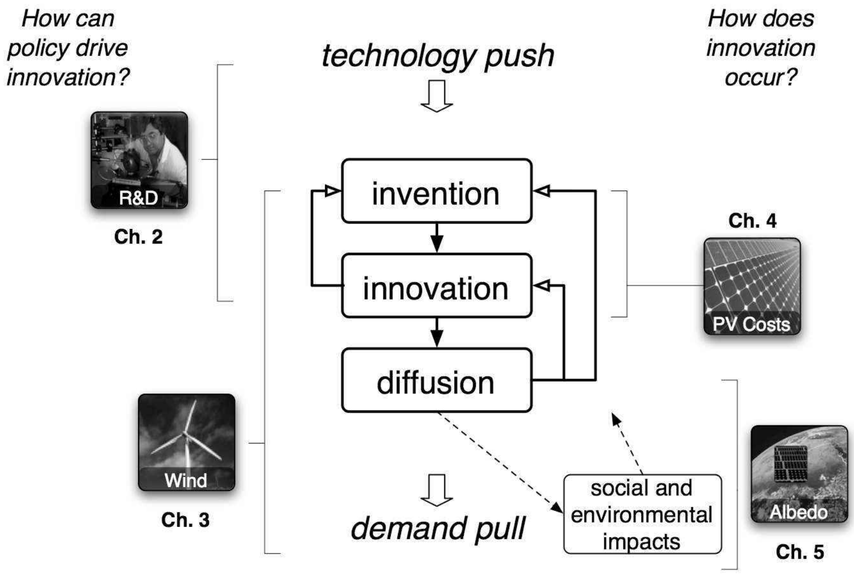

Figure 1.5: Framework for the interaction between policy and innovation. The four numbered studies within this dissertation are situated in this framework.

to understand some of these indirect effects.

In my dissertation research, I examined four distinct parts of the innovation process and its interactions with government activities. The following chapters present the results. Chapter 2, addressing Question 1 at the beginning of this section, analyzes the past several decades of R&D investment to examine the trends, effectiveness, and future prospects for the technology-push aspect of energy technology policy. The scope for that study is the entire U.S. energy sector. Investment in R&D has been decreasing, most notably in the private sector. A comparison of past federal R&D programs in all sectors showed that scaling up energy R&D to 5–10 times current levels over ten years—which one model implies would be adequate to address economic and environmental concerns—would fit easily within the range established by the major technology-push programs of the past sixty years.

Chapter 3 assesses the effectiveness of demand-pull in stimulating innovation using a case study of wind power in California. In that case, demand-pull policies effectively stimulated both diffusion of the technology and post-adoption improvements. However, patenting activity appears to have declined precipitously just as policy-led demand for wind power increased. The chapter explores possible explanations for this lack of responsiveness by inventors to demand-pull, including policy uncertainty, lags in investment payoffs, and trade-offs associated with the convergence on a dominant design.

In Chapter 4, I decomposed the innovation process for solar photovoltaics (PV) to understand quantitatively the factors driving cost reductions; the cost of PV has declined more than that of any other energy technology. Empirical data are assembled to populate a simple engineering-based model identifying the most important factors affecting the cost of PV over the past three decades. The results indicate that learning from experience only weakly explains change in the most important cost-reducing factors: plant size, module efficiency, and the cost of silicon. I point to other explanatory variables that may need to be included in future models.

Chapter 5 examines a potentially adverse impact of widespread technology adoption in assessing whether very large-scale deployment of PV might lower the earth's albedo and contribute to global warming. The results indicate that the avoided radiative forcing due to substitution of PV for fossil fuels is on the order of 50 times larger than that due to the albedo effect. While the size of this albedo effect, and its robustness to uncertainty in the assumptions, are not of concern, the results do suggest that expanding the scope for targeting technical improvements beyond cost reductions and conversion efficiency may provide new opportunities for enhancing PV's value as a mitigation technology. For example, evaluating future technology options based on their net radiative forcings may offer different conclusions than optimizing based on least cost.

In Chapter 6 the results of Chapters 2 through 5 are summarized and some specific implications of these findings for policy makers are discussed. Some broader observations that are supported by the full set of studies are introduced. The primary thrust of that section is that the case studies point out how difficult it is to use policy to create incentives for private actors to make risky investments that take years to pay off. Two main culprits are (1) the fragile credibility of government commitments and (2) the tendency of political compromises to produce implementation details that undermine long-term incentives. The discussion points to the need to devise a more sophisticated way to manage the trade off between flexibility and commitment in policy making, if long-term climate targets are to be relied upon to stimulate innovation in the energy sector.

Chapter 2

Technology Push:

U.S. Energy Research and Development

2.1 “A High-Priority National Need”

The notion of technology-push introduced in Chapter 1 holds that advances in science and technology are an essential part of the innovation process because they create new technological opportunities.[1] But technology push, on its own, is not sufficient to drive innovation. Freeman (1974) observed that successful innovations need to “connect” a technological opportunity with a market opportunity. Mowery and Rosenberg (1979) argued that both demand-pull and technology-push must exist simultaneously. More recently, Yang and

---

[1] Portions of this chapter were drawn from an article published as:
Nemet, G.F. and D.M. Kammen (2007) “U.S. energy R&D: declining investment, increasing need, and the feasibility of expansion” *Energy Policy* 35(1): 746-755.

Oppenheimer (2007) criticized efforts to focus on technology-push because they ignore "consumers' preferences" and because the federal government has a poor track record of "picking winning technologies." Still, investment in pre-commercial efforts to understand and improve technologies, which are typically referred to as research and development (R&D), has played an important role in a variety of fundamentally important technologies (Ruttan, 2006b,a).

Support for R&D is especially important in the energy sector to address long-term challenges such as climate change, where the incentives created by demand-pull policy instruments on their own may be insufficient to motivate sufficient investment in early-stage technology development (Jaffe *et al.*, 2005). The need for R&D support may be especially acute for nascent technologies, e.g. energy storage, and infrastructure-based technologies, whose benefits private investors may find particularly difficult to appropriate. The challenges of renewing the U.S. energy infrastructure to enhance economic and geopolitical security (Cheney, 2001) and prevent global climate change (Kennedy, 2004) are particularly acute, and depend on the improvement of existing technologies as well as the invention, development, commercial adoption of emerging ones (Holdren, 2006). Meeting these challenges also depends on the availability of tools to both effectively manage current energy technology investments, and to permit analysis of the most effective approaches and programs to significantly expand the resource of new energy technologies (Gallagher *et al.*, 2006; Moore *et al.*, 2007).

The federal government allocates over $100b annually for research and development (R&D) and considers it a vital "investment in the future" (Colwell, 2000). Estimates of the percent of overall economic growth that stems from innovation in science and technology are as high as 90% (Mansfield, 1972; Evenson *et al.*, 1979; Griliches, 1987; Solow, 2000). Fig. 1.3 shows that the social return to R&D consistently exceeds the private return. The low investment and large challenges associated with the energy sector however, have led numerous expert groups to call for new commitments to energy R&D. A 1997 report from the President's Committee of Advisors on Science and Technology and a 2004 report from the bipartisan National Commission on Energy Policy each recommended doubling federal R&D spending (PCAST, 1997; Holdren *et al.*, 2004). The importance of energy has led several groups to call for much larger commitments (Schock *et al.*, 1999; Davis and Owens, 2003), some on the scale of the Apollo Project of the 1960s (Hendricks, 2004). These recommendations build on other studies in the 1990s that warned of low and declining investment in energy sector R&D (Dooley, 1998; Morgan and Tierney, 1998; Margolis and Kammen, 1999b). More recently, the need for large public commitment to support energy R&D has emerged prominently as a public issue (Revkin, 2006; Friedman, 2007).

These concerns however lie in stark contrast with recent budget authorizations. Although the Bush administration lists energy research as a "high-priority national need" (Marburger, 2004) and points to the energy bill passed in the summer of 2005 as evidence of action, the 2005 federal budget *reduced* energy R&D by 2 percent from 2004 and the

2006 budget contained a mere 2% increase (AAAS, 2006). The American Association for the Advancement of Science projects a decline in federal energy R&D of 18 percent by 2009 (AAAS, 2004). Initial analyses of the 2007 budget show encouraging signs, with a 13% rise in funding (Gallagher *et al.*, 2007). Nuclear power is by far the largest beneficiary of this increase, with fission and fusion together accounting for 70% of the increase in funding. Despite these positive developments, the U.S. Government itself finds these funding levels inadequate. In December 2006, after a review of Department of Energy (DOE) budgets, the General Accounting Office concluded:

> "It is unlikely that DOE's current level of R&D funding or the nation's current energy policies will be sufficient to deploy alternative energy sources in the next 25 years that will reverse our growing dependence on imported oil or the adverse environmental effects of using conventional fossil energy... To meet the nation's rising demand for energy, reduce its economic and national security vulnerability to crude oil supply disruptions, and minimize adverse environmental effects, the Congress should consider further stimulating the development and deployment of a diversified energy portfolio by focusing R&D funding on advanced energy technologies" (GAO, 2006).

Low investment in R&D also plagues the private sector. Investments in energy R&D by U.S. companies fell by 45 percent between 1991—the beginning of restructuring in the energy sector—and the most recent year for which data are available, 2003 (Wolfe, 2006).

This rapid decline is especially concerning because commercial development is the critical step to turn laboratory research into economically viable technologies and practices (Mowery, 2006).[2] In either an era of declining energy budgets, or in a scenario where economic or environmental needs justify a significant increase in investments in energy research, quantitative assessment tools, such as those developed and utilized here, are needed (NRC, 2001).

The objective of this chapter is to gain insight on four key questions related to energy R&D investment in the U.S. To do this, I first address two descriptive questions:

1. What are the historical trends in U.S. energy R&D investment in both the public and private sectors?
2. Is there evidence of innovation outcomes from this investment?

I then respond to two questions that involve more direct normative implications for policy makers:

3. Can socially optimal R&D investment levels be estimated?
4. How do these levels compare to previous social investment priorities?

Correspondingly, this chapter consists of four parts: analysis of R&D investment data, development of indicators of innovative activity, estimation of a target levels for energy

---

[2] See the ‘valley of death’ discussion in PCAST, “Report to the President on Federal Energy Research and Development for the Challenges of the Twenty-First Century” (Office of the President, 1997), section 7-15 (PCAST, 1997).

R&D, and assessment of the feasibility of expanding funding to reach these larger levels of R&D. First, time-series records of investments in U.S. energy R&D are compiled to assemble a 30-year record (Figure 2.1) (Jefferson, 2001; Meeks, 2004; Wolfe, 2004). Complementing the data on public sector expenditures, a database of private sector R&D investments was constructed for fossil fuels, nuclear, renewables, and other energy technologies.[3] In addition, U.S. patent classifications are used to evaluate the innovation resulting from R&D investment in five emerging energy technologies. Second, three methods for using patents to assess the effectiveness of this investment were developed: patenting intensity, highly-cited patents, and citations per patent. Third, a target level for energy R&D funding is estimated building on both theoretical and empirical approaches from earlier work. Finally, a 60-year historical data set of major U.S. R&D programs is assembled and the fiscal impacts and macro-economic effects of a large energy R&D program relative to those programs are then assessed.

2.2 Declining R&D Investment

The U.S. invests about $1 billion less in energy R&D today than it did a decade ago. This trend is remarkable, first because the levels in the mid-1990s had already been identified as dangerously low (Margolis and Kammen, 1999a), and second because, as this analysis in-

---

[3] This data set is available at:
`http://ist-socrates.berkeley.edu/~gnemet/RandD2006.html`. It is also provided here as an appendix in Section 2.A.

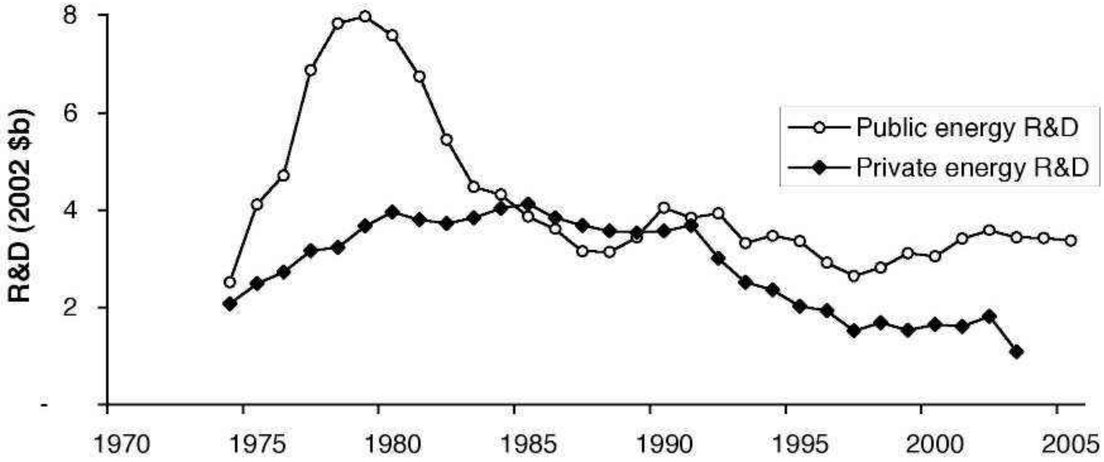

Figure 2.1: Energy R&D investment by public and private sectors.
The percentage of total R&D in the U.S. invested in energy technology has fallen from 10% to 2%. These time series are derived from federal budgets and from surveys of companies conducted by the National Science Foundation (Jefferson, 2001; Meeks, 2004; Wolfe, 2004).

dicates[4], the decline is pervasive—across almost every energy technology category, in both the public and private sectors, and at multiple stages in the innovation process, investment has been either stagnant or declining (Figure 2.2). Moreover, the decline in investment in energy has occurred while overall U.S. R&D has grown by 6% per year, and federal R&D investments in health and defence have grown by 10 to 15% per year, respectively (Figure 2.3). As a result, the percentage of all U.S. R&D invested in the energy sector has declined from 10% in the 1980s to 2% today (Figure 2.4). Private sector investment activity

---

[4] Energy R&D is decomposed into its four major components: fossil fuels, nuclear power, renewables and energy efficiency, and other energy technologies (such as environmental programs). While public spending can be disaggregated into more precise technological categories, this level is used to provide consistent comparisons between the private and public sectors. For individual years in which firm-level data is kept confidential, averages of adjacent years are used.

is a key area for concern. While in the 1980s and 1990s, the private and public sectors each accounted for approximately half of the nations investment in energy R&D, today the private sector makes up only 24%. The recent decline in private sector funding for energy R&D is particularly troubling because it has historically exhibited less volatility than public funding—private funding rose only moderately in the 1970s and was stable in the 1980s; periods during which federal funding increased by a factor of three and then dropped by half. The lack of industry investment in each technology area strongly suggests that the public sector needs to play a role in not only increasing investment directly but also correcting the market and regulatory obstacles that discourage investment in new technology (Duke and Kammen, 1999). The reduced inventive activity in energy reaches back even to the earliest stages of the innovation process, in universities where fundamental research and training of new scientists occurs. For example, a recent study of federal support for university research raised concerns about funding for energy and the environment as they found that funding to universities is increasingly concentrated in the life sciences (Fossum *et al.*, 2004).

A glimpse at the drivers behind investment trends in three segments of the energy economy indicates that a variety of mechanisms are at work. First, the market for fossil fuel electricity generation has been growing by 2 to 3% per year and yet R&D has declined by half in the past 10 years, from $1.5b to $0.7b. In this case, the shift to a deregulated market has been an influential factor reducing incentives for collaboration, and generating persis-

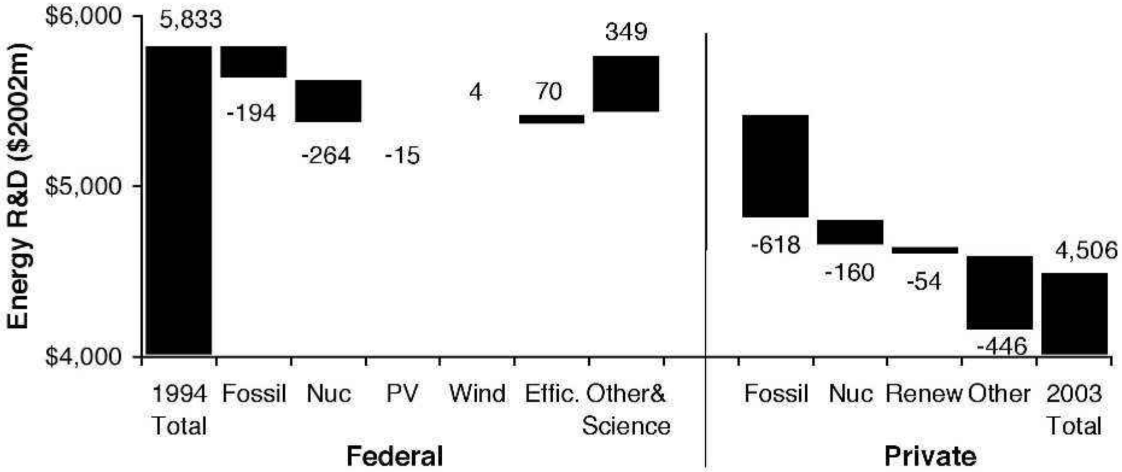

Figure 2.2: Changes in energy R&D investment by sector and technology 1994-2003.
The total change in R&D investment between 1994 and 2003 is disaggregated according to the contribution of each technology category and each sector. For example, of the $1,327m reduction in total energy R&D investment from 1994 to 2003, $618m was due to the decline in fossil fuel funding by the private sector.

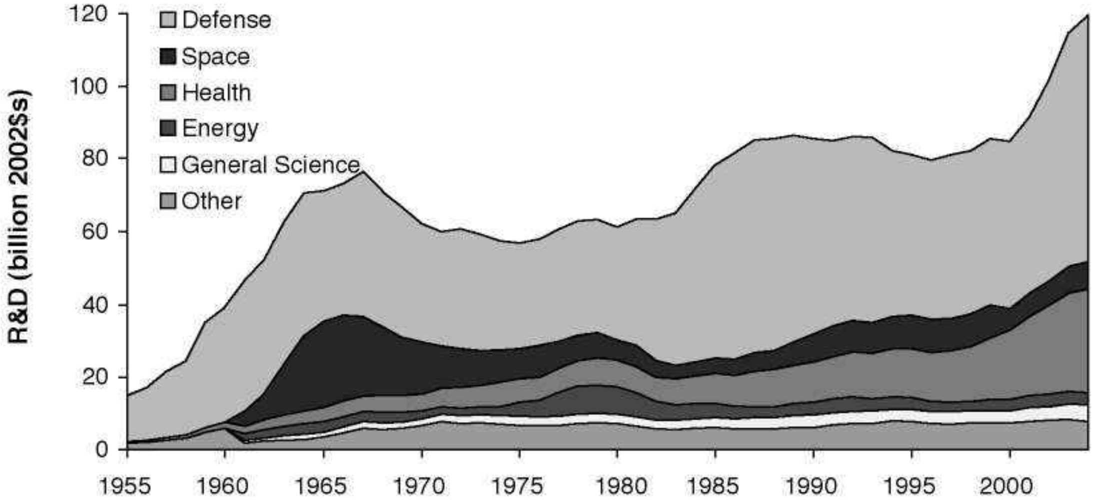

Figure 2.3: Federal R&D 1955 to 2004.
Annual level of R&D funding by federal agency.

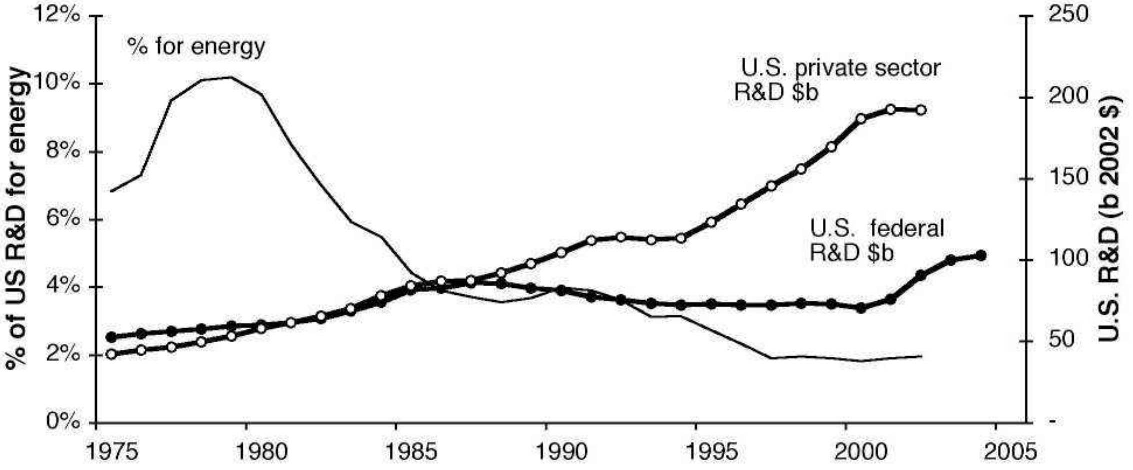

Figure 2.4: Total U.S. R&D and percentage devoted to energy.
Lines with circles indicate R&D investment levels in the U.S for all sectors. White circles show investment by companies and black circles federal government investment. Solid line indicates energy R&D spending as a percentage of total U.S. R&D spending.

tent regulatory uncertainty. The industry research consortium, the Electric Power Research Institute (EPRI), has seen its budget decline by a factor of three. Rather than shifting their EPRI contributions to their own proprietary research programs, investor-owned utilities and equipment makers have reduced both their EPRI dues and their own research programs. The data on private sector fossil R&D validate Dooley's prescient warnings in the mid-1990s (Dooley, 1998) about the effect of electricity sector deregulation on technology investment. Second, the decline in private sector nuclear R&D corresponds with diminishing expectations about the future construction of new plants. Over 90% of nuclear energy R&D is now federally funded. This lack of a "demand pull" incentive has persisted for so long that it even affects interest by the next generation nuclear workforce; enrollment in graduate-level nuclear engineering programs has declined by 26% in the last decade (Kammen, 2003). The Energy Policy Act of 2005 contained several incentives for new nuclear plant construction, for example, a production tax credit of 1.8 cents/kWh for 8 years, loan guarantees, and extension of federal indemnification for accident claims through 2025 (EPACT, 2005).[5] However, so far these incentives have not translated into renewed private sector technology investment. Third, policy intermittence and uncertainty plays a role in discouraging R&D investments in the solar and wind energy sectors which have been growing by 20 to 35% per year for more than a decade. Improvements in technology have made wind power competitive with natural gas (Jacobson and Masters, 2001; Junginger *et al.*, 2005) and have helped the global photovoltaic industry to expand by 50% in 2004 (Maycock, 2005). Yet, investment by large companies in developing these rapidly expanding technologies has actually declined. By contrast, European and Japanese are growing market share in this rapidly growing sector, making the U.S. increasingly an importer of renewables technology.

Venture capital investment in energy provides a potentially promising exception to the trends in private and public R&D. Energy investments funded by venture capital firms in the U.S. exceeded one billion dollars in 2000, and despite their subsequent cyclical decline to $520m in 2004, are still of the same scale as private R&D by large companies (Figure 2.5) (Prudencio, 2005; Makower *et al.*, 2007). Recent announcements, such as California's plan

---

[5] This last item is also known as the Price Anderson Act amendments.

to devote up to \$450m of its public pension fund investments to environmental technology companies and Pacific Gas and Electric's \$30m California Clean Energy Fund for funding new ventures suggest that a new investment cycle may be starting (Angelides, 2004). The tripling of clean energy venture capital investment that occurred in 2006 provides ample evidence of this renewed level of investment (Makower *et al.*, 2007). The emergence of this new funding mechanism is especially important because studies have found that in general, venture capital investment is 3 to 4 times more effective than R&D at stimulating patenting (Kortum and Lerner, 2000). While it does not offset the declining investment by the federal government and large companies, the venture capital sector is now a significant component of the U.S. energy innovation system, raising the importance of monitoring its activity level, composition of portfolio firms, and effectiveness in bringing nascent technologies to the commercial market.

Finally, the drugs and biotechnology industry provides a revealing contrast to the trends seen in energy. Innovation in that sector has been broad, rapid and consistent. The 5,000 firms in the industry signed 10,000 technology agreements during the 1990s, and the sector added over 100,000 new jobs in the last 15 years (Cortwright and Meyer, 2002). Expectations of future benefits are high—the typical biotech firm spends more on R&D (\$8.4 million) than it receives in revenues (\$2.5 million), with the difference generally funded by larger firms and venture capital (PriceWaterhouseCoopers, 2001). Although energy R&D exceeded that of the biotechnology industry 20 years ago, today R&D investment

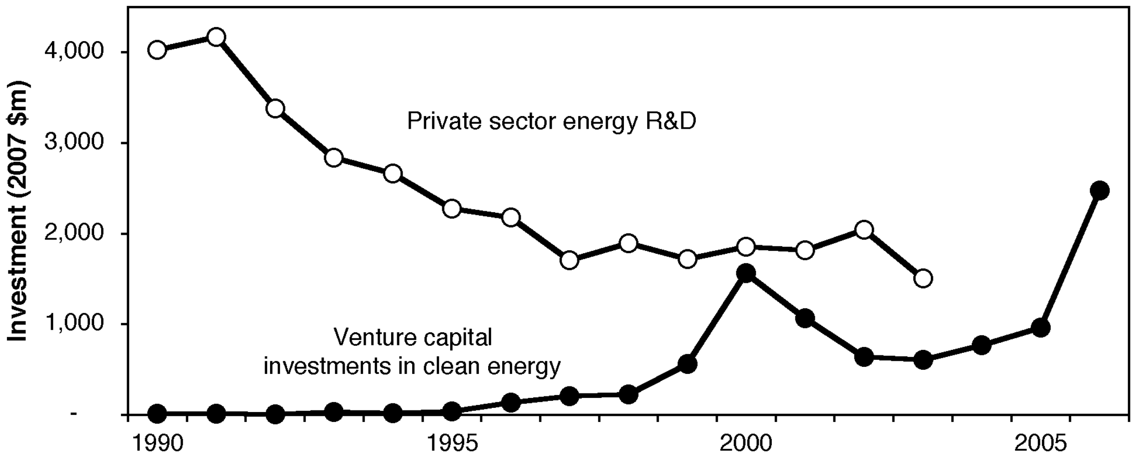

Figure 2.5: U.S. Venture capital investments in energy and private sector energy R&D.
R&D investment by companies (less than 500 employees) is compared to investment in emerging companies by venture capital firms.

by biotechnology firms is an order of magnitude larger than that of energy firms (Figure 2.6). In the mid-1980s, U.S. companies in the energy sector were investing more in R&D ($4.0 billion) than were drug and biotechnology firms ($3.4 billion), but by 2000, drug and biotech companies had increased their investment by almost a factor of 4 to $13 billion. Meanwhile, energy companies had cut their investments by more than half to $1.6 billion. From 1980 to 2000, the energy sector invested $64 billion in R&D while the drug and biotech sector invested $173b. Today, total private sector energy R&D is less than the R&D budgets of individual biotech companies such as Amgen and Genentech.

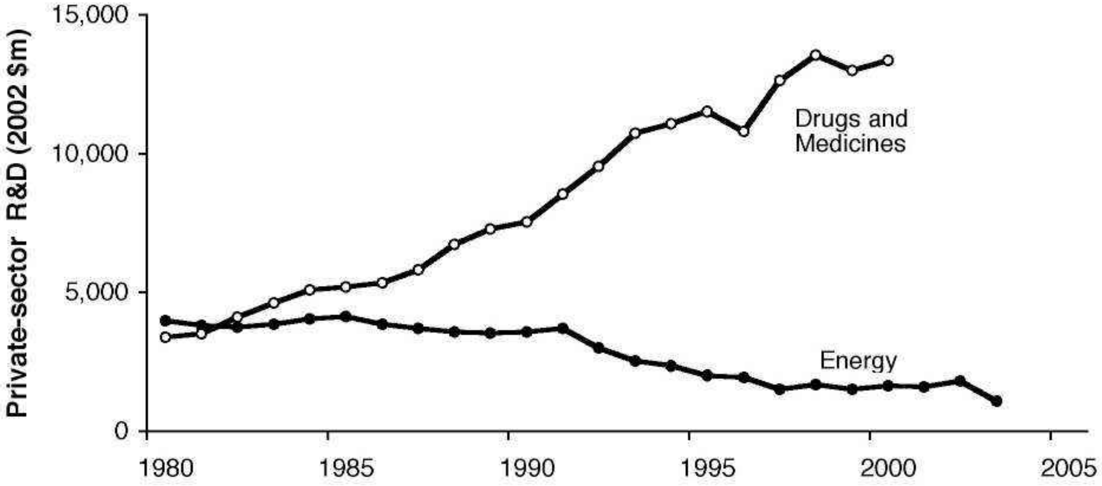

Figure 2.6: Private-sector R&D investment: energy vs. drugs and medicines. R&D investment by companies in the energy sector is compared to investment by those in the drugs and medicines sector.

2.3 Reductions in Patenting Intensity

Divergence in investment levels between the energy and other sectors of the economy is only one of several indicators of under-performance in the energy economy. This section presents results of three methods developed to assess patenting activity, which earlier work has found to provides an indication of the outcomes of the innovation process (Griliches, 1990).

First, records of successful U.S. patent applications are used as a proxy for the intensity of inventive activity and find strong correlations between public R&D and patenting across a variety of energy technologies (Figure 2.7).[6] Since the early-1980s all three indicators—

---

[6] Patents data were downloaded from the U.S. Patent and Trademark Office, “US Patent Bibliographic Database” `www.uspto.gov/patft/` (2004).

public sector R&D, private sector R&D, and patenting—have exhibited consistently negative trends.[7] Public R&D and patenting are highly correlated for wind, PV, fuel cells, bio-energy, and nuclear fusion. Nuclear fission is the one category that is not well correlated to R&D. Comparing patenting against private sector R&D for the more aggregated technology categories also reveals concurrent negative trends.[8] The long-term decline in patenting across technology categories and their correlation with R&D funding levels provide further evidence that the technical improvements upon which performance-improving and cost-reducing innovations are based are occurring with decreasing frequency.

Second, in the same way that studies measure scientific importance using journal citations (May, 1997), patent citation data can be used to identify "high-value" patents (Harhoff *et al.*, 1999). For each patent I identify the number of times it is cited by subsequent patents using the NBER Patent Citations Datafile (Hall *et al.*, 2001). For each year and technology category, I calculate the probability of a patent being cited by recording the number of patents in that technology category in the next 15 years. I then calculate the adjusted patent citations for each year using a base year. "High-value" patents are those that received twice as many citations as the average patent in that technology category. Between 5 and 10% of the patents I looked at fell under this definition of high-value. The Department of Energy accounts for a large fraction of the most highly cited patents, with a direct interest in 24% (6 of the 25) of the most frequently referenced U.S. energy patents, while only

---

[7] From 1980 to 2003, public R&D declined by 54%, private R&D by 67%, and patenting by 47%.

[8] While the general correlation holds here as well, the abbreviated time-series (1985-2002) and the constant negative trend reduce the significance of the results.

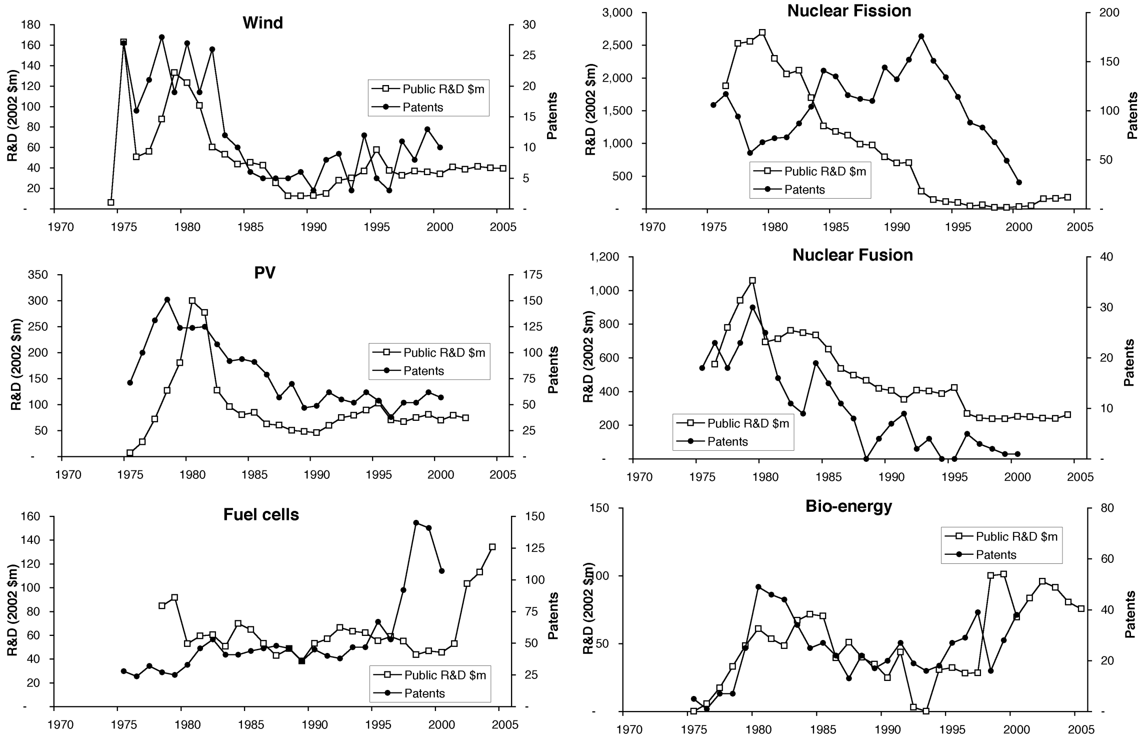

Figure 2.7: Patenting and federal R&D.
Patenting is strongly correlated with federal R&D. To provide comparisons with U.S. R&D funding, foreign patents are excluded. The data include granted patents in the U.S. patent system filed by U.S. inventors only. Patents are dated by their year of application to remove the effects of the lag between application and approval. This lag averages two years.

associated with 7% of total U.S. energy patents. In the energy sector, valuable patents do not occur randomly; they cluster in specific periods of productive innovation (Figure 2.8).[9] The drivers behind these clusters of valuable patents include R&D investment, growth in demand, and exploitation of technical opportunities. These clusters both reflect successful innovations, productive public policies, and mark opportunities to further energize emerging technologies and industries.

Third, patent citations can be used to measure both the return on R&D investment and the health of the technology commercialization process, as patents from government research provide the basis for subsequent patents related to technology development and marketable products. The difference between the U.S. federal energy patent portfolio and all other U.S. patents is striking, with energy patents earning on average only 68 percent as many citations as the overall U.S. average from 1970 to 1997 (Figure 2.9). This lack of development of government-sponsored inventions should not be surprising given the declining emphasis on innovation among private energy companies.

In contrast to the rest of the energy sector investment and innovation in fuel cells have grown. Despite a 17% drop in federal funding, patenting activity intensified by nearly an order of magnitude, from 47 in 1994 to 349 in 2001. Trends in patenting and the stock prices of the major firms in the industry are highly correlated (Figure 2.10). High stock prices create an opportunity for firms to raise capital, albeit at the expense of existing

---

[9] Analysis based on the citation weighting methodology of Dahlin *et al.* (2004).

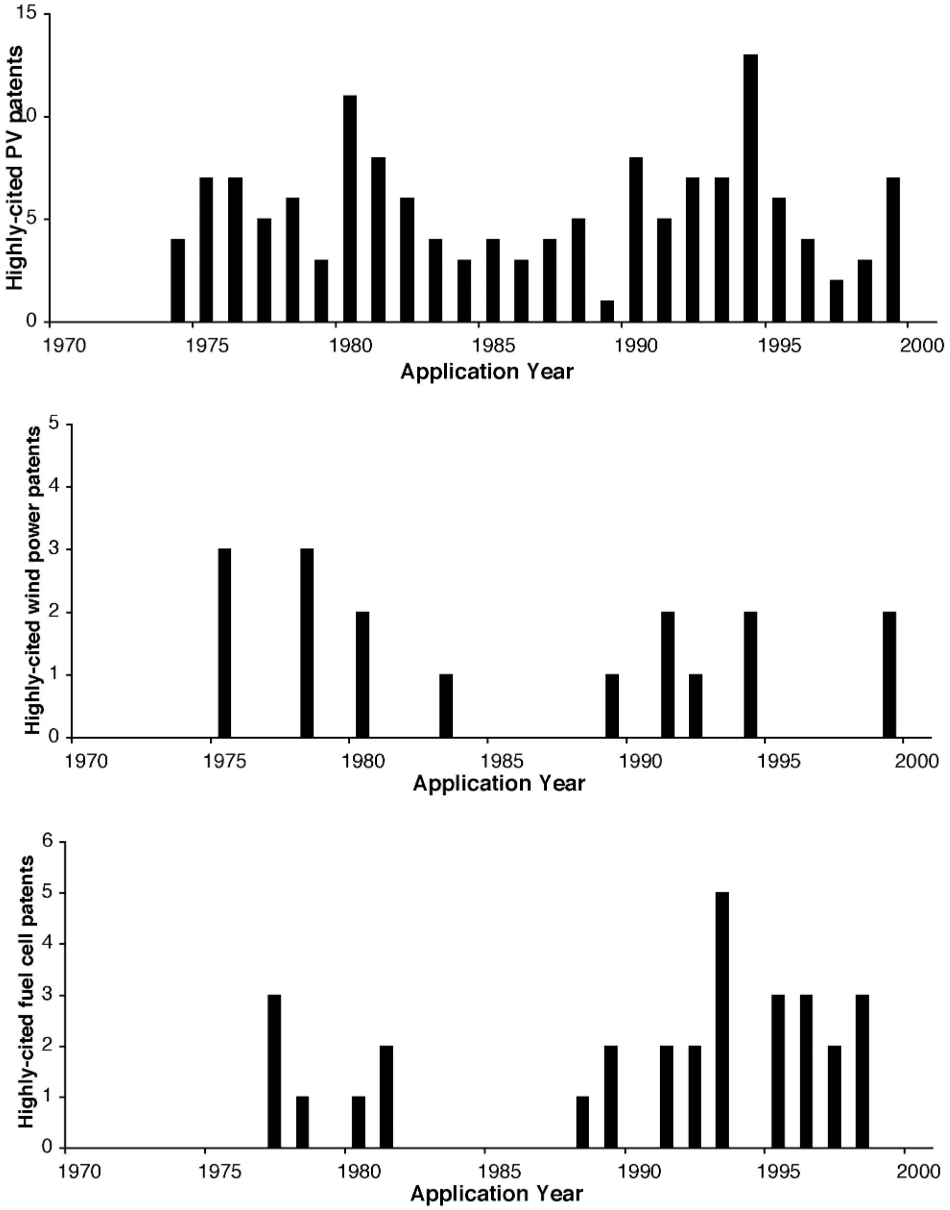

Figure 2.8: Highly-cited patents.
For each patent the number of times it is cited by subsequent patents is calculated. “High-value” patents are those that received twice as many citations as the average patent in that technology category. Between 5 and 10% of the patents examined qualified as high-value.

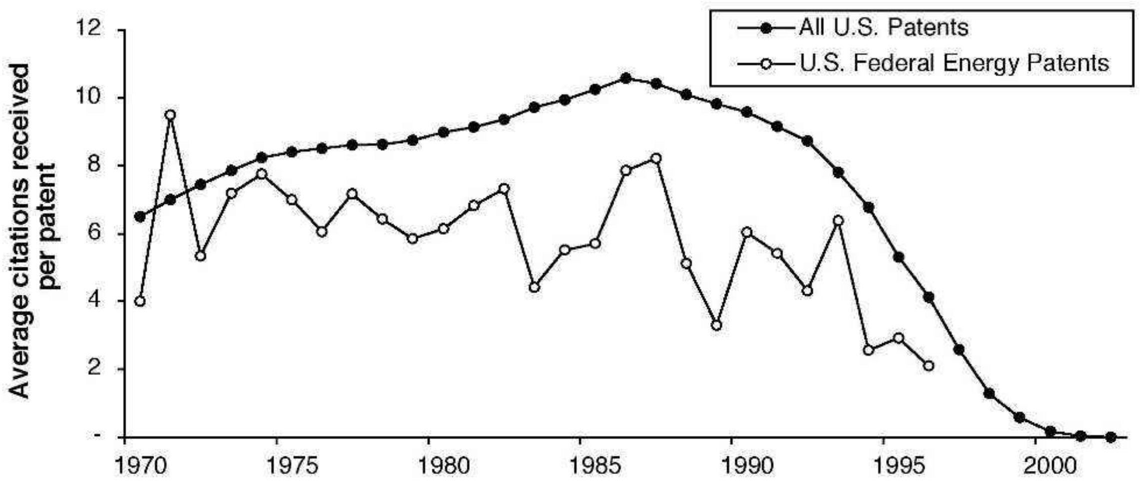

Figure 2.9: Average patent citations received per patent granted.
The y-axis indicates the average number of times a patent was cited by subsequent patents. The average of all patents filed during the year is shown on the x-axis. Recent patents, those issued within the past five years, were omitted because there has been insufficient time for them to accrue a citation history. In each decade, the average energy patent received fewer citations than the suite of all U.S. patents: 6.6 vs. 8.0 in the 1970s, 6.1 vs. 9.8 in the 1980s, and 4.3 vs. 7.4 in the 1990s. In aggregate, between 1970 and 2000 patents in the energy sector received one third fewer citations than did those across all fields.

shareholders, by issuing more stock. The relationship between fuel cell company stock prices and patenting is stronger than that between patenting and public R&D. The five firms shown account for 24 percent of patents from 1999 to 2004. Almost 300 firms received fuel cell patents between 1999 and 2004, reflecting participation both by small and large firms. This combination of increasing investment and innovation is unique within the energy sector. While investments have decreased as venture funding overall has receded since the late 1990s, the rapid innovation in this period industry has provided a large new stock of knowledge on which new designs, new products, and cost-reducing improvements can build. The industry structure even resembles that of the biotechnology industry. A large number of entrepreneurial firms and a few large firms collaborate through partnerships and intellectual property licensing to develop this earlier stage technology (Mowery, 1998). The federal government, therefore, need not be the only driver of innovation in the energy sector if private sector mechanisms and business opportunities are robust.

2.4 Estimating Energy R&D Investments Required

If energy R&D spending is low, then how much would be adequate? Here, I build on earlier empirical and theoretical work to arrive at a range of plausible scenarios for optimal levels of energy R&D and then gauge the feasibility of such a project using historical data. Calls for major new commitments to energy R&D have become common—while both the PCAST study of 1997 (PCAST, 1997) and the 2004 NCEP report (Holdren *et al.*, 2004)

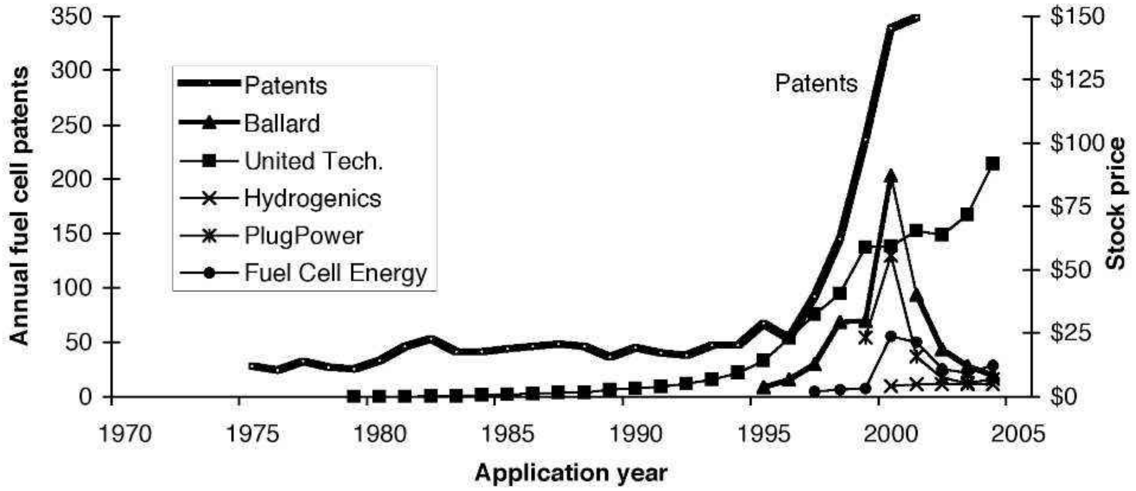

Figure 2.10: Fuel cell patenting and stock prices.
The relationship between fuel cell company stock prices and patenting is stronger than that between patenting and public R&D. The five firms shown account for 24% of patents from 1999 to 2004. 288 firms received fuel cell patents between 1999-2004.

recommend doubling federal energy R&D, others have found that larger increases are warranted. More recent work by Holdren (2006) argues that investment needs to rise by "at least 2–3-fold". Davis and Owens (2003) found that the option value of energy R&D justifies increasing spending to four times the present level. Schock *et al.* (1999) valued energy R&D by providing estimates of the insurance needed against oil price shocks, electricity supply disruptions, local air pollution, and climate change. By estimating the magnitude of the risks in each area and the probabilities of energy R&D programs to reduce them, they found that increasing energy R&D by a factor of four would be a 'conservative' estimate of its insurance value. Note that this estimate assumes a mean climate stabilization target of between 650 and 750 ppm CO_2 and incorporates a 35% probability that no stabilization at all will be needed. This possibility of no stabilization at all is especially concerning as it would potentially involve levels exceeding 1000 ppm CO_2 by the end of the century, with higher levels thereafter.

2.4.1 Model Description

The remainder of this section describes the methodology used to arrive at the scenarios for future energy R&D investment of 5- and 10-times current levels. A recalculation of the Schock et al. model to target the 550-ppm atmospheric level, increases the optimal R&D investment in energy R&D to 3 to 10 times the current level of investment. Figure 2.11 shows the probability distribution assumed by (Schock *et al.*, 1999) and the target value of 550 ppm (vertical dashed line) used in this analysis. Uncertainty in the optimal level is indeed large. To incorporate the range of these estimates, I develop two scenarios for scaling up energy R&D, one for five times the current level and one for ten times.

The model devised by Schock et al. establishes an "insurance value" of federal energy R&D. It is based on assessing risk mitigation due to R&D for four types of energy-related risks. The non-climate risks are discussed at the end of this section. The value of R&D for mitigating climate change is calculated according to the following:

The value of R&D for the U.S. (V_{US}) is the product of the climate mitigation savings derived from R&D programs (S), the assumed probability of R&D success (P), and the probability of needing to achieve each stabilization level (L). These values are summed for

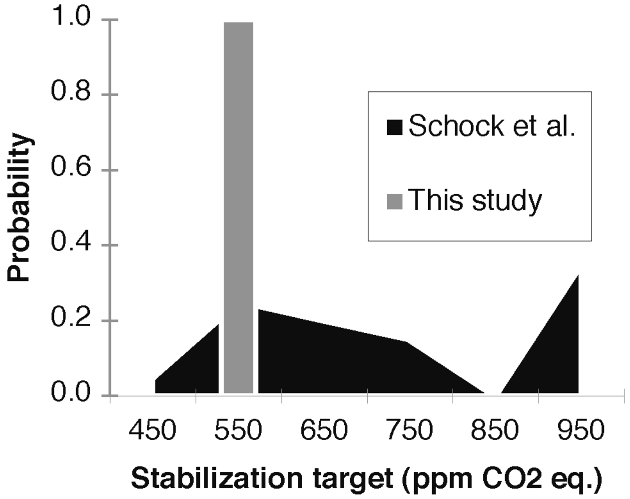

Figure 2.11: Comparison of probability distribution for climate stabilization used by Schock *et al.* (1999) with the 550 ppm target used in this analysis.

each stabilization level (i) and multiplied by the contribution to worldwide climate R&D by the U.S. (A).

$$\mathrm{V_{US}} = \mathrm{A}\sum_{i=1}^{5}(\mathrm{S_iP_iL_i}) \tag{2.1}$$

Like Schock *et al.* (1999), I assume that the contribution to worldwide R&D by the U.S. (A) is in proportion to its current share of worldwide greenhouse gas emissions, approximately 25%.

The subscript, i represents 5 greenhouse-gas stabilization levels: 450 ppm, 550 ppm, 650 ppm, 750 ppm, and the case of no stabilization.

The probabilities (L) of needing to stabilize at each level i, are used as shown in the figure above. For the Schock *et al.* (1999) model these are: 0.05 at 450 ppm, 0.25 at 550 ppm, 0.2 at 650 ppm, 0.15 at 750 ppm, and 0.35 for the case of no stabilization. In contrast to the probability density function they use, I select the doubling of pre-industrial levels as the target and thus assign the level i = 550ppm a “probability” of 1.

I use the values developed by Schock *et al.* (1999) for the assumed probability of R&D success (P). These probabilities decrease with stabilization levels, under the assumption that lower stabilization will require larger contributions from early-stage technologies whose ultimate viability is less likely than near-term options. The range for 550 ppm is 0.5 to 0.8. I use both ends of this range to bound the estimate.

For each stabilization level i, the climate mitigation savings derived from R&D programs (S) is the difference between the costs to stabilize using the outcomes of a successful R&D program (CRD) and the costs to stabilize without the R&D program (C).

$$\mathrm{S_i = C_i - CRD_i} \tag{2.2}$$

I use the costs to stabilize (C) calculated by Schock *et al.* (1999), who used the MiniCAM 2.0 model applied to two sets of mitigation scenarios, those by Wigley *et al.* (1996) and the IPCC Nakicenovic *et al.* (2000). The cost to stabilize at 550 ppm is in the range of \$0.9 to \$2.4 trillion. It is important to note that these scenarios already include technology improvement, although they do not specify how much R&D is implied to achieve this “autonomous” improvement. As Schock *et al.* (1999) point out, if any of this assumed improvement depends on higher levels of R&D, the estimates calculated in this model will then underestimate the R&D required.

The costs to stabilize using the outcomes of a successful R&D program (CRD) are lower because the energy technologies developed in the R&D program can be used to offset greenhouse gas emissions at lower costs than using existing technologies. I use the assumption by Schock *et al.* (1999) that a successful R&D program will enable us to deploy technologies that produce energy at costs similar to business-as-usual costs while reducing emissions sufficient to stabilize at the 550 ppm level.

2.4.2 Data comparison

Table 2.1 below shows the values used in the model. In this version of the model I use the same values as Schock *et al.* (1999) for the 550 ppm level. The one exception is the probabilities assumed for the needing to achieve each stabilization level (L). This model is conditional on a stabilization target of 550 ppm, because I am deriving the amount of R&D required to achieve a specific target. In contrast, Schock *et al.* (1999) treat the stabilization level as an uncertain parameter with a known probability density function.

2.4.3 Outcomes and other risks

In this model, the total required spending was discounted and annualized to arrive at estimates for the required amount of annual federal energy R&D to stabilize atmospheric concentrations of CO_2 at 550 ppm. I arrive at a range of \$6 to \$27 billion in 2005 dollars.

Table 2.1: Comparison of parameter values used in the R&D models

Study:	This study	Schock *et al.* (1999)				
Stabilization level (ppm):	550	550	450	650	750	None
Cost to stabilize without R&D (C) $trillions	0.9–2.4	0.9–2.4	3.7–4.5	0.3–1.3	0.2–0.5	0
Cost to stabilize with R&D (CRD) $trillions	0	0	0.4	0	0	0
Savings from R&D (S) $trillions	0.9–2.4	0.9–2.4	3.3–4.1	0.3–1.3	0.2–0.5	0
Probability of R&D success (P)	0.5–0.8	0.5–0.8	0.1	1.0	1.0	–
Probability of needing to achieve stabilization level (L)	1.0	0.25	0.05	0.2	0.15	0.35
U.S. share of worldwide R&D (A)	0.25	0.25	0.25	0.25	0.25	0.25
Discount rate	0.05	0.05	0.05	0.05	0.05	0.05

Finally, note that in their model, Schock *et al.* (1999) show that energy R&D can be used as insurance against other risks as well, such as oil price shocks, electricity outages, and air pollution. Using energy R&D to mitigate these risks has an annual value of $9 to $10 billion. The figures above are perhaps overly conservative in that they assume that the R&D programs launched to address climate stabilization perfectly overlap with the programs used to address these other risks. In their analysis, Schock et al. examined the federal energy R&D budget and found that about half of the spending consists of programs that are relevant to more than one of the risks. So a more realistic estimate would be to assume that half of the other risks would be addressed by the climate R&D program and half would not. For example, investments to improve the reliability of the electricity grid would reduce damages due to power outages but would not necessarily be included in a large climate R&D program. In that case, optimal energy R&D would rise to $11 to $32 billion per year, or roughly 3 to 10 times current levels. I used this result to devise the scenarios that I use in this study—5x and 10x energy R&D. I compared investment in these scenarios to that of the large R&D programs of the past.

2.5 Comparison to Previous Large R&D Programs

Given persistently low levels of funding, how feasible would it be to raise investment to levels commensurate with the energy-related challenges we face? The performance of previous large-scale R&D programs provides a useful test of the viability of carrying out an energy ‘Apollo’ or ‘Manhattan’ project, as these ventures are often termed. I find that a 5- to 10-fold increase in spending from current levels is not a ‘pie in the sky’ proposal; in fact it is consistent with the growth seen in several previous federal programs, each of which took place in response to clearly articulated national needs. Past experience indicates that this investment would be repaid several times over in technological innovations, business opportunities, and job growth, beyond the already worthy goal of developing a low-carbon economy. I assembled data and reviewed spending patterns of the six previous major federal R&D initiatives since 1940 (Table 2.2 and Fig. 2.12) and use five measures to compare them to scenarios of increasing energy R&D by factors of five and ten. For each of these eight programs I calculate a “baseline” level of spending. The difference between the actual spending and the baseline during the program I call *extra* program spending. I compare the energy scenarios to the other initiatives using five measures that address both the peak year and the full duration of the program. A 10x expanded energy investment scenario is within the range of the previous programs in all but one measure, where it exceeds by 10%. A 5x energy scenario is in the lower half of the range for each measure. Figure 2.13 shows the scenarios (as circles) plotted against the range of previous programs. While expanding energy R&D to five or ten times todays level would be a significant initiative, the fiscal magnitude of such a program is well within the range of previous programs, each of which have produced demonstrable economic benefits beyond the direct program objectives.

A critical role for public sector investment has always been to energize and facilitate

Table 2.2: Comparison of energy R&D scenarios and major federal government R&D initiatives (2002 $b)

			Peak Year		Program Duration		
Program[a]	Sector	Years	Spending	Increase	Spending	Extra Spending[b]	Factor Increase
Manhattan Project	Defence	1942–45	$10.0	$10.0	$25.0	$25.0	n/a
Apollo Program	Space	1963–72	$23.8	$19.8	$184.6	$127.4	3.2
Project Independence	Energy	1975–82	$7.8	$5.3	$49.9	$25.6	2.1
Reagan defence	Defence	1981–89	$58.4	$27.6	$445.1	$100.3	1.3
Doubling NIH	Health	1999–04	$28.4	$13.3	$138.3	$32.6	1.3
War on Terror	Defence	2002–04	$67.7	$19.5	$187.1	$29.6	1.2
5x energy scenario	Energy	2005–15	$17.1	$13.7	$96.8	$47.9	2.0
10x energy scenario	Energy	2005–15	$34.0	$30.6	$154.3	$105.4	3.2

---

[a] “Major R&D initiatives” in this study are federal programs in which annual spending either doubled or increased by more than $10 billion during the program lifetime.

[b] For each of these eight programs I calculate a “baseline” level of spending based on the 50-year historical growth rate of U.S. R&D, 4.3% per year. The difference between the actual spending and the baseline during the program I call extraordinary or, “extra”, program spending.

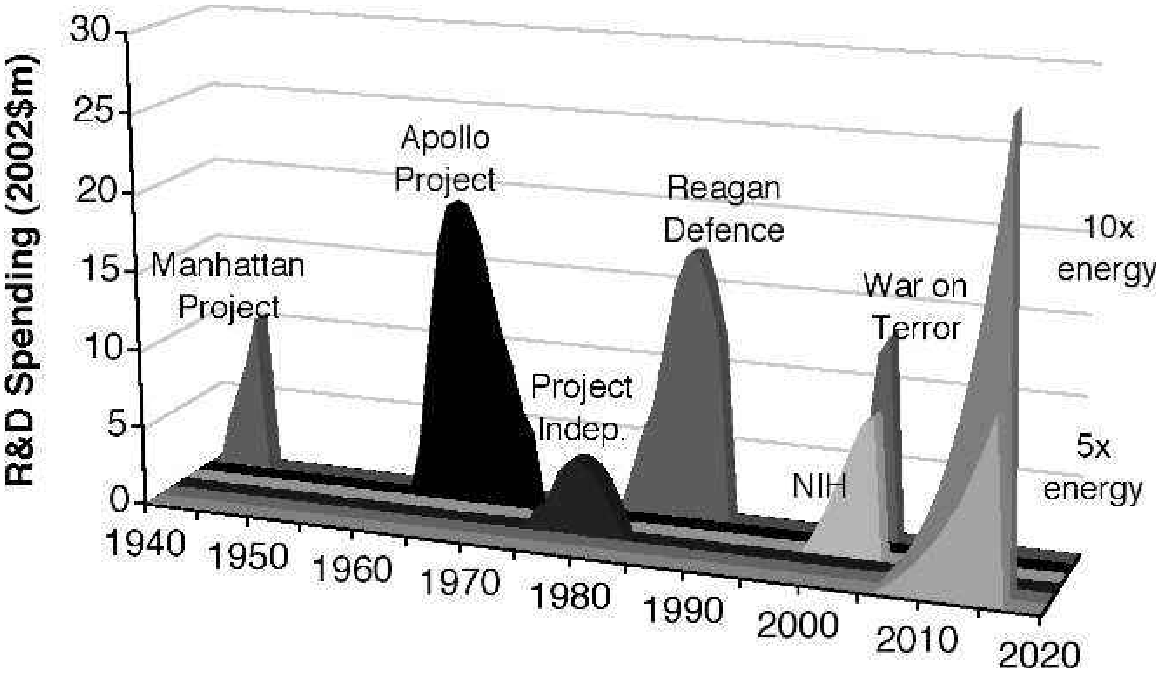

Figure 2.12: Comparison of energy R&D scenarios and major federal government R&D initiatives (2002 $b)

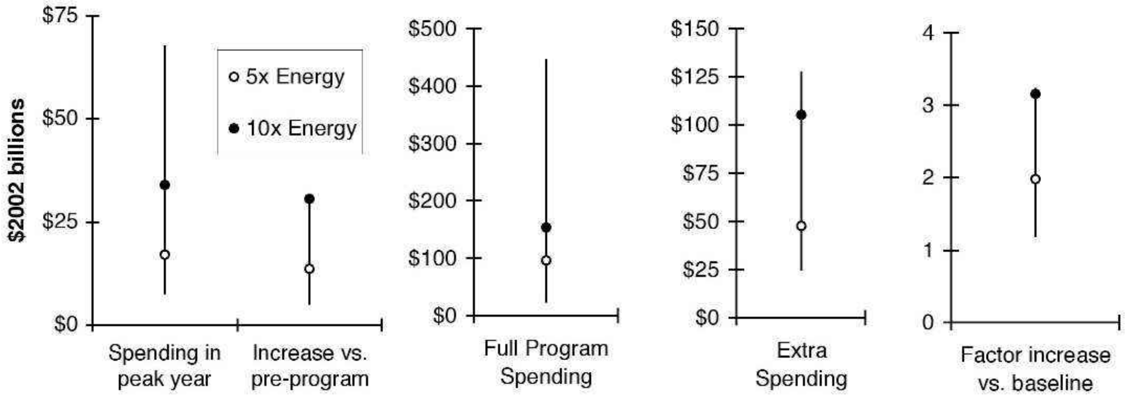

Figure 2.13: Energy R&D scenarios plotted against the range of previous programs.
For each of the five measures, the vertical line represents the range of values exhibited by the previous large federal R&D programs. The white circle (○) indicates the value for a 5x energy R&D scenario and the black dot (●) for a 10x energy scenario.

private sector activity. In fact, increasing energy R&D investment in the private sector by a factor of five or ten would not even rival what is seen in other high-technology sectors. From 1988 to 2003 the U.S. energy industry invested only 0.23% of its revenues in R&D. This compares to the period 1975 to 87 when private sector R&D averaged 1.1%, peaking at 1.4% in 1978. Overall R&D in the US economy was 2.6% of GDP over that time and has been increasing. High-tech industries such as pharmaceuticals, software, and computers routinely invest between 5 and 15% of revenues in R&D (MIT, 2002). An order of magnitude increase in R&D investments by the energy industry would still leave the energy sector's R&D intensity below the average of 2.6% for U. S. industry as a whole (BEA, 2004; Wolfe, 2004). If the electric power industry alone were to devote 2% of revenue to R&D for the next decade, the resulting $50 billion would exceed cumulative energy R&D invested since the 1970s, yet would be smaller than cumulative profits of $168 billion from 1994 to 2003 (Kuhn, 2004) and would be dwarfed by the $1.7 trillion forecast to be spent on new equipment and upgrades in the North American power sector from 2001 to 2030 (Birol, 2003). The confluence of this upcoming capital investment and a federal programmatic initiative and commitment would enable new capacity to make full use of the technologies developed in a research program and would provide opportunities for incorporating market feedback and stimulating learning effects.[10] Given recent investment declines in the private sector, creating an environment in which firms begin to invest at these levels will be an important policy challenge.

2.5.1 Crowding out

I also examined the thesis that these large programs "crowd out" other research and using the data described in this study, found that the evidence for this contention is weak.[11] In fact, large government R&D initiatives were associated with *higher* levels of both private sector R&D and R&D in other federal programs. The economy-wide effects of such major R&D programs could arguably be either negative or positive. The positive macro effects

---

[10] It is important to note that this analysis does not suggest that energy utilities should necessarily be asked or expected to make this investment without strong assurance that public sector investment will itself increase, but more critically that these investments will be facilitated by regulation and incentives that reward research into clean energy technologies and practices.

[11] This finding is consistent with that of David *et al.* (2000), a meta-analysis of empirical R&D crowding-out studies that found little evidence of crowding out across a wide array of sectoral and macro-economic studies.

of R&D accrue from two types of “spillovers”: firms do not capture the full value of their innovations (Jones and Williams, 1998) and indirect benefits emerge, such as the 10:1 benefit ratio of the Apollo program (Apollo-Alliance, 2004). Assuming that the value of the direct outcomes of an R&D program exceed investment, the main negative consequence of large R&D programs is that they may crowd out R&D in other sectors by limiting these other sectors access to funding and scientific personnel (Goolsbee, 1998). The R&D data described above can be used to develop a simple model relating these six major federal R&D programs to R&D spending in other areas, both in the public and private sectors. I test two aspects of the crowding-out hypothesis: First, whether large federal programs are associated with reduced spending in *other federal R&D*, and second, whether these programs lead to lower spending in *private sector R&D*. In a model of spending on other federal R&D activities, I controlled for GDP and found that the coefficient for the targeted R&D effort is small, positive, and significant (see Table 2.3). I found a similar result in a model explaining private R&D.

These data on private R&D extend only to 1985, and therefore do not go back far enough to test for significant results. However, a glance at R&D trends in both energy and biotech show that private investment rose during periods of large government R&D increases. One interpretation of these results is that the signal of commitment that a large government initiative sends to private investors outweighs any crowding-out effects associated with competition over funding or retention of scientists and engineers. Another is

Table 2.3: The effect of large R&D programs on other R&D investment. Dependent variable represents programs susceptible to "crowding-out."

Dependent Variable	Independent variables[a]				
Model 1[b]					
ln(otherfedR&D)	ln(programR&D)	ln(GDP)	constant	R^2	n
	0.03*	0.43*	3.35	0.87	31
	(0.01)	(0.03)	(0.06)		
Model 2[c]					
privateR&D	program	GDP	mean	R^2	n
	7.40*	25.8*	-87.2	0.99	28
	(2.31)	(0.60)	(5.22)		

---

[a] An asterisk indicates that coefficient is significant at the 95% level.

[b] Data Definitions for Model 1:
otherfedR&D: Annual spending on programs other than those being emphasized.
programR&D: Extra-normal annual spending on a *large* government R&D programs.
GDP: Annual level of U.S. Gross Domestic Product.

[c] Data Definitions for Model 2:
privateR&D: Annual U.S. R&D spending by the private sector.
program: Dummy variable for which 1 means a large R&D program was under-way.

that in these long-term programs, the stock of scientists and engineers is not fixed. Just as the dearth of activity in the nuclear sector has led to decreased enrollment in graduate programs, a large long-term program with a signal of commitment from public leaders can increase the numbers of trained professionals within a few years. These results suggest that the crowding-out effect of previous programs was weak, if it existed at all. Indeed these results indicate the opposite of a crowding-out effect: large government R&D initiatives are associated with higher levels of both private sector R&D and R&D in other federal programs.

2.5.2 Conclusion

The decline in energy R&D and innovative activity seen over the past three decades is pervasive. While government funding is essential in supporting early stage technologies and sending signals to the market, evidence of private sector investment is an important indicator of expectations about technological possibilities and market potential. The dramatic declines in private sector investment are thus particularly concerning if we are to employ an innovation-based strategy to confront the major energy-related challenges society now faces. R&D alone is not sufficient to bring the new energy technologies we will require to widespread adoption. However, the correlations reported here demonstrate that R&D is an essential component of a broad innovation-based energy strategy that includes transforming markets and reducing barriers to the commercialization and diffusion of nascent technologies. The evidence observed from past programs indicates that we can effectively scale up energy R&D, without hurting innovation in other sectors of the economy. At the same time, such a large and important project will require the development of additional ways of assessing returns on investments to inform the allocation of support across technologies, sectors, and the multiple stages of the innovation process.

Appendix 2.A R&D Data

The following tables provide the data assembled for this chapter.

Table 2.4: Total United States R&D, 1975–2004 ($2002m)

Year	Total U.S.	Federal	Company and other
1975	95,042	52,780	42,262
1976	99,399	54,686	44,713
1977	102,805	56,100	46,706
1978	107,744	57,834	49,911
1979	113,047	59,569	53,478
1980	117,985	60,027	57,958
1981	123,218	61,621	61,597
1982	129,731	64,067	65,664
1983	139,083	68,808	70,274
1984	152,304	74,313	77,991
1985	165,743	81,601	84,141
1986	169,935	82,818	87,117
1987	173,347	86,032	87,314
1988	177,508	85,531	91,977
1989	180,707	82,811	97,896
1990	185,956	81,264	104,692
1991	189,301	77,332	111,969
1992	189,735	75,622	114,113
1993	185,658	73,351	112,307
1994	185,633	72,182	113,451
1995	196,811	73,166	123,644
1996	207,017	72,272	134,745
1997	217,918	72,216	145,702
1998	229,423	73,326	156,097
1999	242,870	72,974	169,896
2000	257,740	70,611	187,130
2001	268,571	75,974	192,597
2002	283,014	90,770	192,244
2003	n/a	100,062	n/a
2004	n/a	102,884	n/a

Table 2.5: United States Energy R&D, 1974–2005 ($2002m)

Year	Total U.S.	Federal	Company and other
1974	-	2,509	-
1975	-	4,117	-
1976	-	4,706	-
1977	-	6,868	-
1978	-	7,831	-
1979	-	7,968	-
1980	-	7,591	-
1981	-	6,742	-
1982	-	5,456	-
1983	-	4,480	-
1984	-	4,322	-
1985	7,993	3,866	4,127
1986	7,448	3,607	3,842
1987	6,838	3,142	3,695
1988	6,716	3,139	3,577
1989	6,961	3,428	3,533
1990	7,617	4,047	3,570
1991	7,539	3,844	3,694
1992	6,938	3,940	2,998
1993	5,830	3,316	2,514
1994	5,833	3,475	2,358
1995	5,371	3,355	2,016
1996	4,842	2,908	1,934
1997	4,153	2,638	1,515
1998	4,489	2,810	1,678
1999	4,635	3,111	1,524
2000	4,682	3,036	1,646
2001	5,011	3,401	1,609
2002	5,392	3,580	1,812
2003	4,506	3,425	1,081
2004	-	3,418	-
2005	-	3,361	-

Table 2.6: Public-sector Energy R&D by Technology, 1974–2005 ($2002m)

Year	Fossil	Nuclear	Renewable	Other	Non-DoE	Total
1974	291	1,220	63	737	198	2,509
1975	942	1,685	299	713	477	4,117
1976	1,053	1,855	536	750	511	4,706
1977	1,493	2,670	1,276	820	609	6,868
1978	1,717	2,716	1,729	1,002	667	7,831
1979	1,538	2,500	2,120	1,190	619	7,968
1980	1,532	2,332	1,921	1,186	619	7,591
1981	1,252	2,205	1,660	988	637	6,742
1982	739	2,221	937	1,074	485	5,456
1983	533	1,868	712	961	405	4,480
1984	543	1,646	661	1,090	382	4,322
1985	549	1,268	549	1,169	332	3,866
1986	682	1,090	560	994	281	3,607
1987	445	957	482	975	283	3,142
1988	526	911	416	1,051	235	3,139
1989	771	896	404	1,117	241	3,428
1990	1,158	770	381	1,545	193	4,047
1991	955	709	482	1,557	141	3,844
1992	928	755	558	1,547	151	3,940
1993	401	722	613	1,343	237	3,316
1994	670	577	719	1,320	188	3,475
1995	423	655	770	1,342	165	3,355
1996	501	381	644	1,257	126	2,908
1997	320	382	627	1,245	64	2,638
1998	309	318	699	1,387	97	2,810
1999	323	327	763	1,608	91	3,111
2000	348	358	746	1,494	91	3,036
2001	412	339	800	1,766	85	3,401
2002	509	347	825	1,808	92	3,580
2003	476	313	779	1,761	97	3,425
2004	448	378	712	1,799	82	3,418
2005	380	375	693	1,833	81	3,361

Table 2.7: Private-sector Energy R&D by Technology, 1985–2003 ($2002m)

Year	Fossil	Nuclear	Renewable	Other	Total
1985	2,837	308	418	564	4,127
1986	2,649	198	405	590	3,842
1987	2,473	213	393	617	3,695
1988	2,309	242	381	645	3,577
1989	2,155	147	484	748	3,533
1990	2,338	176	308	748	3,570
1991	2,478	154	528	535	3,694
1992	1,957	191	498	352	2,998
1993	1,466	169	315	564	2,514
1994	1,290	161	301	607	2,358
1995	1,173	153	235	455	2,016
1996	1,143	146	205	439	1,934
1997	975	139	214	187	1,515
1998	1,143	42	222	271	1,678
1999	1,218	36	229	41	1,524
2000	1,297	31	236	82	1,646
2001	1,138	23	236	212	1,609
2002	1,123	21	291	377	1,812
2003	672	1	247	161	1,081

Table 2.8: U.S. Federal Government R&D by budget function, 1955–1979 ($2005m).

Year[a]	Defense	Health	Space	Nat. Res. &Env.	Energy	General Science	Other	Total
1955	12,896	402	1,889	-	-	-	-	15,186
1956	14,687	481	2,144	-	-	-	-	17,311
1957	18,622	784	2,603	-	-	-	-	22,009
1958	20,785	968	3,237	-	-	-	-	24,990
1959	30,027	1,259	4,886	-	-	-	-	36,172
1960	32,550	1,626	6,076	-	-	-	-	40,252
1961	36,921	2,135	4,095	385	1,966	722	1,518	47,742
1962	37,634	2,865	7,347	562	2,329	972	1,789	53,498
1963	39,945	3,221	14,468	617	2,650	1,266	2,125	64,291
1964	39,674	3,689	21,491	679	2,894	1,404	2,265	72,096
1965	36,539	3,942	24,321	791	2,911	1,513	2,712	72,730
1966	36,466	4,355	24,078	915	2,782	1,824	3,711	74,131
1967	40,206	4,295	22,426	1,502	2,816	1,920	4,421	77,587
1968	37,252	4,596	19,375	1,490	2,958	1,967	4,038	71,676
1969	35,837	4,666	16,293	1,385	2,560	1,857	4,478	67,077
1970	32,507	4,415	14,688	1,385	2,338	1,841	5,315	62,489
1971	31,459	4,996	11,823	1,614	2,157	1,990	6,261	60,299
1972	33,094	5,751	10,900	1,781	2,134	2,324	5,346	61,330
1973	31,698	5,581	9,944	1,951	2,218	2,317	5,444	59,153
1974	29,126	6,684	8,729	1,667	2,660	2,212	5,166	56,242
1975	28,564	6,404	8,157	1,842	4,316	2,105	4,799	56,187
1976	29,100	6,559	8,733	1,906	4,914	2,080	4,693	57,985
1977	31,122	6,897	7,429	1,975	7,139	2,137	4,822	61,521
1978	31,616	7,275	7,204	2,216	8,250	2,005	5,103	63,669
1979	31,216	7,698	7,098	2,286	8,450	1,917	5,186	63,852

---

[a] Values for 1955–2005 are “actual”, those for 2006 are “preliminary”, and those for 2007 are “proposed”.

Table 2.9: U.S. Federal Government R&D by budget function, 1980–2007 ($2005m).

Year[a]	Defense	Health	Space	Nat. Res. &Env.	Energy	General Science	Other	Total
1980	31,009	7,664	5,681	2,073	8,034	2,000	5,239	61,699
1981	34,929	7,343	5,902	2,013	6,806	2,378	4,625	63,995
1982	39,457	6,917	4,620	1,725	5,779	2,036	4,033	64,568
1983	42,889	7,392	3,670	1,637	4,764	2,253	4,076	66,683
1984	48,548	7,922	3,813	1,596	4,625	2,432	4,353	73,289
1985	54,210	8,716	4,384	1,704	4,138	2,701	4,405	80,256
1986	58,115	8,758	4,555	1,671	3,862	2,683	4,158	83,803
1987	59,990	10,045	5,207	1,736	3,386	2,889	4,191	87,443
1988	59,410	10,484	5,457	1,719	3,395	2,955	4,153	87,572
1989	58,053	11,097	6,503	1,792	3,726	3,115	4,423	88,708
1990	54,870	11,418	7,923	1,905	4,421	2,637	4,482	87,656
1991	52,230	12,253	8,646	2,101	4,176	3,245	4,866	87,517
1992	52,006	13,053	8,755	2,191	4,303	3,243	5,242	88,792
1993	52,344	13,045	8,868	2,286	3,595	3,216	5,328	88,681
1994	46,927	13,660	9,213	2,562	3,757	3,182	5,608	84,909
1995	45,302	13,890	9,639	2,421	3,616	3,248	5,648	83,765
1996	45,174	14,182	9,374	2,153	3,121	3,293	5,220	82,517
1997	46,542	14,895	9,221	2,217	2,840	3,409	5,108	84,232
1998	46,300	15,784	9,531	2,156	3,025	3,146	5,590	85,533
1999	47,340	17,825	9,450	2,111	3,349	3,322	5,582	88,979
2000	47,760	20,043	6,015	2,242	3,268	3,430	5,475	88,233
2001	50,071	22,737	6,710	2,295	3,661	3,767	5,786	95,027
2002	57,073	25,362	6,750	2,325	3,854	3,768	5,962	105,094
2003	67,216	29,201	10,210	2,319	3,687	5,187	6,747	124,568
2004	72,319	29,279	10,020	2,307	3,612	5,354	6,924	129,815
2005	74,641	29,129	9,656	2,245	3,531	5,270	6,787	131,259
2006	75,701	28,365	10,152	2,147	3,611	5,082	6,787	131,845
2007	74,796	27,695	10,952	1,949	4,271	4,970	5,946	130,580

---

[a] Values for 1955–2005 are “actual”, those for 2006 are “preliminary”, and those for 2007 are “proposed”.

Chapter 3

Demand Pull: Policies, Diffusion, and Improvements in California Wind Power

3.1 Creating Markets for Innovation

Despite the debates outlined in Chapter 1 about whether demand-pull or technology-push policy instruments are more effective at stimulating innovation, the notion that policy can induce investment—and consequent improvements—in technologies by creating markets for them enjoys support from a wide range of disciplinary perspectives. If, as discussed in Chapter 1, the government must play a central role in creating incentives for the private sector to invest in low-carbon innovation, and demand-pull policies are to be used, then proper allocation of public funds for these programs requires estimation of the extent to which demand-pull policy instruments create sufficient incentives to meet public goals. For example, California is currently developing plans for a far-reaching policy, a cap-and-trade program for restricting greenhouse gas emissions (Nunez, 2006). The 'cap' component of the scheme would use an enforceable limit to reduce emissions over time to a more socially-desirable level. The 'trade' component would reduce the cost of meeting the target level by giving all regulated entities access to the least costly greenhouse gas abatement measures through the creation of a market for emissions permits.

One of the anticipated benefits of a cap and trade system is that it will provide strong incentives for investments in technological innovation (Burtraw *et al.*, 2005). Empirical studies have used the observed technological improvements in pollution abatement systems after implementation of the pollution credit trading programs for sulfur dioxide and nitrogen oxides to argue that cap-and-trade provided stronger incentives for innovation than previous regimes, which afforded polluters less flexibility (Ellerman and Harrison, 2003; Popp, 2003). But the relevant question in this context is not whether cap-and-trade regulation provides stronger incentives than less flexible regulation, but rather, does cap-and-trade regulation by itself provide enough incentives to induce sufficient innovation to affordably meet the emissions targets? And although the SO_2 and NO_x cases provide valuable insight, they are not entirely applicable to the CO_2 case, because they involve changes in inputs or end-of-pipe additions with no changes to the existing infrastructure, rather than the development and diffusion of completely new technological systems. While it may be feasible to meet California's greenhouse gas emissions targets for 2010, and even 2020, through incremental efficiency improvements to the existing energy system, meeting the target for 2050—80% reductions from 1990 levels—cannot be met with incremental improvements to existing technologies (Hanemann and Farrell, 2006). Non-incremental changes to the energy system will be needed to meet those targets.

This distinction is important because, as the literature on innovation frequently points out, there are qualitative differences between incremental and non-incremental technological change, particularly in how each responds to external influences (Freeman and Soete, 1997). Incremental improvements arise from the continuous accumulation of small changes. Non-incremental, or 'radical', technologies involve the establishment of "new connections"; they are discrete, discontinuous events, usually involving deliberative effort; and they may have only a minor relatedness to existing products (Garcia and Calantone, 2002; Dahlin and Behrens, 2005). A prominent stand of this literature has argued that, in their continuing "search" for better techniques, technical change tends to occur as incremental change within a dominant design (Benner and Tushman, 2003); firms adhere to "routines" (Simon, 1959), which may restrict their efforts to innovate to improvement toward a local, rather than a global optimum (Nelson and Winter, 1977, 1982). Different types of incentives, or perhaps even different types of firms, are needed for firms to commit substantial resources to radically new technologies that may require competencies outside the standard domain. This view argues that the incentives needed for incremental and non-incremental vary by more than simply their stringency (Kemp, 1997, 2000).

There is not yet sufficient historical evidence from cap-and-trade programs for greenhouse gases to measure their impact on innovation. However, there is ample experience with other demand-pull programs. Moreover, analysis of programs that apply to electricity production in general, rather than to specific pollutants, is promising because such programs are open to a much broader set of technological solutions, and thus may offer additional insights. For example, the history of demand-pull policies for wind power in California encompasses a wide variety of demand-pull incentives. These policy instruments are similar to the cap and trade programs in that they do not involve mandates to use specified technologies, and also because they offer rewards for high performance. In this case the demand pull hypothesis is examined by looking more deeply into the various aspects of the innovation process that demand-pull policies affected. The goal is to arrive at a richer, qualitative, understanding about how these demand-pull mechanisms interact with the innovation process and possibly to identify limits on their effectiveness.

3.1.1 The case of wind power in California

This study uses the case of wind power because the technology has been commercially available for decades and has undergone substantial technological change. In the context of informing technology policy related to climate change, wind power is an appealing technology case for three reasons. First, wind power is a potentially important non-greenhouse gas emitting energy source. Worldwide power production from wind has been growing at over 20% per year (Martinot, 2005) and prominent scenarios of future energy supply assume large amounts of wind deployed in the future (Nakicenovic and Riahi, 2002).[1] Second, wind power has a long history as a commercially available technology—it has been deployed on the scale of tens of megawatts in the U.S. since the 1850s, mainly for mechanically pumping water. Third, technological change has been dramatic. Capital costs for wind turbines have declined by a factor of five over the past two decades (Gipe, 1995; IEA, 2002; AWEA, 2004; Junginger *et al.*, 2005), bringing wind power from a hobby of eccentrics to cost-competitiveness with conventional sources of electricity generation such as natural gas-fired power plants. The range of inventions in wind power has occurred across nearly all components—including blade shapes and materials, tower designs, ball bearings, generators, electronic controls, and foundations (McGowan and Connors, 2000). Siting of turbines to optimize wind energy captured, avoid avian collisions, and improve aesthetics has also been an important area of improvement.

California is chosen as the geographical bound because during the period of interest it had active policymaking and was the focus of global wind power activity. The state's government has consistently implemented the most comprehensive set of energy policies

---

[1] A review of 34 emissions scenarios developed by IIASA and WEC found a median assumption of 18 exajoules of electricity production from wind in 2050, equivalent to approximately 1.5 terawatts of installed capacity, or close to 40% of current worldwide electricity generation capacity.

of any state, often overriding legislation passed by the federal government. Its share of the world wind power market in the 1970s and 1980s ($> 90\%$) was so overwhelming that the influence of demand from other parts of the U.S. and the world is considered negligible. In order to assess the effects of demand in California, the focus of the analysis is on the period from 1975 to 1991.

3.1.2 Chapter outline

This chapter proceeds with a description of the three hypotheses to be evaluated. A methodology for evaluating these hypotheses is subsequently described in Section 3.2. Section 3.3 presents results for each of the hypotheses. Finally, Section 3.4 offers a discussion of the main finding—that the period of most aggressive demand-pull policies is associated with a precipitous decline in the most valuable patents.

3.2 Hypotheses and Measurement

The application of the demand-pull hypothesis to technology policy encompasses a series of arguments: (1) that policy can enhance demand for a technology, (2) that increasing expected future demand raises the payoff to successful innovation, and (3) that higher payoffs stimulate efforts to improve the technology. Using the case of California wind power, this study assesses three hypotheses related to the effect of *demand pull* policies on distinct aspects of the innovation process:

H1: Demand-side policies increased the profitability of wind power and stimulated *diffusion* of the technology.

H2: Policy-led diffusion created opportunities for *learning-by-using*.

H3: Policy-led demand created expectations of larger future markets, raising incentives for investments in patentable *inventions*.

A series of analyses are assembled to evaluate these hypotheses.

3.2.1 Measuring the strength of demand-pull

First, the history of policies relevant to wind power is documented and their effect on the profitability of wind power as an investment is calculated. This study uses the difference between the cost of wind power and the price at which utilities purchase power as an estimate of the profitability of wind power projects. Expected future demand for wind power is assumed to be a function of its profitability. Since demand-pull incentives are driven by expectations about future demands, the evaluation of the strength of demand-side policy focuses on the extent to which policies reduce this difference. Appendix 3.A describes a simple model used to calculate the effect of the policies implemented on the levelized cost of wind power.[2] Appendix 3.B provides a description of each.

---

[2] An important caveat, is that this model represents the average for installed turbines in California. Operators which had access to the windiest sites and the cheapest capital, as well as those which operated their

3.2.2 Measuring diffusion

Second, *diffusion* of the technology is measured in both megawatts of installed capacity and investment in constant dollars. Widespread diffusion is the ultimate goal of innovation in environmentally beneficial technologies. Without it, the social benefits from the accumulation of scientific knowledge and technical know are unrealized. Three measures of diffusion are relevant to wind power: turbines installed, capacity installed (MW), and electricity produced (kWh). Installed capacity is a more comprehensive measure of social benefits than the number of turbines installed. Electricity produced is the most comprehensive measure of diffusion in wind power because, from a carbon-emissions perspective, we are concerned with electricity from wind displacing electricity generated from carbon-emitting sources, such as gas and coal. This study decomposes this comprehensive measure into intermediate two parts: *new installations*, measured in both megawatts (MW) of capacity and investment ($), is used to represent diffusion, while the residual factors affecting electricity production are taken up next.

3.2.3 Measuring learning-by-using

Third, evidence of *learning-by-using* is obtained using a time series of *capacity factors* for California wind turbines. The concept of learning-by-doing originates from observations that workers in manufacturing plants became more efficient as they produced more units

---

turbines most efficiently, were able to achieve costs below this average.

(Wright, 1936; Alchian, 1963; Rapping, 1965). The roots of these micro-level observations can be traced back to early economic theories about the importance of the relationship between specialization and trade, which were based in part on individuals developing expertise over time (Smith, 1776). Drawing on the concept of *learning* in psychological theory, Arrow (1962a) formalized a model explaining technical change as a function of learning derived from the accumulation of experiences in production. Rosenberg (1982b) distinguished between learning-by-*doing*, in which a producer improves a good through knowledge gained through accumulated experience in manufacturing, and learning-by-*using* in which consumers of a good increase its productivity by making changes to the way they operate it, based on insights gleaned from their experience in using it. In this case, the good of interest is a wind turbine, and the relevant experience is that which operators acquire in *using* wind turbines, so the analysis focuses on learning-by-using rather than learning-by-doing. Capacity factor captures improvements in the choice of turbine siting, wind farm configuration, and operation and maintenance activities, which are difficult to replicate in laboratory or even demonstration settings.

$$\text{capacity factor} = \frac{\text{electricity produced (kWh/year)}}{\text{(capacity installed (kW))(hours/year)}} \tag{3.1}$$

The complex aerodynamics of a curved turbine blade moving through a perpendicularly-flowing medium, the location-specific turbulence that turbines cast on downwind turbines, as well as the longer-term effects of the ambient environment on component materials, are difficult to replicate in a laboratory or even demonstration setting. As a result, raising capacity factor depends heavily on the improvements that accrue through experience, or learning-by-using.

3.2.4 Measuring patenting

Fourth, three measures of patenting activity are used to assess the intensity and characteristics of inventive activity (see Appendix 3.C). The economics of innovation literature has used patents extensively as a measure of inventive activity (Griliches, 1990; Watanabe *et al.*, 2001; Jaffe and Trajtenberg, 2002; Hall *et al.*, 2005; Popp, 2005), although debates about what exactly they measure remain unresolved. Patents provide an attractive way to measure inventive activity for several reasons: comprehensive data are publicly available, the technical characteristics are described in detail, the definition of what constitutes a patent in the U.S. has changed little for over 200 years, and every patent is categorized by experts using a standard classification scheme (Griliches, 1990; Watanabe *et al.*, 2001; Jaffe and Trajtenberg, 2002; Hall *et al.*, 2005; Popp, 2005). Studies have revealed that patent counts are an imperfect way to represent the rate of invention; all patents are not equally important, not all inventions are patentable, firms use alternative means to protect their intellectual property, and sometimes they patent strategically (Harhoff *et al.*, 1999; Bessen, 2005). In interviews in previous work on this case, industry experts generally agreed with the statement, "Patents represent the major innovations in the California wind industry" (Taylor and Nemet, 2006). Patent citation frequency is used here to address issues about heterogeneity in the quality of patented inventions. This study's relatively modest assumption—that patents represent *effort* to invent—also avoids issues that arise in using patents to make stronger claims, such as using them to measure the outcomes of the innovation process. Appendix 3.C explains how the wind power patent data set was constructed.

3.3 Evaluation of Demand-pull Effects

The data assembled for this case generally support the demand-pull hypothesis with respect to diffusion (H1) and learning-by-using (H2). However, they provide little evidence that demand-pull polices stimulated inventive activity (H3).

3.3.1 Demand-pull policies for wind power in California

Throughout the study period, the U.S. federal and California state governments implemented demand-side policy instruments that at times created incentives for private firms to invest in wind power in California. These measures included investment tax credits, production tax credits, guaranteed tariffs, and renewables obligations. Fig. 3.1 shows the sequence and duration of these policies and Appendix 3.B provides descriptions of each. In addition to these incentives, a federal law passed in 1978, the Public Utility Regulatory

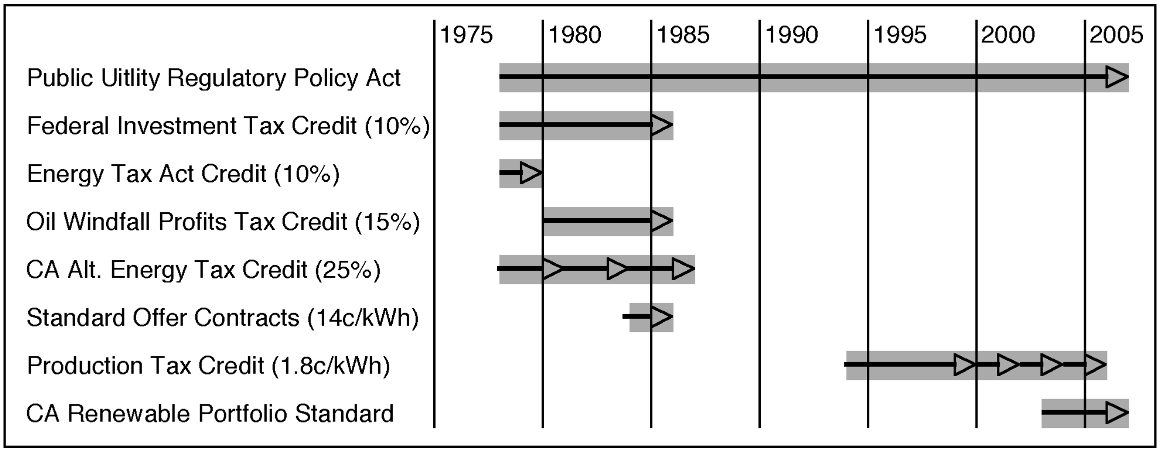

Figure 3.1: Demand-side incentives for wind power in California (arrowheads indicate dates policies expired).

Policy Act (PURPA), mandated that electric utilities offer power purchase agreements to small power generators at rates that reflect the cost to the utility of obtaining additional power. In effect, PURPA gave independent wind power producers access to a rudimentary wholesale power market.

The combination of these policy instruments has in certain periods substantially increased the profitability of wind power for private sector developers. Fig. 3.2 displays the results of calculating the effects of these policy instruments on the levelized annual costs for wind power projects, and the revenues these projects received (see Appendix 3.A for methodology). The investment tax credits reduced the capital cost that would be amortized over the life of the project. The standard offer contracts increased the revenues that wind farm developers could expect to receive to levels that were well above the short run average cost of electricity in California (CEC, 1997). Note that the solid lines apply to the *average* wind project in California in each year. Data on capacity factors for the *best* wind farm operators (WPRS, 1986, 1987, 1988, 1989, 1990, 1991, 1992, 1993, 1995a,b, 1997, 2001, 2002) is used to calculate their costs and shows that at times, from 1985–1992 in particular, their revenues, which were guaranteed for ten years, were greater than their costs.

3.3.2 H1: Demand-pull and diffusion

The first hypothesis within the demand-pull argument that is examined here is that *government intervention on the demand-side stimulated diffusion of the technology*. Nearly two gigawatts of wind power capacity were installed in the state from 1980 to 1995, an investment of over $5 billion in 2005 dollars. Straightforward calculations of the financial incentives created by policy appear to be sufficient to explain the timing of investment. In addition, the access to the wholesale market provided by PURPA overcame a formidable institutional barrier.[3] Construction of new capacity occurred in two periods: 1981–94 (1,978 MW) and 1998–2005 (802 MW) (bottom Fig. 3.3). As Fig. 3.2 and 3.3 show, unsubsidized wind energy did not become profitable until around 2000. Yet the overwhelming majority of investment occurred in the mid-1980s.[4] The first round of investment was almost

---

[3] PURPA gave independent power generators access to the wholesale market by forcing utilities to purchase power from independent generators at regulator-specified rates, which were supposed to reflect the utilities' "avoided costs". While the rates under PURPA contracts have varied substantially over time—by a factor of four—the importance of the access to the wholesale market is made clear by the role that independent generators play in the California wind industry; non-utility generators account for 99% of wind electricity production in California today and have never accounted for less than 96% (CEC, 2006).

[4] The construction in the early 1990s is overwhelmingly comprised of projects for which contracts were secured in 1984–85, but did not come on line until later; over 90% of the new capacity installed from the end of 1983 until 1992 was under ISO4 contracts (Gipe, 1995).

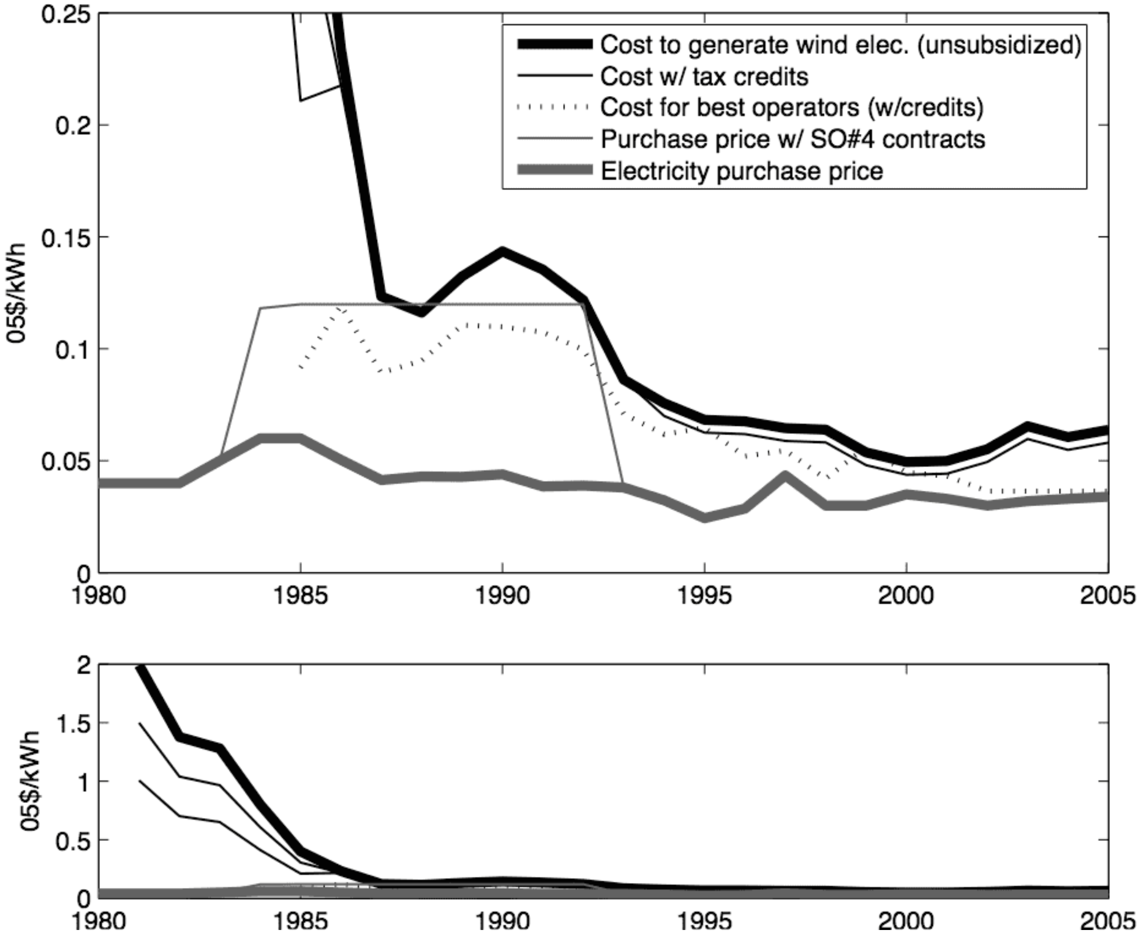

Figure 3.2: Effects of policies on the cost of new wind power projects and the price of purchased electricity in California. The lower figure uses an order of magnitude larger scale on the y-axis to display the high costs in the early-1980s. Data: Gipe (1995); CEC (1997); Sawin (2001); IEA (2002); AWEA (2004); WPRS (2006).

certainly due to the combination of tax credits and guaranteed power purchase agreements at generous rates (CEC, 1997).[5] This combination brought the effective average cost of wind power from ten times the wholesale price without policies to near parity. As shown in Fig. 3.3, the best operators had post-rebate costs below their revenues, which were guaranteed by Interim Standard Offer #4 (ISO4) contracts. These contracts turned out to be well above the short-run average cost of electricity in California (CEC, 1997). This makes it difficult to reject the claim that diffusion of the technology in the 1980s would not have occurred in the absence of the demand-side policy instruments shown in Fig. 3.1.

3.3.3 H2: Demand-pull and learning-by-using

A second argument associated with the demand-pull hypothesis is that *when policy enhances the size of the market for new technologies, it provides opportunities for learning-by-using*. Capacity factor embodies the outcomes of the application of incremental technology improvements derived from experience. In California, the statewide capacity factor for wind power producers rose by a factor of nine from the early-1980s until the late-1990s. While the technical changes involved were incremental—including better site selection, increased up-time, and better maintenance—the effects were large. The increase in electrical output due to capacity factor is equivalent to the production from adding nine times as many additional turbines. Based on the formula in Appendix 3.A, these improvements in capac-

---

[5] The second period of investment occurs when California played a much smaller role in the global wind power market so attributing outcomes to policies in California is less clear.

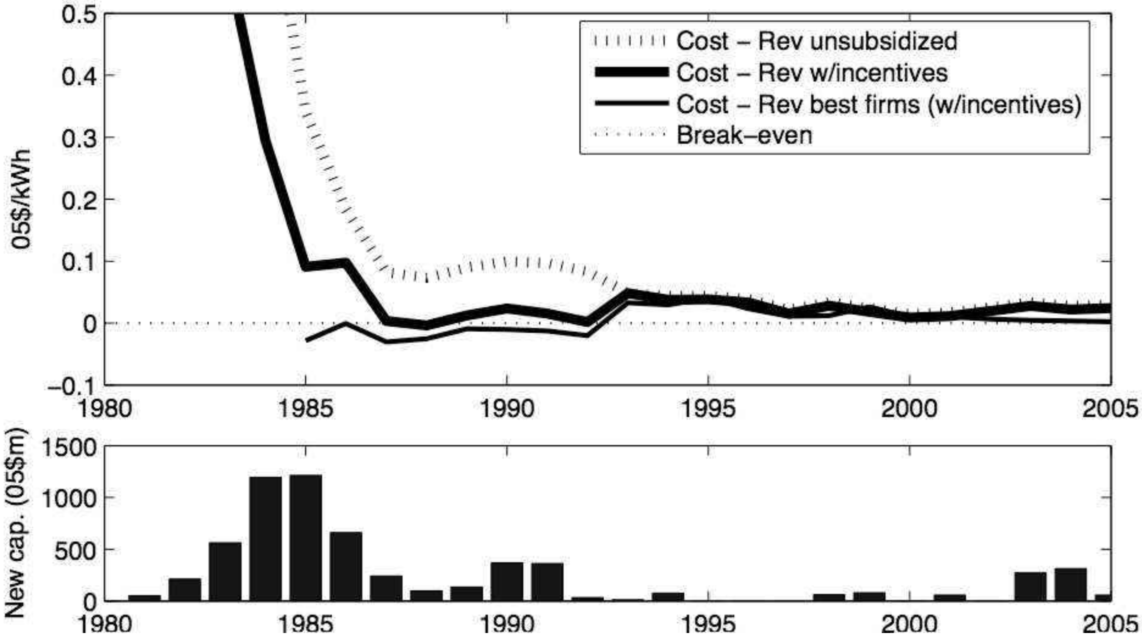

Figure 3.3: Investment in new capacity and the difference between levelized cost and revenue. Data: Gipe (1995); CEC (1997); IEA (2002); AWEA (2004); WPRS (2006).

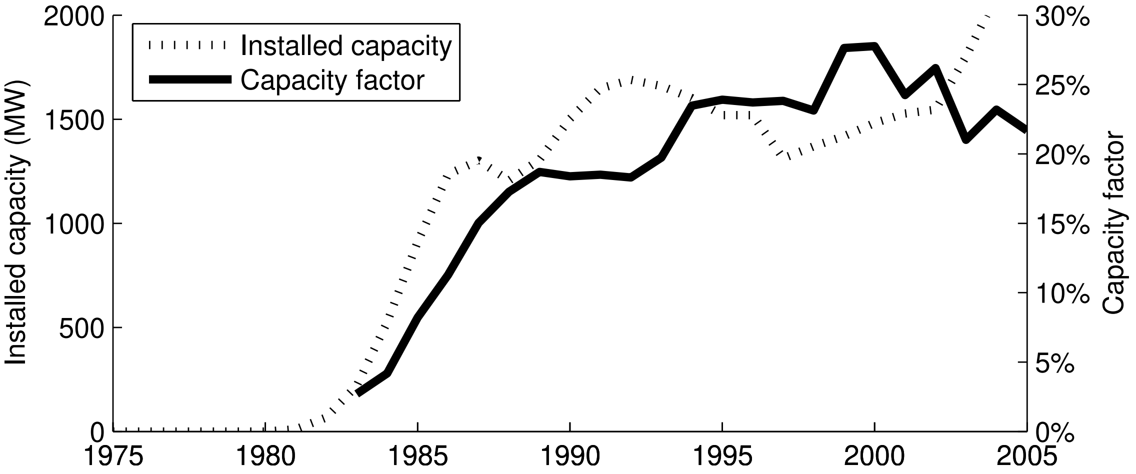

Figure 3.4: Installed capacity and capacity factor of wind turbines in California. Data: AWEA (2005, 2006); CEC (2006); WPRS (2006).

ity factor account for 57% of the factor of 10 reduction in the cost of electricity produced from wind power in California between 1983 and 1994. Fig. 3.4 shows that the timing of capacity factor increases is highly correlated with increases in installed capacity, supporting the notion that improvements in maximizing the electrical output from a given capacity were associated with experience in installing, operating, and maintaining the turbines.

Fig. 3.5 shows two additional indicators of learning-by-using. The left-hand figure shows that *availability*—the portion of time that the turbine is on-line and capable of producing power—improved by 50% so that by the late-1980s, turbines were on-line 97% of the time. Similarly, operation and maintenance (O&M) costs (right-hand side of Fig. 3.5) were cut nearly in half during the 1980s. That O&M costs were reduced while performance, in terms of availability and capacity factor, increased, indicates that new knowledge about

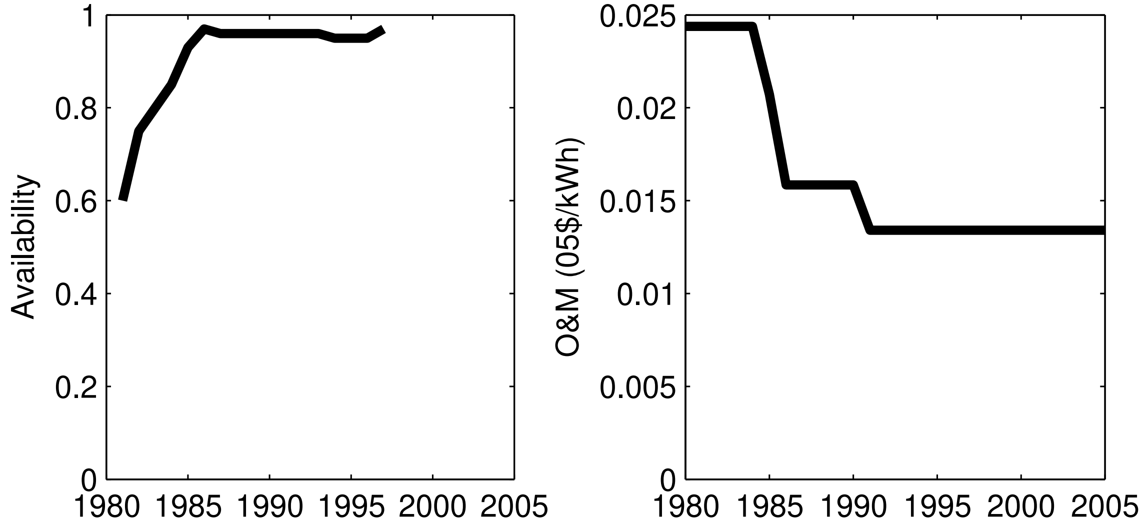

Figure 3.5: Availability (uptime as a % of total) and Operation and Maintenance costs for wind farm operators in California. Data: Gipe (1995).

how to operate wind turbines was acquired, possibly through experience in operating them.

In addition to experience in production, two other factors may have contributed to the increase in capacity factor. First, turbine technology was improving over time and may have resulted in designs that required less maintenance. Second, the use of *average* capacity factor data for the entire state raises the possibility of a survival bias; firms with the worst capacity factors may have exited, pushing up the statewide average once their machines were taken out of operation.[6] Looking at data for individual firms addresses both. As shown in, Fig. 3.6, three large firms increased their capacity factors substantially as they gained experience with the technology, although the progression was not monotonically

---

[6] This is likely given evidence both of decommissioning of turbines, and of early installations which may have been opportunistic projects to take advantage of the investment tax credits when performance incentives were not yet in place.

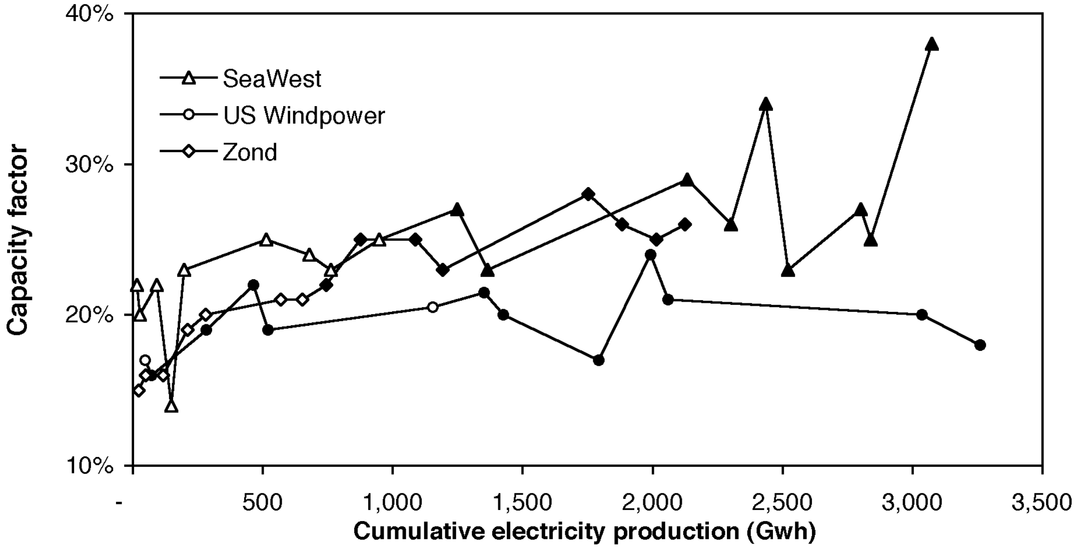

Figure 3.6: Cumulative electricity produced and capacity factor for three large wind farm operators in California. White markers indicate years in which capacity was added and black markers, years in which no new capacity was added. Data: WPRS (2006).

increasing. Second, using data on when the firms installed new units (white markers in figure), one can see that capacity factor increased even in years when no new technology was added (black markers).[7]

Accepting (1) the findings of Section 3.3.2, that policies were instrumental in enabling the diffusion of wind technology in California, and (2) the evidence here that increases in capacity factor are, at least in part, attributable to experience with wind power production, makes the case supportive of the hypothesis that demand-pull policies in the 1980s provided opportunities for learning-by-using.

3.3.4 H3: Demand-pull and inventive activity

According to the demand-pull argument, the emergence of new market opportunities sends signals to firms and creates incentives for them to invest in innovation. The assertion examined here is that policy-led demand created expectations of larger future markets, raising incentives for firms to devote resources to making improvements to the technology. Demand here is operationalized with a time series of *investment* in new wind turbine installations. Hypothesis 3 is that *patenting activity should be correlated with investment*, perhaps with a lag. According to the demand pull hypothesis, this new demand should stimulate efforts to design and develop improved towers, blades, gearboxes, and other components for new installations. Successful efforts to develop novel, useful, and non-trivial improvements to

---

[7] Note that U.S. Windpower filed for bankruptcy in the mid-1990s, near the end of the time series shown.

these parts are patentable so an increase in demand for turbines should increase the attractiveness of a twenty-year monopoly on such inventions and should be associated with an increase in patenting activity. This section looks at three aspects of patenting activity.

Investment and patenting activity

Between the beginning of 1975 and the end of 2001, inventors, companies, and governments filed 830 wind patents that were eventually granted by the U.S. Patent and Trademark Office. U.S. inventors filed 76% of these patents and foreign inventors, 24%. Firms filed 36%, governments, 3% and individuals, 61% (see Appendix 3.C).

There have been two periods of high rates of patenting in wind power technology: 1975–1983 and from 1998 onwards (see upper panel in Fig. 3.7).[8] The first period occurred when there was almost no market for the technology; patenting activity increased dramatically in the mid-1970s when the first new installations were over five years away, an installed base of a gigawatt was over ten years away, when there were no demand-side policies in place, and when the cost of wind power was still 10 to 20 times as expensive as power from gas- and coal-fired power plants.[9] Inventors in the mid-1970s may have been anticipating a market for wind power, but it is dubious that anticipation of the crucial

---

[8] Counts for 2002 and 2003 are included here to show recent trends, although detail on patents is not included due to the truncation and identification issues mentioned below.

[9] The second period of installations corresponds with an increase in patenting. However, the market had become global by the late-1990s, so it is not possible with this data set to attribute patenting in the 2000s to demand in California. The focus in this section is on patenting activity in the 1970s, 80s, and early 90s when California dominated the U.S. and world wind power markets.

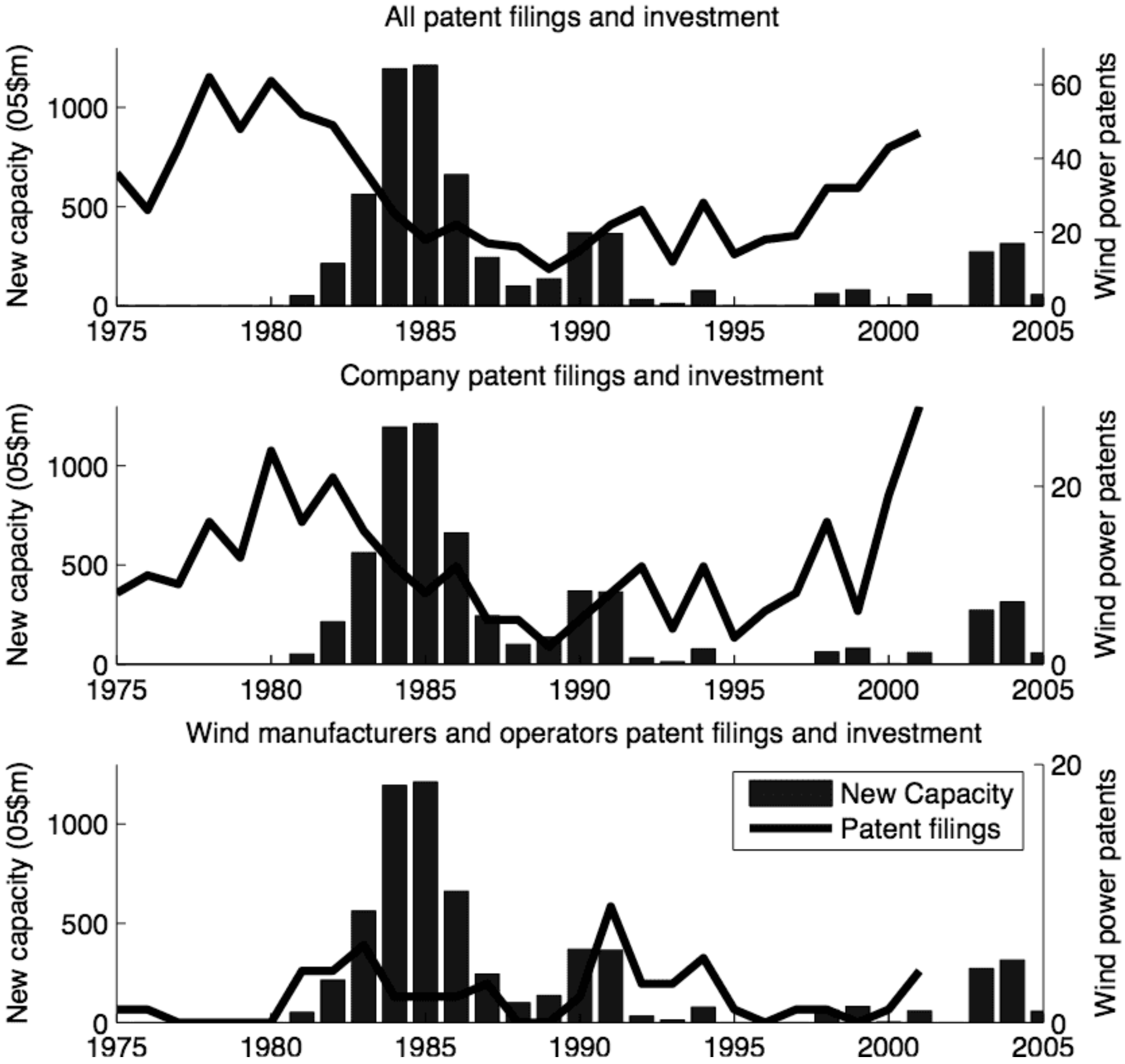

Figure 3.7: Investment in new capacity and patenting activity. Data: CEC (2006); USPTO (2006); WPRS (2006).

details of the policies that were implemented five to ten years later affected these expectations. Further, the most dynamic period of market growth, the mid-1980s, is associated with a decline in patenting activity.

Fig. 3.7 compares the timing of wind power investment with patenting activity by types of patent holders. The middle panel, showing patents assigned to *firms*, displays a similar shape to that for all patents (top panel); patenting declined as investment rose in the mid-1980s. The lower panel, showing patents assigned to firms that either *manufactured* turbines installed in California or *operated* wind farms in the state, has a higher correlation with investment. However, operators of California wind farms and their suppliers account for only 6% of patents, indicating that they are not major producers of patentable inventions.

Investment and highly-cited patents

Previous work indicates that patents that are more frequently cited by other patents tend to be more valuable (Harhoff *et al.*, 1999; Hall *et al.*, 2005). When only *highly-cited* patents—those that were cited five times or more within five years—are included in the time series, the patterns of investment and patenting are even more asynchronous (Fig. 3.8).[10] In this case, 71% of these highly-cited patents were filed before 1981, when the first wind project was installed. Only five were filed during the peak years of the wind boom, 1982–1991. Of

---

[10] Only patents filed by the end of 1997 are displayed to avoid truncation issues associated with retaining five full years of subsequent citation data for each patent.

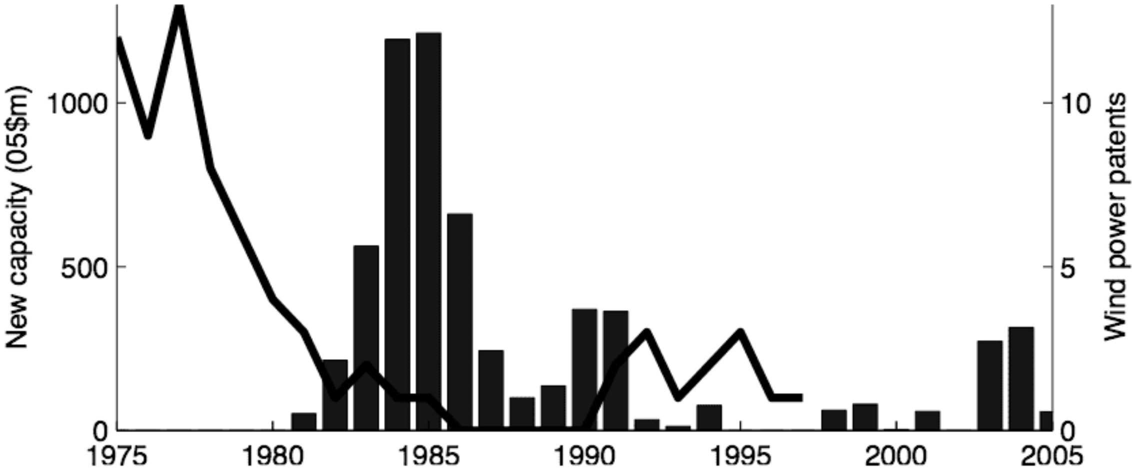

Figure 3.8: Investment in new capacity and highly-cited patents (≥ 5 citations received). Data: Gipe (1995); Hall *et al.* (2001); CEC (2006); USPTO (2006).

the 73 highly-cited patents, 63% were filed by individuals, 31% by companies, and 3% by government agencies.

Highly-cited patents and the emergence of a dominant design

There are a wide variety of practical ways to extract mechanical and electrical energy from the wind; a proliferation of designs have been represented as experimental prototypes, demonstration plants, hobbyists' projects, and even early commercial devices. However, during the 1980s, the variety of designs available as commercial products narrowed considerably. By 1990, 94% of California wind power capacity was built on a horizontal axis, 60% had the turbine blades upwind of the supporting tower—and by the mid-1990s no company was manufacturing a downwind design, 97% used 3 blades, and 90% used blades made of glass-reinforced polyester or fiberglass (Gipe, 1995). The 3-blade, vertical axis, upwind mounted design became the preferred design, and has increasingly dominated the industry (Fig. 3.9). However, prior to the experience gained with widespread diffusion, it was not obvious which combination of features would work best. Other designs have advantages, for example, vertical axis turbines are simpler because they do not require a yaw system to rotate to face the wind, do not need a complex twisted blade-shape, and the drive train is conveniently accessible at ground level. Even more recently some have argued that "There are enough advantages, however, to the vertical axis rotor that it may be worth considering for some future applications" (McGowan and Connors, 2000). Similarly, downwind designs avoid concerns about bending blades striking the tower, two turbine blades require less material than three, and aluminum has a higher strength to weight ratio than fiberglass.

Once the 3-blade, vertical axis, upwind mounted design emerged as dominant in the 1980s, the range of technical improvements occurred within a much narrow range of alternatives. Subsequent areas of innovation and divergence had to do with reducing weight, increasing strength, subtle changes to blade shapes, improving transmissions and drive trains, increasing the efficiency of generators, and improving bearings. A central thrust was capturing the economies of scale that could be obtained by increasing the blade swept area of the turbine.

Given this transition from variety to standardization and an emphasis on incremental change, understanding the dynamics of patenting in the dominant design, and in alterna-

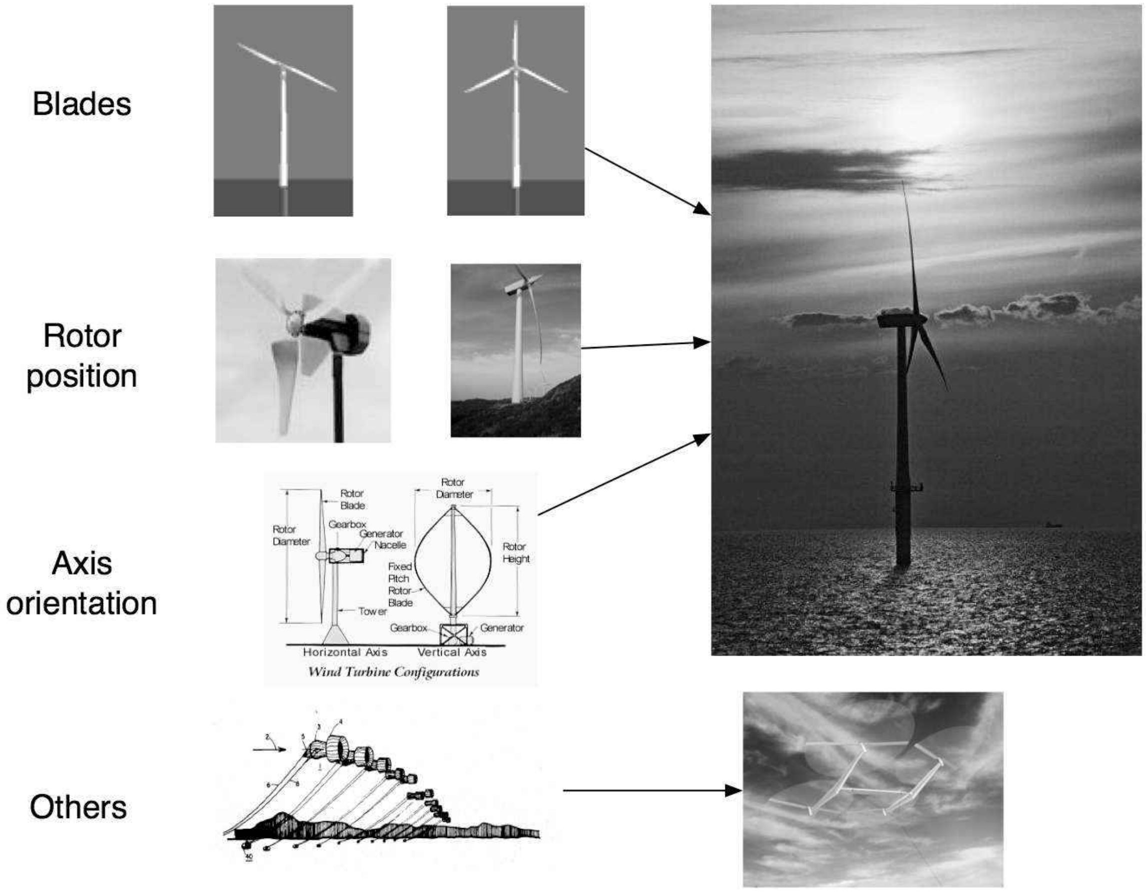

Figure 3.9: Design characteristics and the convergence on the three-blade, upwind, horizontal-axis design.

Table 3.1: Highly-cited (≥ 5) U.S. wind power patents categorized by type of design

	n	%
Dominant design (3-blade, upwind, horizontal axis)		
Power controllers	11	15%
Blade pitch control	7	10%
Blade designs	4	5%
Drive train	0	0%
sub-total	22	30%
Alternative designs		
Vertical axis	15	21%
Integrated end-use	9	12%
Other alt. designs	27	37%
sub-total	51	70%

tives, may give insight as to the relationship between patenting and investment. Here, each of the 73 highly-cited patents was assigned to one of seven categories according to the type of system proposed in the patent.[11] Four of these categories fit under the 3-blade, vertical axis, forward mounted design, which eventually dominated the industry. The other three categories offer alternatives to that design. Only 30% of the patents covered devices that fit under the dominant design (Table 3.1). The remaining 70% were for alternatives, the largest share of which was for vertical axis turbines. Looking at these highly-cited patents over time, the decline in highly-cited patents observed in Fig. 3.8 can be almost completely attributed to the decline in highly cited patents covering devices outside the dominant 3-blade, vertical axis, forward mounted design (Fig. 3.10).

---

[11] The categories correspond to the important innovations identified in Gipe (1995) and Taylor *et al.* (2006).

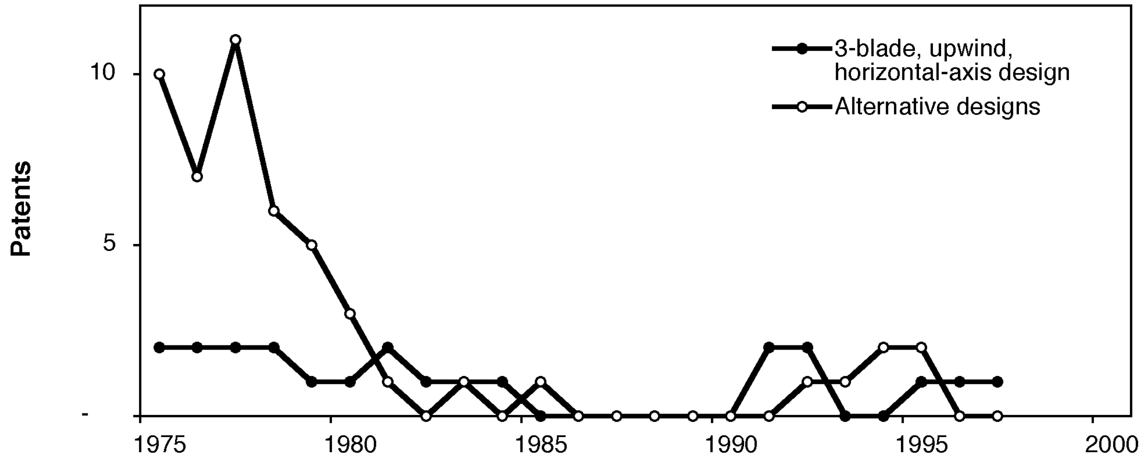

Figure 3.10: Highly-cited patents by type of design. Data: Hall *et al.* (2001); USPTO (2006).

3.4 Why Didn't Inventors Respond?

The case of wind power in California provides only partial support for the demand-pull hypothesis. Diffusion of the technology was clearly driven by demand-side policies. Learning-by-using is at least partially attributable to the experience gained with the turbines installed as a result of these policies. However, it is much harder to find evidence that patenting responded to the demand created by policy. The only case in which patenting activity corresponds with investment in new wind capacity in California is among the small share of patents held by California wind farm operators and the turbine manufacturers that supplied them. Other types of inventors, both firms and individuals, do not appear to have been positively influenced by the demand-side policies put in place in California. And the timing of patent filings that became most frequently cited—ones that we would expect to be more valuable—coincided even less with investment. The results raise the question: *why did diffusion and learning-by-using respond to changes in demand but inventive activity did not?* The study concludes by offering possible explanations.

3.4.1 Policy uncertainty and time lags

The interval between making an investment and realizing its payoff is longer for invention than it is for diffusion and learning-by-using. Constructing a wind farm takes less than two years (Gipe, 1995). The gains from learning-by-doing and -using also arrive quickly (Arrow, 1962a), often within a year or two (Hall and Howell, 1985). Moreover, the investments in these incremental changes are probably quite small. On the other hand, patentable inventions, such as those listed in Table 3.1, may take several years to payoff. New devices must be adapted to real world conditions, integrated into large technological systems, and often require the development of supporting technologies for users to adopt them (Mowery and Rosenberg, 1998).

With uncertain technical and market expectations, lags in investment payoffs mean that investors incur more risk (Dixit and Pindyck, 1994). And uncertainty about expected future demand can be more important than technological risk (Mowery, 1995). In this case, demand for wind power depended on the incentives that policies created. The frequent changes to these policies made demand volatile; investment tax credits varied from 10% to 50% and back to zero in the course of five years. Three times, production tax credits were extended for two years before expiring and subsequently being reinstated. Generous procurement contracts were offered for thirty months and then were replaced with ones at rates a factor of four lower. As a result of the longer time lags that inventors faced, their investments in innovation were more vulnerable to the vagaries of policy than were those of wind farm developers and operators. New installations received credits that could be claimed as soon as the plants were constructed. Production subsidies and purchase contracts were guaranteed for ten years, and were honored even after these programs were cancelled. Because demand was so heavily influenced by policy, expectations about demand five or ten years in the future were more uncertain than those about nearer-term demand. For investments that took longer to pay off, uncertainty in expectations may have dampened the incentives that demand-side policy created. These results support the arguments of van Soest and Bulte (2001); Montgomery and Smith (2005); and Requate (2005b) about the pernicious impact of policy uncertainty on its effectiveness in stimulating investment in innovation.

Future work might examine whether what preliminarily appears to be a *positive* correlation between patenting and demand since 1998 is robust to truncation and geographic identification concerns. An important difference in the recent period is that in the absence of government intervention the technology was much closer to being competitive with conventional power sources. Both David (1971) and Rosenberg (1982a) offered the possibility of a threshold effect, such that the changes in demand may affect investment in a technology when it is close to becoming competitive with another, but not before. For examining trends from the late-1990s onwards, the time series used here run into problems arising from truncation and globalization of the wind industry. Still, indications of a re-emergence of patenting activity are intriguing and potentially important for future work. Testing these ideas would be enabled by the availability of more recent data in coming years and would be contingent on resolving identification issues for a context in which multiple geographical concentrations of demand influence flows of goods, capital, and knowledge.

3.4.2 Emergence of a dominant design

The management literature suggests another difference between invention and the other types of innovation. Firms face tradeoffs between 'exploitation', optimizing their existing operations, and 'exploration', discovering new products or devising new ways to improve existing products (Abernathy, 1978; Levitt and March, 1988; Levinthal and March, 1993; Benner and Tushman, 2003). Once they were installing hundreds of turbines each month, wind farm developers may have shifted their focus toward maximizing the output from their recently installed machines and away from investing in developing alternative, or even 'radical', inventions that may not fit easily within their existing technological and organizational structures. That the gains they achieved from improving capacity factors were so substantial suggests that this shift in focus is would have been likely to pay off. Similarly, suppliers may have focused on improving the 3-blade, vertical axis, forward mounted design, once the market was growing quickly.

3.4.3 Exhaustion of the technical frontier

Invoking the concept of technological opportunity (Nelson, 1988), one could argue that inventive activity in the 1970s exhausted the set of easily discoverable inventions, such that by the 1980s, inventive activity declined because there was little left to discover. This argument, however, is difficult to reconcile with the surge of patenting that began in the late 1990s, the important inventions that were discovered in the early-1990s,[12] and the vast changes in the design and size of new turbines since the 1980s (Dannemand Andersen, 2004). Rather, widespread deployment is more likely to have offered new areas of technological exploration as new problems were encountered, such as dealing with large amounts of reactive power, grid congestion, and complications with megawatt-scale turbines.

3.4.4 Other factors drove inventive activity

Finally, factors other than expected future demand may have influenced patenting rates. Echoing aspects of the technology-push/demand-pull debates, the most prominent competing hypothesis to explain the trend in inventive activity is that public-sector investments in R&D enabled the discovery of new knowledge upon which patentable inventions could build. This arguments fits with work suggesting that technology push may dominate for

---

[12] For example, U.S. Windpower's 1991 "variable speed wind turbine" covered 59 claims.

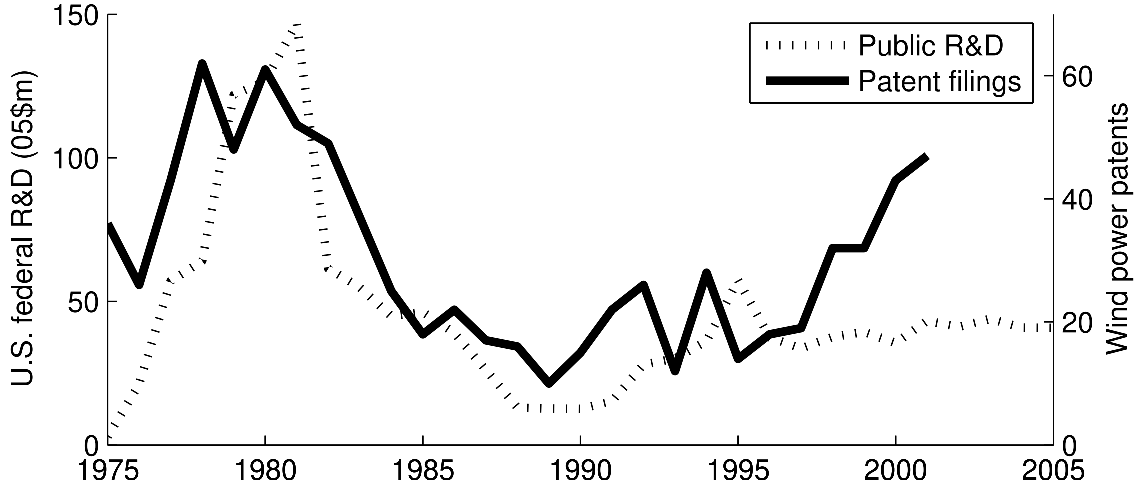

Figure 3.11: Public R&D spending and patenting. Data: (Nemet and Kammen, 2007).

radical innovations, and demand pull for incremental ones (Freeman and Perez, 1988; Dosi, 1988). U.S. federal wind energy R&D spending peaked at $160m in 1979 and the R&D time series is highly correlated with that of patenting (Fig. 3.11).[13]

Another alternative explanation is that a broader societal shift stimulated inventive activity. The oil price shocks of the 1970s did not directly affect the market for wind power, but they may have triggered a shift from perceiving energy as an inexpensive and plentiful resource to one that was limited, and whose future appeared to be influenced by new forces that were difficult to understand and predict (Hirsch, 1999). The shift may have been most succinctly captured in President Nixon's announcement in the 1974 State of the Union address of "Project Independence," his call for the U.S. to become energy self-sufficient by

---

[13] One could argue that demand pull did stimulate inventive activity and that it was only the decline in R&D spending that caused the decline. But patenting rates in the mid-1980s were so low that that line of reasoning would acknowledge that the effect of R&D dominates that of demand.

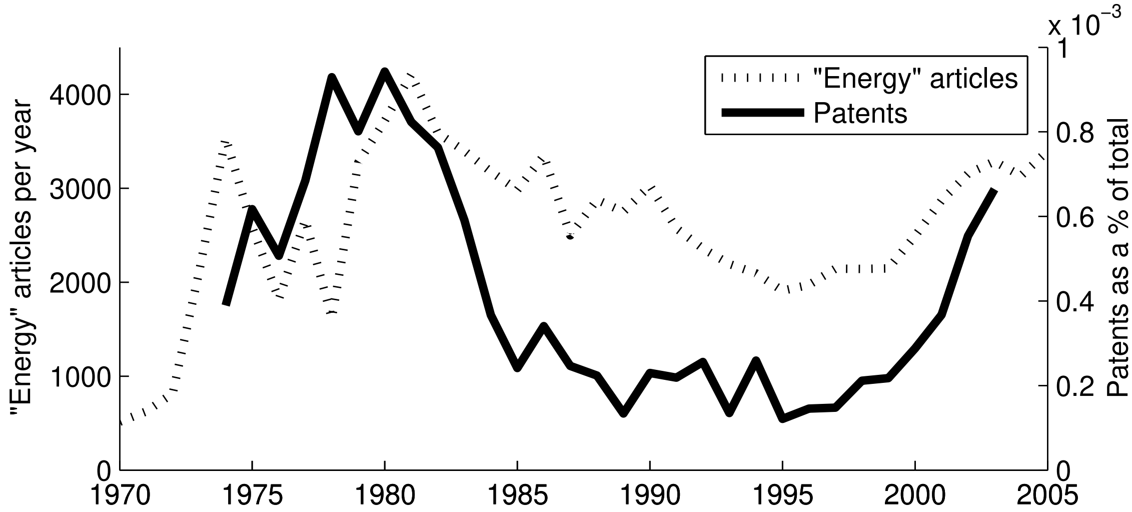

Figure 3.12: Public discourse indicator and patenting. Articles in the New York Times that mention “energy” in their tile and abstract compared to wind power patents as a percentage of total patents. Data: (NYT, 2006; USPTO, 2006).

1980 (Nixon, 1974). This shift may have stimulated scientists, engineers, inventors, and entrepreneurs to turn their attention to “alternative” forms of energy, such as wind power. Literature-based indicators have been used to capture trends in discourses about innovation (Santarelli and Piergiovanni, 1996). Here, a time series is constructed of counts of the word “energy” in a national newspaper. Fig. 3.12 shows that energy issues were becoming more relevant to the public discourse when inventive activity was most intense. This explanation falls outside the realm of the technology-push/demand-pull framework and is more compatible with the approaches taken by studies of wind power in other countries that have focused on the roles of individual agents (Garud and Karnoe, 2003), “social factors” (Heymann, 1998), socio-technical systems (Astrand and Neij, 2006), and interest groups

(Jacobsson and Lauber, 2006).

3.4.5 Future work: demand pull in recent years

Policy uncertainty may also be central to addressing the issue of why demand pull did not stimulate inventive activity in 1975-99, but appears to after 1998 (Fig. 3.7). Policy was a less important driver in the later period because the technology was close to being competitive even in the absence of government intervention (Fig. 3.2). An important difference between periods was that, in the absence of government intervention, the technology was much closer to being competitive with conventional alternatives in the second period than in the first. An aggressive set of demand-side policies, put in place when the technology was far from being competitive on its own, did *not* give rise to a demand-pull effect, while a much weaker set of policies implemented when the unsubsidized cost of the technology was within 30% of the cost of conventional alternatives *was* associated with an inventive response. One interpretation is that investors discounted the effect of policy by incorporating a probability that the policy would change to a less favorable state.Whereas in 1985 subsidies accounted for 62% of the cost of producing wind power, in 2002, they only accounted for 10%. If investors are discounting the long-term value of incentives, then periods, like the mid-1980s, when policy is the dominant driver of demand should have a substantially diminished effect on investment. Earlier work supports the notion that uncertain expectations impose limits on demand-pull. Both David (1971) and Rosenberg (1982a) offered the possibility of a threshold effect, such that the changes in demand may affect investment in a technology when it is close to becoming competitive with another, but not before.

3.4.6 Conclusion

In a sense it is neither a surprise nor a disappointment that patenting declined just as California wind power became a multi-billion dollar industry. The cost of California wind power declined by a factor of ten without any radical changes to the 3-blade, upwind, horizontal-axis design. By the 2000s, the accumulation of incremental changes to blade shapes, transmissions, power controllers, and the gradual scaling up of the sizes of turbines brought wind power close to being competitive with power from natural gas, even without subsidies (Junginger *et al.*, 2005).

But it is far from clear that this path was optimal. It took three decades between the time that studies correctly predicted that turbines would have to exceed 1 MW to be competitive with conventional electricity sources and when that scale was reached. Wind power provides less than 1% of electricity production in the U.S. and as the windiest feasible sites are used up, substantial cost reductions are still needed. It is impossible to assess whether a technology strategy that enabled "coupling" of technical opportunity with market opportunity, a la Freeman (1974), by combining an R&D program with a demand-side program would have accelerated diffusion. Nor is it possible to assess whether increasing the longevity of demand-side measures would have improved outcomes. However, it is clear that the distinct switch from a technology-push policy regime in the 1970s to a demand-pull one in the 1980s provided stronger incentives for investment in incremental improvements to the dominant design than for investment in alternative designs, or even improvements to the dominant design that were novel enough to be patentable. It is also apparent that it is very difficult for policy makers to determine when it is appropriate to support multiple technical paths to encourage technical diversity and when to shift to encouraging refinement of a leading technology through incremental improvement. Policy makers at least need to be aware that their various policy interventions do affect the two types of innovation differently in designing policies to encourage technology improvements.

Appendix 3.A Calculating the Effect of Policies on the Cost of Wind Power

Levelized annual cost of electricity production from wind (LAC) in terms of \$/kWh in each year (t) was calculated using data on the capital cost of installed wind power projects (C), an assumed interest rate (r), the lifetime of turbines (L), capacity factors (F), operations and maintenance costs (OM) and the number of hours in a year (h) using:

$$LAC_t = C_t \frac{\frac{r_t}{(1+r_t)^{-L_t}}}{F_t h} + OM_t \tag{3.2}$$

Using state averages for wind turbines in California, unsubsidized capital costs and the outcomes of eq. 3.2—the cost of electricity—are shown in Fig. 3.13. The effect of policy instruments on levelized annual cost was estimated by making adjustments to the values of the variables in this equation.

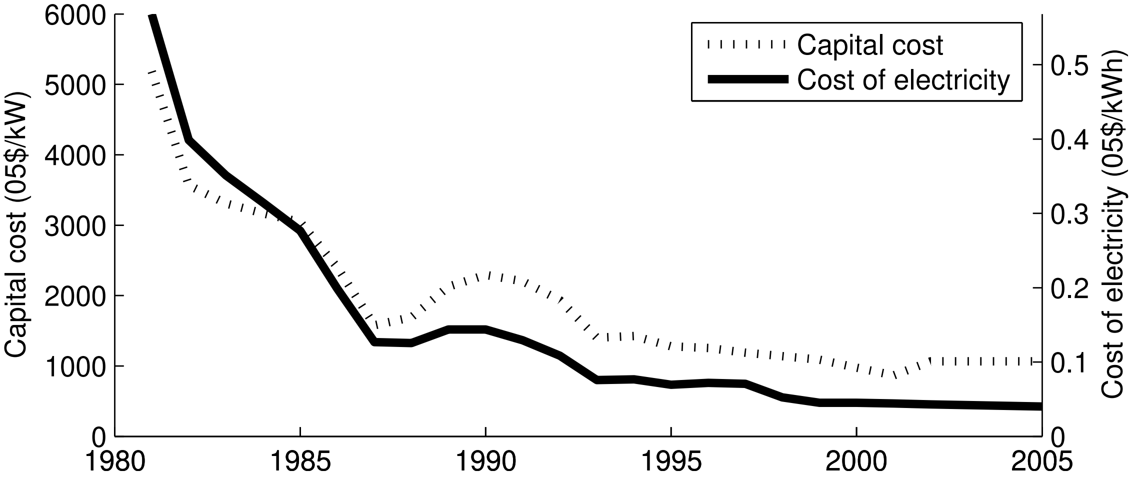

Figure 3.13: Cost of wind turbines and wind power in California. Data: Gipe (1995); AWEA (2004); IEA (2002); WPRS (2006).

Appendix 3.B Description of Policies

This appendix provides detail about the policies discussed in the main text of this chapter.

3.B.1 PURPA: 1978

In 1978, Congress passed the National Energy Act to grow domestic energy production and to encourage conservation. A critical component of the act was the Public Utility Regulatory Policies Act (PURPA). Section 201 of the act removed barriers to independent energy producers by ensuring that utilities would purchase power from them and sell power to them at non-discriminatory rates. These producers were established as "qualifying facilities" (QF's). PURPA mandated that utilities pay for power from QF's at the utilities' "avoided costs," that is, the costs saved by not having to build new power plants. The federal statute left the implementation of PURPA to the discretion of the states.

3.B.2 Investment Tax Credits: 1978–1986

The 1978 federal Energy Tax Act provided investment tax credits of 10% to energy producers. Later, the Crude Oil Windfall Profits Tax Act of 1980 increased this credit to 15% and extended it until 1985. During this period, wind producers were eligible for an additional 10% investment tax credit that the federal government passed to encourage investment at a time of high interest rates. Meanwhile, beginning in 1978, California provided a 25% state tax exemption for "alternative energy sources." At the end of 1985, the federal 10% and 15% credits ended, and on June 30, 1986, the California credits ended.

3.B.3 Interim Standard Offer Contracts: 1983–85

The body responsible for implementing PURPA in California, the California Public Utilities Commission (CPUC), decided in mid-1983 to reward QF's with an avoided cost that reflected the expectations of high future energy prices in the early-1980's. Interim Standard Offer #4 contracts provided ten-year guarantees of payments based on energy produced and capacity installed. These contracts guaranteed an effective tariff of 12.8 cents/kWh (in 2005 dollar terms) (CEC, 1997; Guey-Lee, 1999). The CPUC stopped issuing these contracts in 1985, although capacity under contracts signed before the deadline continued to come on line until the early-1990s.

3.B.4 Production Tax Credit: 1994–2005

The Energy Policy Act of 1992 provided a federal production tax credit (PTC) of 1.5 cents per kilowatt-hour for new wind turbines installed after December 31, 1993. The level has been adjusted for inflation so that it has remained at 1.8 cents/kWh in constant 2005 dollars. Congress has authorized the PTC four times and has allowed it to expire four times. The dates during which wind power producers were eligible to receive the PTC include: 1994–99, 2000–01, 2002–03, and 2004–05.

3.B.5 Renewable Portfolio Standard: 2002

California officially adopted its RPS when Governor Davis signed Senate Bill 1078 on September 12, 2002. Under this bill, California's investor-owned utilities must increase their sales of electricity from renewable sources by at least 1% per year and must reach 20% by 2017 (Sher, 2002; CPUC, 2003).

Appendix 3.C Constructing the Patent Data Set

A central challenge of this methodology was to identify a set of wind power patents from the more than six million patents granted by the U.S. Patent and Trademark Office (PTO). This task involved both ensuring that I identified all relevant patents while at the same time excluding non-relevant patents from my set of wind power patents. Based on the methodology of Taylor (2001), I used two approaches—using U.S. PTO-defined patent classifications, or "classes," and using keywords to search the text of patent abstracts. I began by interviewing a patent examiner at the U.S. PTO.

3.C.1 Examiner interview

The first step in constructing my searches was to interview the patent examiner responsible for wind power (Ponomarenko, 2004). Primary Examiner, Nicholas Ponomarenko has examined 723 wind patents since 1992. As part of his job in reviewing patent applications, he is responsible for searching for "prior art," earlier patents which relate to the current patent application. As a result, he is familiar both with searching using relevant classes and using keywords. The patent examiner said that "most" wind energy patents are in classes 290/44 and 290/55. He indicated that classes 290/43 and 290/54 might also include some wind energy patents. He then suggested several keywords and described his methodology for finding patents outside the usual classes. This description corresponded well to my abstract based search, and I used several of the keywords he mentioned in refining my search.

3.C.2 Class-based search

The class-based search uses classifications defined by the U.S. Patent and Trademark Office for categorizing patents (USPTO, 2005). Patents are assigned to on "primary class" and can be also assigned to "cross classes." The main advantage of the class based patent search is that electronic records exist back to 1790. I used the two classes the patent examiner recommended, 290/44 and 290/55, as the basis of my search. I then read a sample of the patents in the other two recommended classes, 290/43 and 290/54, to see how many relevant patents were in those classes.

I read 50 of the 232 patents (22%) in class 290/43 and found only one patent that was a wind energy patent and that was not also cross-listed under classes 290/44 and /55. I read 50 of the 374 patents (13%) in class 290/54 and found only one additional patent that was a wind energy patent and was not also cross-listed under classes 290/44 and /55. With less than 2% of my sample in each class relevant to wind power, I decided to include only patents listed in classes 290/44 and 290/55 in my class-based patent search (Table 3.2).

3.C.3 Abstract-based search

An alternative method for identifying wind patents involves using keywords to search the abstracts of patents in the U.S. Patent and Trademark Office Bibliographic Database (USPTO, 2006). This method has the advantage of allowing a more precise definition of which patents should be included. The main disadvantage is that abstracts are only search-

Table 3.2: U.S. PTO patent classifications relevant to wind power

Class/sub-class	Definition	
290/44	Electric-control prime-mover dynamo plants including a wind-motor.	Included
290/55	Prime-mover dynamo plants including wind-motors.	Included
290/43	Electric-control prime-mover dynamo plants including a fluid-current motor.	Excluded
290/54	Prime-mover dynamo plants including a fluid current motor.	Excluded

able for patents granted after 1975.

The main task in the abstract based search is to devise a boolean search string that maximizes the number of relevant patents and minimizes the number of non-relevant patents it includes. Building the search string is an iterative process that balances errors of inclusion with errors of omission. I used three reference points in constructing my search string. First, I used the set of keywords for wind power defined by Margolis and Kammen (1999a) for a similar purpose. Second, I used the keywords recommended by the patent examiner. Third, I used the set of patents in classes 290/44 and 290/55. I iteratively adjusted the search string so that it included nearly all patents in class 290/44 and 290/55, while minimizing irrelevant patents. Through iteration, I found that the best string for searching the U.S. PTO Bibliographic Database was that shown in Fig. 3.14.

Finally, I manually read the set of patents the abstract-based search found and discarded those not relevant to wind power. I discarded 14.6% of these patents as not relevant to

```
ABST/((“wind power” OR (wind AND turbine) OR windmill) OR
(wind AND (rotor OR blade$ OR generat$) AND (electric$)))
```

Figure 3.14: Abstract-based patent search

wind power and arrived at a final set of 830 wind patents. Because it is the most accurate representation of wind energy patents, I used this set of patents for all subsequent analyses.

3.C.4 Highly-cited patents

Previous work indicates that patents that are more frequently cited by other patents tend to be more valuable (Harhoff *et al.*, 1999; Hall *et al.*, 2005). This section explains the construction of a set of highly-cited patents that is intended to represent a more meaningful measure of patenting activity. First, the set of patents described above was matched to the NBER patent citation data file to develop a set of citing- and cited-patent pairs (Hall *et al.*, 2001). Self-citations were eliminated using assignee codes; self-citations represented 7% of the citation pairs involving companies. The set of "citing" patents was restricted to patents applied for within five years of the application date for the cited patent. The distribution of of citing-to-cited patent time lags shows that patents cited within 5 years or less of the citing patent account for 40% of the citation pairs (Fig. 3.15). Because the NBER file contains records through the end of 2002, patents are included through 1997 to allow five years of citation time. Based on counts of patents with the various combinations of citation lag and citation frequencies displayed in Table 3.3, "highly-cited" patents were defined as those that received ≥ 5 citations within five years. Finally, highly-cited patents were assigned to one of seven technology design categories.

Table 3.3: Patent citation pairs and patent counts by time lag and citation frequency

		Counts of patents		
Time lag $\leq$	Citation pairs	≥ 1 cite	≥ 5 cites	≥ 10 cites
4 years	1113	459	50	6
5 years	1368	500	73	12
6 years	1589	531	92	17
7 years	1779	554	111	25
All patents	3486	830	830	830

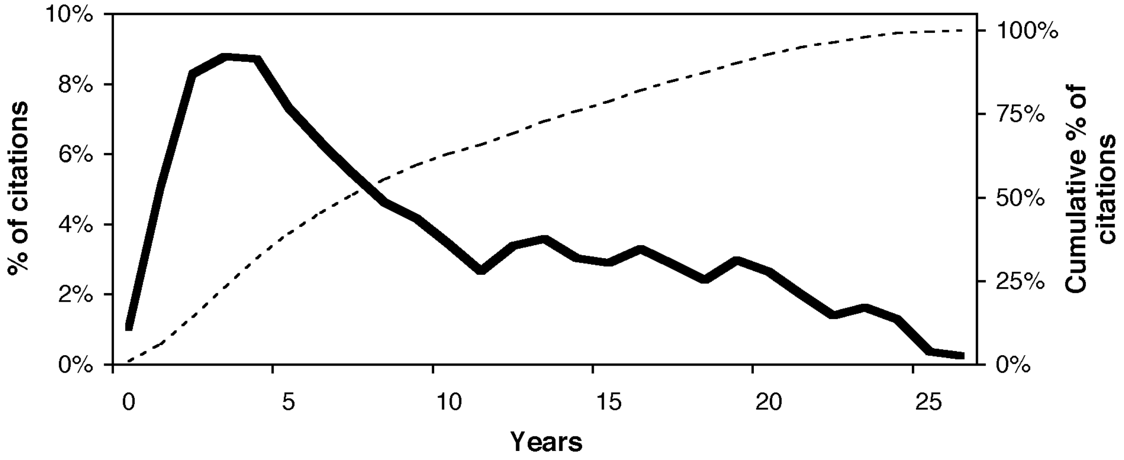

Figure 3.15: Distribution of wind power citation lags. Data: Hall *et al.* (2001); USPTO (2006).

3.C.5 Patent time series of inventor types

The patent data were sorted according to inventor type and origin. Fig. 3.16 shows the time series of patenting by inventor type (upper) and by country of inventor (lower). Assignee types for highly-cited patents are shown in Fig. 3.17.

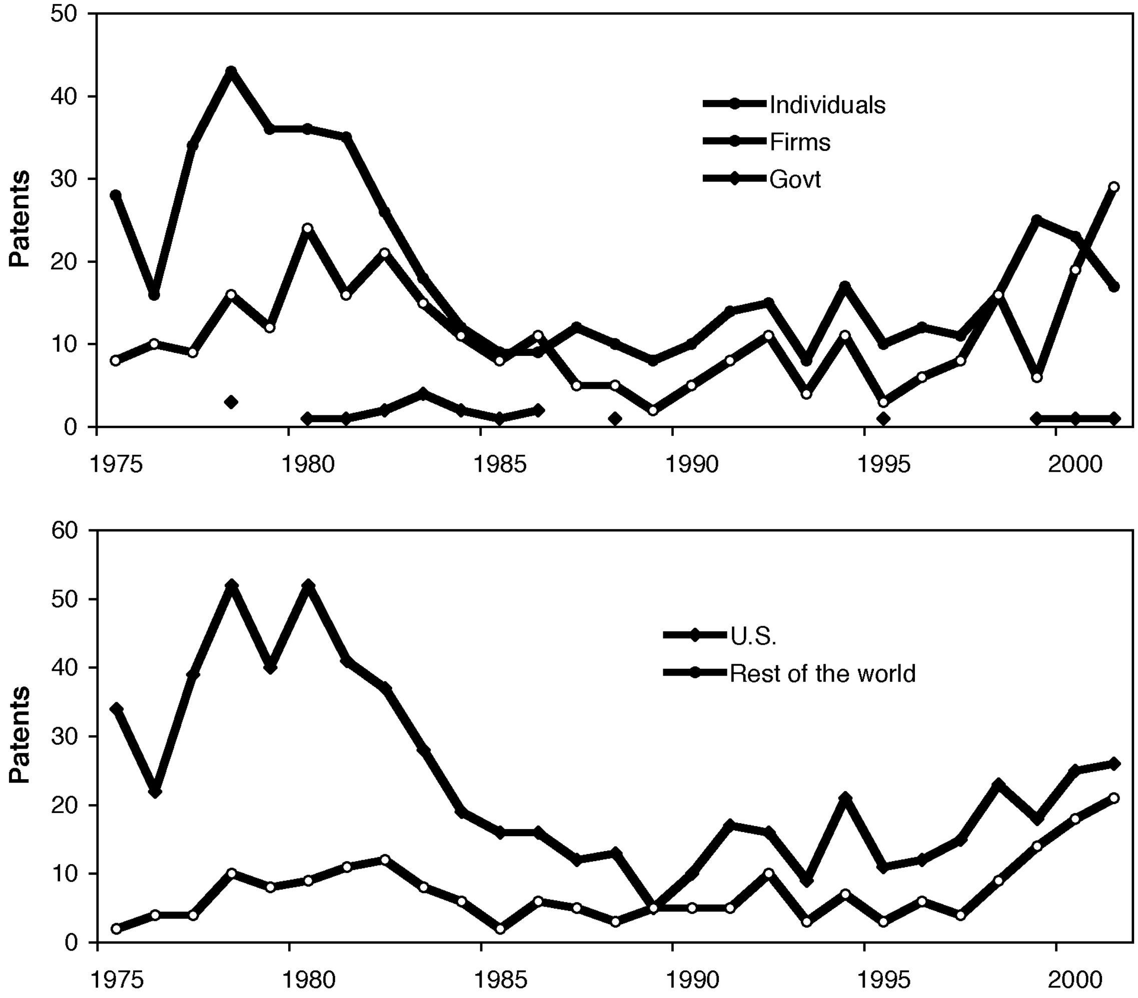

Figure 3.16: Patents by inventor type (upper) and origin (lower). Data: USPTO (2006).

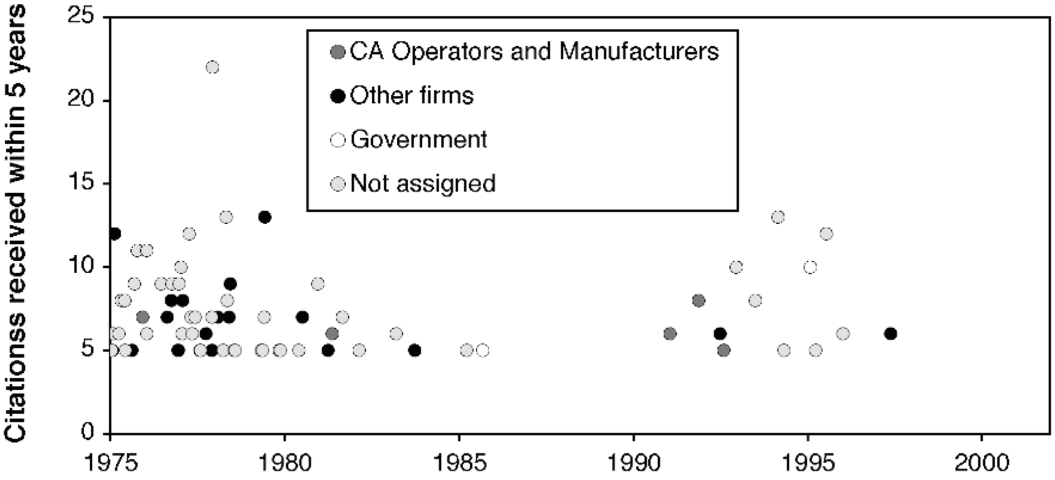

Figure 3.17: Highly-cited patents by assignee type. Data: Hall *et al.* (2001); USPTO (2006).

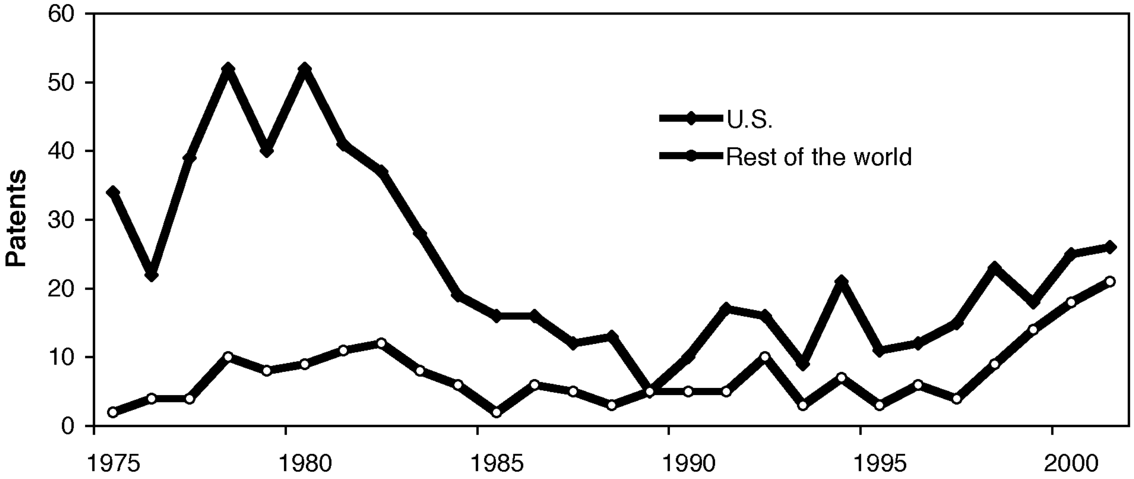

Figure 3.18: Patents by inventor origin. Data: USPTO (2006).

Chapter 4

Inside the Innovation Process: Sources of Cost Reductions in Photovoltaics

In evaluating technology-push and demand-pull, Chapters 2 and 3 addressed the normative question: *how can policy drive innovation?* The focus of this chapter, and of Chapter 5, now shifts to the descriptive question: *what are the characteristics of the innovation process itself?*[1] Under the definition of innovation in this document (see Footnote 1.3), the outcomes of the innovation process are ultimately manifest as cost reductions or performance

---

[1] Portions of this chapter were drawn from an article published as:
Nemet, G.F. (2006) "Beyond the learning curve: factors influencing cost reductions in photovoltaics" *Energy Policy* 34(17): 3218-3232.

improvements. The purpose in this chapter is to decompose the historical improvements in one technology into observable technical factors. Here, policy is temporarily put aside and the empirical assessment is of the technology itself. The motivating idea behind this approach is that if we can identify the driving forces behind the technical change, which we can easily see (e.g. in Fig. 6.1 and 6.2), we can reach a better understanding of *how* technologies improve. Being able to speak more precisely about what activities we are actually concerned with when we use the term "learning" may provide insights for energy technology policy.

4.1 Technological Change and Learning Curves

The rate and direction of future technological change in energy technologies are important sources of uncertainty in models that assess the costs of stabilizing the climate (Edenhofer *et al.*, 2006). Treatment of technology dynamics in integrated assessment models has become increasingly sophisticated (Grubb *et al.*, 2002) as models have incorporated lessons from the economics of innovation and as increased processing power and improved algorithms have enabled optimization of phenomena, such as increasing returns, which in the past had made computation unwieldy (Messner, 1997). Yet the representation of technological change in large energy-economic model remains highly stylized relative to the state-of-the-art of understanding about the economics of innovation (Nordhaus, 2002). Perhaps one reason for the lag between the research frontier for the economics of innovation and that for the modeling of it has to do with incompatibilities in the methodological approaches of the two fields. On the one hand, research on the economics of innovation has tended to emphasize uncertainty (Freeman and Louca, 2001), cumulativeness (Rosenberg, 1994), and non-ergodicity (Arthur, 2006). The outcomes of this line of inquiry, which dates back to Schumpeter (1934), and even Marx (1867)[2], have often been characterized by richness of description, a case study approach, and arguably, more progress with rigorous empirical observation than with strong theoretical claims. On the other hand, optimization and simulation models require compact quantitative estimation of parameters, with uncertainties that do not become infinite once propagated through the model. One of the few concepts that has bridged the epistemological gap between the economics of innovation and the integrated assessment of climate change is the learning curve (Grübler *et al.*, 1999b).

One of the most important technologies related to the long term assessment of climate policy is photovolatics (PV). The cost of PV has declined by a factor of nearly 100 since the 1950s, more than any other energy technology in that period (Wolf, 1974; McDonald and Schrattenholzer, 2001; Maycock, 2002). Markets for PV are expanding rapidly, recently growing at over 40% per year (Maycock, 2005). Future scenarios that include stabilization of greenhouse-gas (GHG) concentrations assume widespread diffusion of PV. In a review of 34 emissions scenarios, Nakicenovic and Riahi (2002) found a median of 22 terawatts

---

[2] "A critical history of technology would show how little any of the inventions of the 18th century are the work of a single individual. . . Technology discloses man's mode of dealing with Nature, the process of production by which he sustains his life, and thereby also lays bare the mode of formation of his social relations, and of the mental conceptions that flow from them" (Marx (1867), Ch. 15, footnote #4).

(TW) of PV deployed in 2100 for those scenarios that include GHG stabilization.[3] At present however, PV remains a niche electricity source and in the overwhelming majority of situations does not compete economically with conventional sources, such as coal and gas, or even with other renewable sources, such as wind and biomass. The extent to which the technology improves over the next few decades will determine whether PV reaches terawatt scale and makes a meaningful contribution to reducing GHG emissions or remains limited to niche applications.

The learning curve is an important tool for modeling technical change and informing policy decisions related to PV and other energy technologies. For example, it provides a method for evaluating the cost effectiveness of public policies to support new technologies (Duke and Kammen, 1999) and for weighing public technology investment against environmental damage costs (van der Zwaan and Rabl, 2004). Energy supply models now also use learning curves to endogenate improvements in technology. Prior to the 1990s, technological change was typically included either as an exogenous increase in energy conversion efficiency or ignored (Azar and Dowlatabadi, 1999). Studies in the 1990s began to use the learning curve to treat technology dynamically (Williams and Tarzian, 1993; Grübler *et al.*, 1999a) and since then it has become a powerful and widely used model for projecting technological change. Recent work however has cautioned that uncertainties in key parameters may be significant (Wene, 2000), making application of the learning curve to evaluate

---

[3] In comparison, total PV deployed at the end of 2006 was just under 6 gigawatts. Assuming constant growth, reaching 22 TW would require increasing capacity by 9% per year over the course of the century.

public policies inappropriate in some cases (Neij *et al.*, 2003). This study examines some of these concerns. After a review of the advantages and limitations of the learning curve model, the applicability of learning curves to PV is then assessed by constructing a bottom-up cost model and comparing its results to the assumptions behind the learning curve.

4.1.1 From learning-by-doing to experience curves

Characterizations of technological change have identified patterns in the ways that technologies are invented, improve, and diffuse into society (Schumpeter, 1947). Studies have described the complex nature of the innovation process in which uncertainty is inherent (Freeman, 1994), knowledge flows across sectors are important (Mowery and Rosenberg, 1998), and lags can be long (Rosenberg, 1994). Perhaps because of characteristics such as these, theoretical work on innovation provides only a limited set of methods with which to *predict* changes in technology. The learning curve model offers an exception.

The learning curve originates from observations that workers in manufacturing plants became more efficient as they produced more units (Wright, 1936; Alchian, 1963; Rapping, 1965). The roots of these micro-level observations can be traced back to early economic theories about the importance of the relationship between specialization and trade, which were based in part on individuals developing expertise over time (Smith, 1776). Drawing on the concept of *learning* in psychological theory, Arrow (1962a) formalized a model explaining technical change as a function of learning derived from the accumulation of experiences in production. In its original conception, the learning curve referred to the changes in the productivity of labor which were enabled by the experience of cumulative production within a manufacturing plant. It has since been refined, for example, Bahk and Gort (1993) make the distinction between "labor learning," "capital learning," and "organizational learning." Others developed the *experience curve* to provide a more general formulation of the concept, including not just labor but all manufacturing costs (Conley, 1970) and aggregating entire industries rather than single plants (Dutton and Thomas, 1984). Though different in scope, each of these concepts is based on Arrow's explanation that "learning by doing" provides opportunities for cost reductions and quality improvements. As a result, these concepts are often, and perhaps misleadingly, grouped under the general category of learning curves. An important implication of the experience curve is that increasing accumulated experience in the early stages of a technology is a dominant strategy both for maximizing the profitability of firms and the societal benefits of technology-related public policy (BCG, 1972).

The learning curve model operationalizes the explanatory variable *experience* using a cumulative measure of production or use. Change in cost typically provides a measure of learning and technological improvement, and represents the dependent variable.[4] Learning curve studies have experimented with a variety of functional forms to describe the relationship between cumulative capacity and cost (Yelle, 1979). The log-linear function is most

---

[4] Cost is often normalized by an indicator of performance, e.g. $/watt. Alternative performance measures are also sometimes used such as accident and defect rates.

common perhaps for its simplicity and generally high goodness-of-fit to observed data. The central parameter in the learning curve model is the exponent defining the slope of a power function, which appears as a linear function when plotted on a log-log scale. This parameter is known as the learning coefficient (b) and can be used to calculate the progress ratio (PR) and learning ratio (LR) as shown below where C is unit cost and q represents cumulative output.

$$\mathrm{C}_t = \mathrm{C}_0 \left(\frac{q_t}{q_0} \right)^{-b} \tag{4.1}$$

$$\mathrm{PR} = 2^{-b} \tag{4.2}$$

$$\mathrm{LR} = (1 - \mathrm{PR}) \tag{4.3}$$

Several studies have criticized the learning curve model, especially in its more general form as the experience curve. Dutton and Thomas (1984) surveyed 108 learning curve studies and showed a wide variation in learning rates leading them to question the explanatory power of experience. In Fig. 4.1, I combine their learning rate data with those in a more recent survey of learning rates by McDonald and Schrattenholzer (2001) and those for the experience curves shown in Fig. 6.1. The learning rate for PV, 0.23, lies near the mode of the distribution. Argote and Epple (1990) explored this variation further and pro-

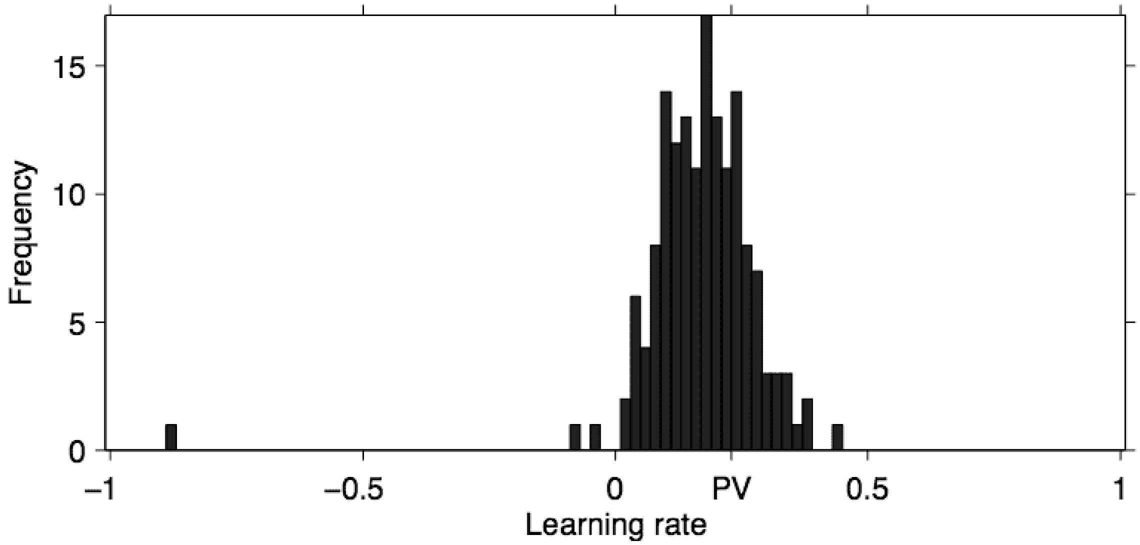

Figure 4.1: Frequency distribution of learning rates calculated in 156 learning curve studies. The learning rate for PV, 0.23, lies slightly above the mode of the distribution. Data: Dutton and Thomas (1984); McDonald and Schrattenholzer (2001) and Fig. 6.1.

posed four alternative hypotheses for the observed technical improvements: economies of scale, knowledge spillovers, and two opposing factors, organizational forgetting and employee turnover. Despite such critiques, the application of the learning curve model has persisted without major modifications as a basis for predicting technical change, informing public policy, and guiding firm strategy. Below, the advantages and limitations of using the more general version of the learning curve, the experience curve, for such applications are outlined.

The experience curve provides an appealing model for several reasons. First, availability of the two empirical time series required to build an experience curve—cost and production data—facilitates testing of the model. As a result, a rather large body of empirical studies has emerged to support the model. Compare the simplicity of obtaining cost and production data with the difficulty of quantifying related concepts such as knowledge stocks (Romer, 1990) and inventive output (Hall and Mairesse, 2006). Still, data quality and uncertainty are infrequently explicitly addressed and as shown below can have a large impact on results. Second, earlier studies of the origin of technical improvements, such as in the aircraft industry (Alchian, 1963) and shipbuilding (Rapping, 1965), provide narratives consistent with the theory that firms learn from past experience. Third, studies cite the generally high goodness-of-fit of power functions to empirical data over several years, or even decades, as validation of the model. Fourth, the dynamic aspect of the model—the rate of improvement adjusts to changes in the growth of production—makes the model superior to forecasts that treat change purely as a function of time.[5] Finally, the reduction of the complex process of innovation to a single parameter, the learning rate, facilitates its inclusion in large optimization and simulation models.

The combination of a rich body of empirical literature and the more recent applications of learning curves in predictive models has revealed weaknesses that echo earlier critiques. First, the *timing* of future cost reductions is highly sensitive not only to changes in the market growth rate but also to small changes in the learning rate. Although, an experience curve R^2 value of >0.95 is considered a strong validation of the experience curve model, variation in the underlying data can lead to uncertainty about the timing of cost reductions

---

[5] An example of the opposite, a non-dynamic forecast, is autonomous energy efficiency improvement (AEEI) in which technologies improve at rates exogenously specified by the modeler (Grubb *et al.*, 2002).

on the scale of decades. Fig. 4.2 shows experience curves based on the two most comprehensive world surveys of PV prices (Maycock, 2002; Strategies-Unlimited, 2003). The Maycock survey produces a learning rate of 0.26 while the Strategies Unlimited data gives 0.17.[6] What may appear as a minor difference has a large effect. For example, assuming a steady industry growth rate of 15% per year, consider how long it will take for PV costs to reach a threshold of \$0.30/W, an estimate for competitiveness with conventional alternatives. Just the difference in the choice of data set used produces a crossover point of 2039 for the 0.26 learning rate and 2067 for the 0.17 rate, a difference of 28 years. McDonald and Schrattenholzer (2001) show that the range of learning rates for energy technologies in general is even larger. Neij *et al.* (2003) find that calculations of the cost effectiveness of public policies are sensitive to such variation. Wene (2000) observes this sensitivity as well and recommends an on-going process of policy evaluation that continuously incorporates recent data.

Second, the experience curve model gives no way to predict discontinuities in the learning rate. In the case of PV, the experience curve switched to a lower trajectory around 1980. As a result, experience curve-based forecasts of PV in the 1970s predicted faster technological progress than actually occurred (Schaeffer *et al.*, 2004). Discontinuities present special difficulties at early stages in the life of a technology. Early on, only a few data points define the experience curve, while at such times decisions about public support may be most crit-

---

[6] Note that the largest differences between the price surveys are in the early stages of commercialization when using experience curves may be least appropriate.

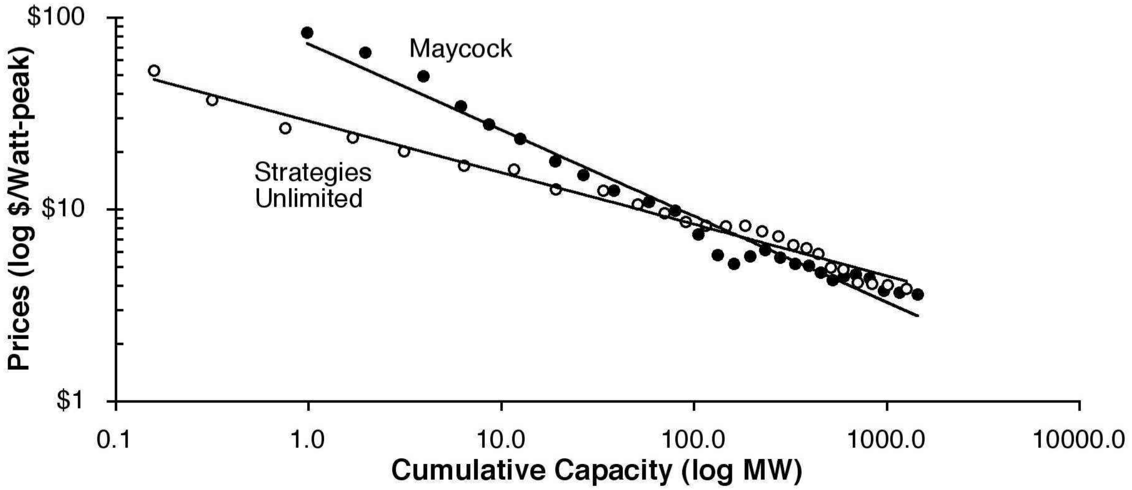

Figure 4.2: Experience curves for PV modules and sensitivity of learning rate to underlying data. Data: Maycock (2002); Strategies-Unlimited (2003).

ical. Early work in economics is skeptical about the assumption that historically observed rates of learning can be expected to continue in the future. (Arrow, 1962a) argued that that learning is subject to "sharply diminishing returns." Looking at studies within single plants, (Hall and Howell, 1985) and (Baloff, 1966) find that learning rates become essentially flat after a relatively short amount of time—approximately 2 years in these studies. Some have suggested, that as a result, a cubic or logistic function offers a more realistic functional form than a power function (Carlson, 1973).

Third, studies that address uncertainty typically calculate uncertainties in the learning rate using the historical level of variance in the relationship between cost and cumulative capacity. This approach ignores uncertainties and limitations in the progress of the specific technical factors that are important in driving cost-reductions (Wene, 2000). For example, constraints on individual factors, such as theoretical efficiency limits, might affect our confidence in the likelihood of future cost reductions.

Fourth, due to their application in planning and forecasting, emphasis has shifted away from learning curves based on employee productivity and plant-level analysis, toward experience curves aggregating industries and including all components of operating cost. While the statistical relationships generally remain strong, the conceptual story begins to look stretched as one must make assumptions about the extent to which experience is shared across firms. In the strictest interpretation of the learning-by-doing model applied to entire industries, one must assume that each firm benefits from the collective experience of all. The model assumes homogenous knowledge spillovers among firms.

Fifth, the assumption that experience, as represented by cumulative capacity, is the *only* determinant of cost reductions ignores the effect of knowledge acquired from other sources, such as from R&D or from other industries. Earlier, Sheshinski (1967) wrestled with the separation of the impact of two competing factors, investment and output. Others have addressed this limitation by incorporating additional factors such as workforce training (Adler and Clark, 1991), R&D (Buonanno *et al.*, 2003; Miketa and Schrattenholzer, 2004), and the interactions between R&D and diffusion (Watanabe *et al.*, 2000). Colinearity among the explanatory variables requires large detailed data sets, the scarcity of which has so far limited widespread application of these more sophisticated models.

Finally, experience curves ignore changes in *quality* beyond the single dimension being analyzed (Thompson, 2001).[7] The dependent variable is limited to cost normalized by a single measure of performance—for example, hours of labor per aircraft, $ per watt, or ¢ per megabyte. Measures of performance like these ignore changes in quality such as aircraft speed, reliability of power generation, and the compactness of computer memory.

4.1.2 Methodology

This study seeks to understand the drivers behind technical change in PV by disaggregating historic cost reductions into observable technical factors. The mechanisms linking factors such as cumulative capacity and R&D to technological outcomes, while certainly important, are at present not well understood. Many of the problems mentioned above arise because the experience curve model relies on assumptions about weakly understood phenomena. Rather than making assumptions about the roles that factors like experience, learning, R&D, and spillovers play in reducing costs, a set of observable technical factors are identified whose impact on cost can be directly calculated.

The time period included here begins with the period of nascent commercialization, 1975 and continues to 2001. During this 26-year period, there was a factor of 20 cost reduction in the cost of PV modules. Only PV modules are examined and balance-of-system components such as inverters, storage, and supporting structures are excluded.[8] The focus

---

[7] Payson (1998) provides an alternative framework that incorporates both changes in quality and cost improvements.

[8] Inverters and other components have similar progress ratios to modules and have exhibited cost decreases by factors of 5 and 10 respectively. Inverter and battery replacement will become an important component of overall system cost in coming years as PV systems age. The labor-intensive tasks involved in replacement

here is on explaining change in the capital cost of PV modules, rather than on the cost of electricity produced, mainly due to data quality considerations and to be able to exclude influential but exogenous factors such as interest rates. The study is limited to PV modules manufactured from mono-crystalline and poly-crystalline silicon wafers because crystalline silicon has been the overwhelmingly dominant technology for PV over this period. Crystalline silicon PV comprised over 90% of production over this period and its share increased in the second half of the period.[9] While photovoltaic electricity has been produced from a wide variety of other materials, such as cadmium-telluride and copper-indium-diselenide, during the study period these competing technologies remained in the development stage were not commercially relevant. The price data used in the study are weighted averages of the two types of silicon crystals. The study uses worldwide data rather than country-level data because over this time period the market for PV became global. Some of the change often attributed to within-country costs is due to the globalization of the industry, rather than learning from that country's experience. Junginger *et al.* (2005) articulated the need for such an international view and as a result developed a global experience curve for wind power. This study adopts a similarly global view. The scope of this study thus addresses the concerns raised by Schaeffer *et al.* (2004) regarding the importance of data quality, system boundaries, and sufficient historical time period for assessing experience in energy technologies. Finally, the technological characteristics of PV provide two simplifying aspects

---

may be particularly suited to improvement through learning-by-doing.

[9] Crystalline silicon makes up close to 100% of the market for applications of >1kW, a definition of the market that includes household-scale and larger power generation and excludes consumer electronics.

that help restrict the influence of potentially confounding factors in the study. First, there has been no significant change in per unit scale in PV panels. PV panels have been sized on the order of one square meter per panel for three decades. Compare this to wind turbines in which the size of individual units has increased by almost two orders of magnitude over the same period (Madsen *et al.*, 2003; Junginger *et al.*, 2005). Second, there are essentially no operation and maintenance costs associated with PV, other than regular cleaning and inverter replacement. This limits the role of "learning by using," which would normally be an important additional factor to consider (Rosenberg, 1982b).

The analysis began by identifying factors that changed over time and had some impact on PV costs. Using empirical data, the annual level of these seven factors over the study period, 1975 to 2001 was compiled and a model to quantify the impact of the change in each factor on module cost developed.

4.2 Description of the Cost Model

This cost model simulates the effect of changes in each of seven factors on manufacturing cost in each year, t, as follows.

4.2.1 Cost

Average module cost (C) in \$/Watt$_{\mathrm{peak}}$ is the dependent variable in the model.[10] The time series for cost uses an average of the two most comprehensive world surveys of PV prices (Maycock, 2002; Strategies-Unlimited, 2003). Using prices as a proxy for costs is a widespread practice whose validity is discussed below. The model uses module cost, rather than cost of energy produced, to avoid the large uncertainties associated with making assumptions about capacity factors, lifetimes, and financing mechanisms.

4.2.2 Module efficiency

Improvements in the energy efficiency ($\eta = \mathrm{W_{out}}/\mathrm{W_{in}}$) of modules sold have nearly doubled the rated power output of each square meter (m^2) of PV material produced (Christensen, 1985; Maycock, 1994; Grubb and Vigotti, 1997; Maycock, 2002). The model simulates the impact of efficiency changes on module cost using:

$$\triangle \mathrm{C}_{t(\eta)} = \mathrm{C}_{t-1}\left(\frac{\eta_{t-1}}{\eta_t} - 1\right) \tag{4.4}$$

This simple formulation applies the annual change in efficiency to the previous year's cost, C_{t-1}, to calculate the change in cost due to efficiency, $\triangle \mathrm{C}_{t(\eta)}$. As an example, a doubling in efficiency would, ceteris paribus, reduce \$/Watt cost by 50%.

---

[10] All monetary values presented in this study are in U.S. dollars at constant 2002 prices.

4.2.3 Plant size

Growth in the expected future demand for PV has led to an increase in the average annual output of PV manufacturing plants of more than two orders of magnitude (Maycock and Stirewalt, 1985; Maycock, 1994; Ghannam *et al.*, 1997; Maycock, 2002; Mitchell *et al.*, 2002). Growing demand has enabled manufacturers to build larger facilities, which exploit economies of scale by absorbing indivisible costs. The effect of increasing plant size (SZ) is estimated using eq. 4.5. A scaling factor for operating costs is borrowed from the semiconductor industry ($b = -0.18$)(Gruber, 1996), the industry whose production processes are most similar to those of PV. This value is within the range of assumptions used in studies that calculate future cost savings for large scale PV.[11]

$$\triangle \mathrm{C}_{t(\mathrm{SZ})} = \mathrm{C}_{t-1}\left(\left(\frac{\mathrm{SZ}_t}{\mathrm{SZ}_{t-1}}\right)^b - 1\right) \tag{4.5}$$

4.2.4 Yield

Improved cell and module processing techniques have increased yield, the proportion of functioning units available at the end of the manufacturing process (YD) (Little and Nowlan, 1997; Sarti and Einhaus, 2002; Rohatgi, 2003).[12] Because post-wafer yield measures the

---

[11] 'Large scale' means >100 MW per plant per year. Other PV scaling factors include the following: $b = -0.07$ (Bruton and Woodock, 1997), $b = -0.09$ (Rohatgi, 2003), $b = -0.12$ (Frantzis *et al.*, 2000), $b = -0.18$ (Maycock, 1997), $b = -0.20$ (Ghannam *et al.*, 1997). It is not surprising that the chosen value lies at the upper end of this range because it is being applied historically, when smaller plant sizes probably were yielding more economies of scale than they would at the levels of 100-500MW/year in these studies.

[12] Yield improvements in the manufacturing of wafers are captured in the section on silicon consumption below.

final stages of the production process, firms incur the entire cost of modules they discard for mechanical or electrical reasons. The trend toward thinner wafers increased the brittleness of cells. This more delicate material increased the possibility of breakage, offsetting some of the gains in yield delivered by automation.

$$\triangle \mathrm{C}_{t(\mathrm{YD})} = \mathrm{C}_{t-1}\left(\frac{\mathrm{YD}_{t-1}}{\mathrm{YD}_{t}} - 1\right) \tag{4.6}$$

4.2.5 Poly-crystalline share

Wafers cut from silicon ingots comprised of multiple crystals (poly-crystalline) rather than individual crystals (mono-crystalline) have accounted for an increasing share of world production (Costello and Rappaport, 1980; Maycock, 1994, 2002, 2003; Menanteau, 2000; JPEA, 2002; Goetzberger *et al.*, 2003). Based on comparisons of mono- and poly-crystalline prices (Maycock, 1994, 1997; Bruton and Woodock, 1997; Sarti and Einhaus, 2002), it is assumed that poly-crystalline modules cost 90% that of mono-crystalline modules (PF=0.9). Eq. 4.7 calculates the cost of poly-crystalline modules (PC) based on average prices of all types of modules. The effect of the growing market share for poly-crystalline modules (PS) on average module cost is obtained in eq. 4.8 :

$$\mathrm{PC}_t = \mathrm{PF}\frac{\mathrm{C}_t}{1-(1-\mathrm{PF})\,\mathrm{PS}_t} \tag{4.7}$$

$$\triangle\mathrm{C}_{t(\mathrm{PS})} = \left(\mathrm{PS}_t - \mathrm{PS}_{t-1}\right)\left(\mathrm{PC}_{t-1} - \mathrm{C}_{t-1}\right) \tag{4.8}$$

4.2.6 Silicon cost

The basic material input for producing PV wafers is solar-grade silicon feedstock, the cost of which (SC) fell by nearly a factor of 12 over the study period (Ghosh, 1979; Costello and Rappaport, 1980; Bruton, 2002; Swanson, 2006)(Fig. 4.3). Changes in the other major materials—glass, ethyl-vinyl acetate (EVA), aluminum and framing materials—are ignored because they are orders of magnitude less costly than silicon.[13] The annual effect of the change due to silicon cost is estimated by calculating the cost of the silicon necessary to produce a watt of PV module, while holding the amount of silicon used (SU) per watt constant:

$$\triangle\mathrm{C}_{t(\mathrm{SC})} = \left(\mathrm{SC}_t\mathrm{SU}_{t-1}\right) - \left(\mathrm{SC}_{t-1}\mathrm{SU}_{t-1}\right) \tag{4.9}$$

---

[13] Note that in Fig. 4.3 a log scale is necessary to show the changes in the other materials.

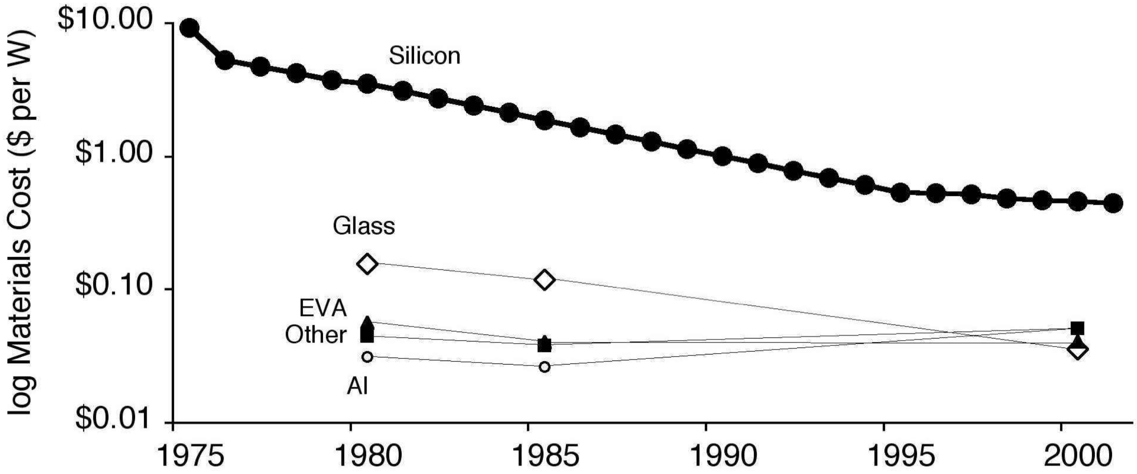

Figure 4.3: Materials costs for PV modules. Data: Christensen (1985); Maycock (2002).

4.2.7 Silicon consumption

The amount of silicon used per watt of PV module has fallen by a factor of 1.5 over the period (Maycock, 2002; Woditsch and Koch, 2002; Swanson, 2006). Manufacturers have accomplished this change by reducing the thickness of silicon wafers from 500μm to 250μm and by reducing kerf losses, from the sawing of each wafer, from 250μm to 190μm. The amount of silicon saved each year is calculated and is combined with data on silicon cost to estimate the effect on module cost.

$$\triangle \mathrm{C}_{t(\mathrm{SU})} = (\mathrm{SC}_{t-1}\mathrm{SU}_{t}) - (\mathrm{SC}_{t-1}\mathrm{SU}_{t-1}) \tag{4.10}$$

4.2.8 Wafer size

Improved crystal growing methods have increased the cross-sectional area of each wafer (WS) by a factor of four (Christensen, 1985; Symko-Davies *et al.*, 2000; Rohatgi, 2003; Swanson, 2006). Larger wafers facilitate savings in the cell and module assembly processes where there are costs that are fixed per wafer, e.g. forming electrical junctions and testing. Using studies that disaggregate costs, the model assumes that post-wafer processing accounts for 40% of the cost of producing a module in all periods (WP=0.4)(Moore, 1982; Bruton and Woodock, 1997; Maycock, 2002) and that fixed per wafer costs are 10% of cell and module assembly costs (WF=0.1).

$$\triangle \mathrm{C}_{t(\mathrm{WS})} = \mathrm{C}_{t-1}\left(\frac{\mathrm{WS}_{t-1}}{\mathrm{WS}_t} - 1\right)\mathrm{WP}\cdot\mathrm{WF} \tag{4.11}$$

4.2.9 Full model

The total change in module cost each year is the sum of the changes in each of the seven factors described above (F).[14]

$$\triangle \mathrm{C}_t = \sum \triangle \mathrm{C}_{\mathrm{F},t} \tag{4.12}$$

---

[14] Other factors such as labor, automation, and other material inputs were also considered. However, they are excluded from the model because these changes are either very small or are captured as changes in other factors that were included in the model. Similarly, the negative interaction terms among the factors are assumed to be negligible and are excluded from the model.

Table 4.1: Summary of model results, 1975-2001

Factor	Change	Effect on module cost ($/W)
Module efficiency	6.3% → 13.5%	-17.97
Plant size	76 kW/yr → 14 MW/yr	-13.54
Si cost	300 $/kg → 25 $/kg	-7.74
Si consumption	30 g/W → 18 g/W	-1.06
Yield	87% → 92%	-0.87
Wafer size	37 cm^2 → 180 cm^2	-1.16
Poly-crystal	0% → 50%	-0.38
Sum of factors		-42.72
Actual change		-70.36
Residual		-27.63

4.3 Plant Size, Efficiency, and Silicon Dominate

Three factors were most important in explaining cost declines from 1975 to 2001: plant size, cell efficiency, and to a lesser extent, the cost of silicon (Table 4.1). The other four factors each account for less than 2% of the cost decline. However, these seven factors together explain just over 60% of the change in cost over the period. Such a large residual requires understanding the reasons for this residual before drawing conclusions about the model results. Analysis of the residual shows that the model predicts the actual change in prices much better after 1980 than it does before 1980.

The following sections present results obtained by partitioning the model into two time periods; Period 1: 1975-79 and Period 2: 1980-2001. These periods were chosen for three reasons. First, by 1980 terrestrial applications had become dominant over space-based applications. The emergence of niche markets for navigation, telecommunications, and remote residences signaled the start of a viable commercial market. Second, global public R&D spending on PV reached its peak, $370m, in 1980 (Kammen and Nemet, 2005). The subsequent decline in R&D reflected a less active government role in technology development as the experiences of the 1970s oil crises faded. Third, in 1980, governments such as Japan began subsidizing commercial applications, indicative of the shift from research-oriented to diffusion-oriented technology policies.

4.3.1 Period 1: 1975-1979

In the first four years of this study, cost declined by a factor of three. Of the factors identified in the model, efficiency, cost of silicon, and plant size accounted for the most change in cost. Three other factors, module size, yield, and silicon consumption, were of less importance but played a role. The share of modules that were poly-crystalline did not change and thus had no effect. These seven factors however fail to explain most of the change in cost over this period, as 54% of the change is unexplained. In the rest of this section, other factors are discussed that may help explain some of this large residual. Understanding the early period of commercialization is important because many technologies tend to attract widespread interest as they emerge from the laboratory and find their first commercial applications. As a result, policy and investment decisions must be made at this early stage when the factors discussed below may be at work.

As a starting point for identifying alternative explanations in this period, it is important to note that there was a dramatic change in the market for PV over these four years. During this period, terrestrial applications overtook space-based satellite applications as the dominant end-use. In 1974, the market share of terrestrial applications was 4%—satellites accounted for the remaining 96% (Moore, 1982). By 1979, the terrestrial market share had grown to 64%. The following sections address the large residual with four possible explanations, each of which is associated with this shift in end use.

Weaker preferences for quality

One reason for the unexplained change in cost is that the shift from space to terrestrial applications led to a reduction in the *quality* of modules. The shift away from space applications rendered certain characteristics non-essential, allowing manufacturers to switch to less costly processes.[15]

First, spatial and weight constraints on rockets required high efficiency panels to maximize watts delivered per m^2. The relaxation of this requirement for terrestrial applications enabled manufacturers to employ two important cost-saving processes (Moore, 1982). Modules could use the entire area of the silicon wafer—even the portions near the edges which tend to suffer from defects and high electrical resistivity. Also, the final assembly process could use a chemical polish to enhance light transmission through the glass cover,

---

[15] As a result, the earliest terrestrial modules in the early-1970s were built from reject space cells (Christensen, 1985).

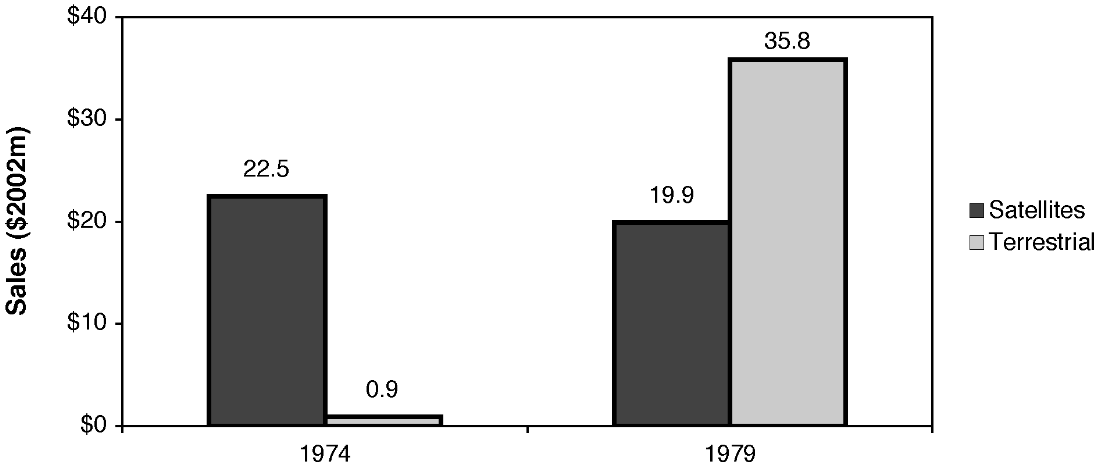

Figure 4.4: PV sales for satellite and terrestrial applications. Data: Costello and Rappaport (1980); Moore (1982).

rather than the more expensive ground optical finish that was required for satellites.

Second, reliability targets fell. Satellite programs, such as Vanguard and Skylab, needed satellite PV modules that would operate reliably without maintenance, perhaps for twenty years. Terrestrial applications, on the other hand, could be still be useful with much shorter lifetimes. During the 1970's the market for PV modules shifted towards terrestrial applications. Whereas in 1974, 94% of PV systems were manufactured for applications in space, by 1979, the market share for space had fallen to 36%. The rapid growth in the terrestrial market was the main driver of this change (Fig. 4.4).

Combining lifetime data (Christensen, 1985; Wohlgemuth, 2003) with the shares of satellite and terrestrial applications shows a decline in average industry module lifetime during the late-1970s (Fig. 4.5). The transition from 20-year reliability targets in the early

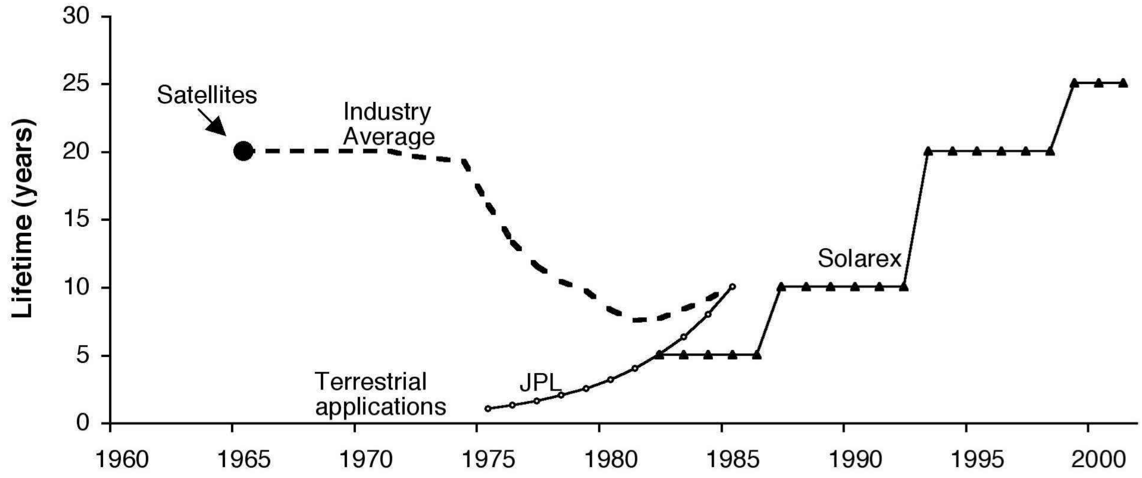

Figure 4.5: Module lifetime. Data: Moore (1982); Christensen (1985); Wohlgemuth (2003).

and mid-1970s to 5 years in 1980s, allowed the use of cheaper materials and less robust assembly processes that would have enabled less costly manufacturing.

Increasing demand elasticity

Another, and possibly complementary, explanation is that the shift from satellites to terrestrial applications affected prices because of a difference in the demand elasticity of the two types of customers. Price data from the period provide some supporting evidence. In 1974–79, the price per watt of PV modules for satellite use was 2.5 times higher than the price for terrestrial modules (Moore, 1982). The impact of this price difference on *average* PV prices is calculated by taking into account the change in market share mentioned above. The combination of these price and market shifts accounts for $22 of the $28 price decline not explained by the model in this period. Satellite customers, with their hundreds of millions of dollars of related investments, almost certainly had a higher willingness to pay for PV panels, than early terrestrial applications such as telecom repeater sites or buoys for marine navigation. The difference in quality must account for some of the price difference. But the difference in willingness to pay may also have led to higher differences between cost and price for satellite than for terrestrial applications.

Intensifying competition

Market share data indicate an increase in competition during this period. A decline in industry concentration typically produces an increase in competitiveness, a decline in market power, and lower profit margins. There were only two U.S. firms shipping terrestrial PV from 1970–1975 (Wolf, 1974; Maycock and Stirewalt, 1985). In 1978, about 20 firms were selling modules and the top three firms made up 77% of the industry (Roessner, 1982). By 1983, there were dozens of firms in the industry with the largest three firms accounting for only 50% of the megawatts sold (Maycock, 1984).

The Herfindahl-Hirschman Index (HHI) provides a way of measuring industry concentration (Hirschman, 1945; Herfindahl, 1950). The HHI is calculated by summing the squares of the market shares of all firms in an industry. The maximum possible HHI is 10,000.[16] The data show a trend to a less concentrated U.S. market during Period 1, 1975-

---

[16] The U.S. Department of Justice uses HHI to assess competitiveness in anti-trust decisions and considers industries with values below 1000 “unconcentrated”, 1000 to 1800 “moderately concentrated”, and values above 1,800 “highly concentrated” (DOJ, 1997).

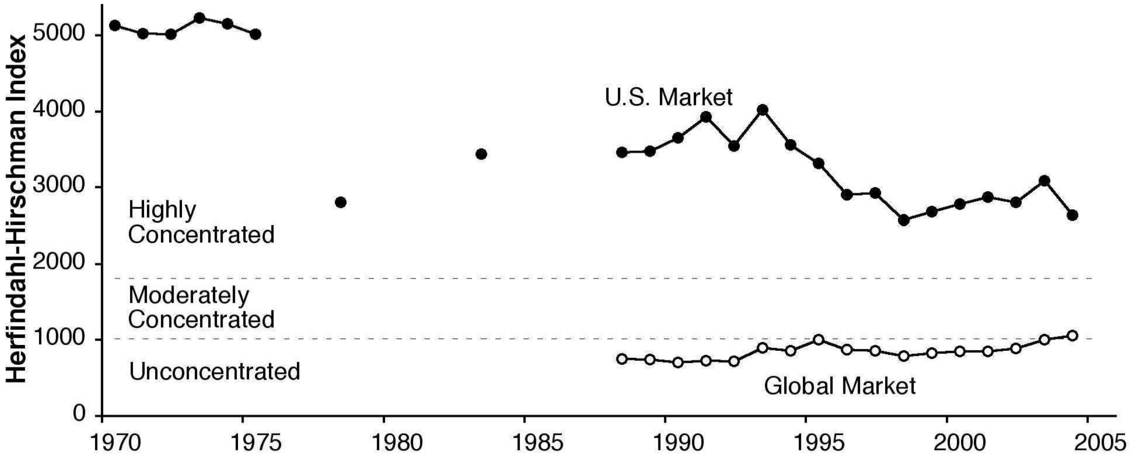

Figure 4.6: Industry concentration (Herfindahl-Hirschman Index). Data: Wolf (1974); Roessner (1982); Maycock (1984, 1994, 2002, 2005).

1979 (Fig. 4.6). Concentration in the global market remained stable in the 1990's, the period for which comprehensive worldwide data available. The increase in international trade in PV over the last three decades indicates that the relevant scale of analysis shifted from a national market in the earlier years to an international market today. Thus the most relevant measure of concentration would involve not only the trends in the curves themselves but also a shift from the upper domestic curve to the lower global curve.

Standardization

A final explanation for the change in cost is that changes in production methods occurred due to an increase in the number of customers and the types of products they demanded. There was a shift away from a near-monopsony market in the early-1970s when a single customer, the U.S. space program, accounted for almost all sales. In the terrestrial market, in contrast, the U.S. government accounted for only one third of terrestrial PV purchases in 1976 (Costello and Rappaport, 1980). With the rise of the terrestrial industry, a larger set of customers emerged over the course of the decade. One result from this change in the structure of demand was the shift away from producing customized modules, such as the 20kW panels on Skylab, to producing increasingly standard products at much higher volumes.

4.3.2 Period 2: 1980-2001

In the second period, from 1980 to 2001, PV cost declined by a factor of 7. In contrast to Period 1, the model explains the change in the second period well—just over 5% of the change is unexplained by the model (Table 4.2). The higher explanatory power of the cost model indicates that the factors mentioned above to explain the residual in Period 1—quality, demand elasticity, competition, and standardization—were either stable or were dynamic but offsetting in Period 2. Two factors stand out as important in Period 2: plant size accounts for 43% of the change in PV cost and efficiency accounts for 30% of the change (Fig. 4.7). The declining cost of silicon accounts for 12%. Yield, silicon consumption, wafer size, and poly-crystalline share each have impacts of 3% or less.

Table 4.2: Summary of model results for time period 2: 1980-2001

Factor	Change	Effect on module cost ($/W)
Plant size	125 kW/yr → 14 MW/yr	-9.22
Module efficiency	8.0% → 13.5%	-6.50
Si cost	131 $/kg → 25 $/kg	-2.67
Wafer size	48 cm^2 → 180 cm^2	-0.64
Si consumption	28 g/W → 18 g/W	-0.62
Yield	88% → 92%	-0.43
Poly-crystal	0% → 50%	-0.38
Sum of factors		-20.46
Actual change		-21.62
Residual		-1.16

4.3.3 Sensitivity of results to data uncertainty

The model is most sensitive to uncertainty in three areas: the change in plant size, the scaling factor, and the change in efficiency.[17] Fig. 4.8 shows that despite the model's sensitivity to uncertainty in these three areas, the relative importance of the three main factors does not change. Even with the relatively large uncertainty resulting from the choice of the scaling factor, the two orders of magnitude increase in plant size makes it the dominant driver of change in cost. So taking into account the full range of uncertainty in each parameter and conservatively assuming a uniform distribution across the estimates obtained, it can still be concluded that: (1) Module efficiency and plant size were the most important con-

---

[17] Uncertainty is calculated based on the full range of estimates obtained. The sensitivity of the model is estimated using opposite ends of ranges to simulate the extremes of large changes and small changes in each factor from 1975-2001. For example, in the case of efficiency, a *small change* is calculated using the upper bound in 1975 and the lower bound in 2001. Similarly, a *large change* consists of the time series using the lower bound in 1975 and the upper bound in 2001.

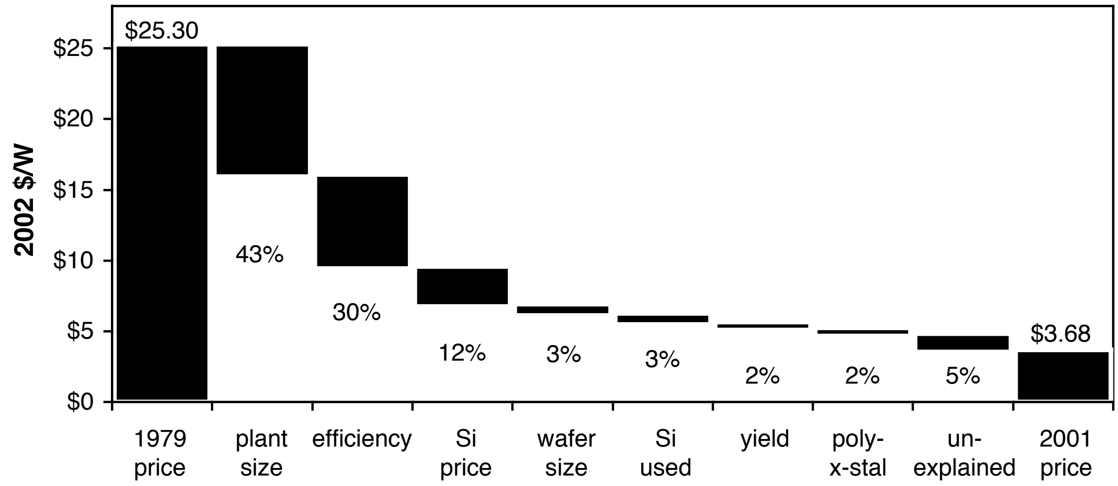

Figure 4.7: Portion of cost reduction accounted for by each factor, 1980-2001

tributors to cost reduction, (2) cost of silicon was moderately important, and (3) the other factors were of minor importance. This finding on the importance of economies of scale fits with other studies estimating the contribution of economies of scale to cost reduction in wind power such as Madsen *et al.* (2003) who estimated that scale accounted for 60% of reductions in turbine costs.[18]

4.4 Limits to the Explanatory Power of Experience

Experience curves are based on the theory that experience creates opportunities for firms to reduce costs and that as a result costs decline in logarithmic proportion to increases in

---

[18] One reason that the effect of economies of scale is apparently larger for wind than for PV is that wind benefits both from economies of *unit scale* (larger turbines) as well as economies of *manufacturing scale* (larger production facilities). To date, PV has not benefitted meaningfully from larger systems and depends primarily on expanding manufacturing facilities to take advantage of economies of scale.

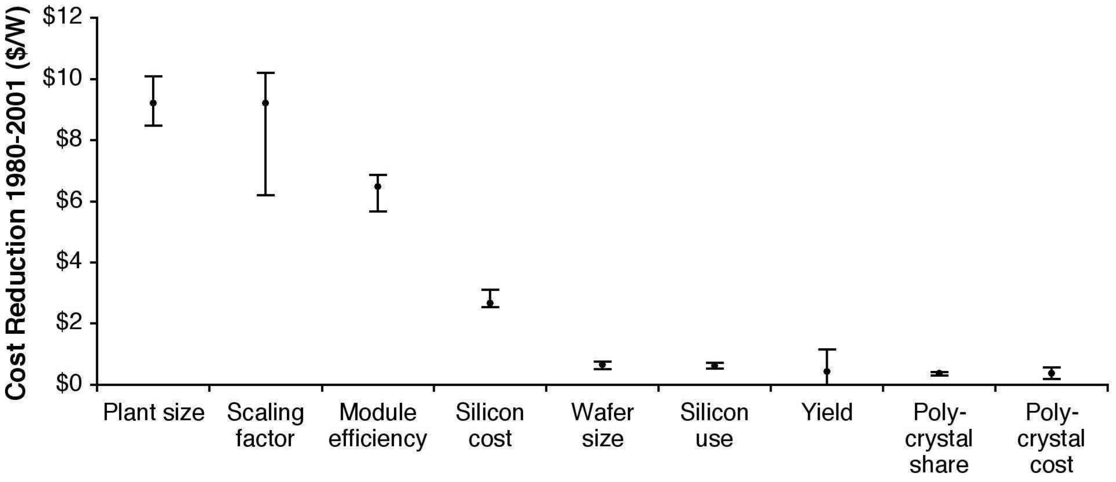

Figure 4.8: Sensitivity of model results to uncertainty in data and parameters, 1980-2001

cumulative capacity. Indeed, in the case of PV, cumulative capacity is a strong predictor of cost.[19] However, the mechanistic basis for this apparently strong statistical relationship is rather weak. In this section, the influence of increasing cumulative capacity in driving change in the most important cost-reducing factors is assessed. The results indicate that the most important factors are only weakly explained by cumulative capacity (Table 4.3). Overall, the "learning" and "experience" aspects of cumulative production do not appear to have been major factors in enabling firms to reduce the cost of PV, which is the assumption underlying the experience curve model.

---

[19] $log(\mathrm{CumCapacity})$ as a predictor of $log(\mathrm{C})$ has an R^2 value of 0.985.

Table 4.3: Role of learning-by-doing (lbd) in each factor, 1980-2001

Factor	Cost impact	Main drivers of change in each factor
Plant size	43%	Demand and risk management
Efficiency	30%	R&D, some lbd for lab-to-market
Silicon cost	12%	Spillover benefit from IT industry
Wafer size	3%	Strong lbd
Si use	3%	Lbd and technology spillover
Yield	2%	Strong lbd
Poly share	2%	New process, lbd possible
Other factors	5%	Not examined

4.4.1 Experience and plant size

Growth in expected future demand and the ability to manage investment risk were the main drivers of the change in plant size over the period. Whether experience plays a role in enabling the shift to large facilities depends on whether new manufacturing problems emerge at larger scales and whether experience helps in overcoming these problems. Examples from three PV firms indicate that limited manufacturing experience did not preclude rapid increases in production. Mitsubishi Electric expanded from essentially zero production in 1997 to 12 MW in 2000 and plans to expand to 230 MW in 2006 (Jaeger-Waldau, 2004). While the firm had decades of experience in research and satellite PV applications, its cumulative production was minimal. It only began substantial manufacturing activity with the opening of its Iida plant and its entry into the Japanese residential PV market in 1998. Similarly, Q-Cells, a German firm, only began producing cells in 2001 with a 12 MW line and increased production to 50 MW in only two years (Maycock, 2005). Sharp is considering construction of a 500 MW/year plant in 2006, which would amount to a ten-fold expansion in the firm's capacity in only 5 years. In the rapid expansions of the past five years, the ability to raise capital and to take on the risk of large investments that enable construction of large manufacturing facilities appear to have played more important roles than learning by experience in enabling cost reductions. These results support the claim of Dutton and Thomas (1984) that "sometimes much of what is attributed to experience is due to scale."

4.4.2 Experience and module efficiency

Learning-by-doing is only one of several reasons behind the doubling in commercial module efficiency. Data on the highest laboratory cell efficiencies over time shows that of the 16 advances in efficiency since 1980 (Surek, 2003)[20], only six were accomplished by firms that manufacture commercial cells. Most of the improvements were accomplished by universities, none of which would have learned from experience with large-scale production. That government and university R&D programs produced 10 of the 16 breakthroughs in cell efficiency while producing a trivial amount of the industry's cumulative capacity suggests that the effect of learning-by-doing on improving module efficiency is weak. Further, the rapid rise in laboratory cell efficiency from 1983-1990 (Fig. 4.9) immediately followed the unprecedented $1.5b investment in worldwide PV R&D in the previous 5 years (IEA,

---

[20] 'Advances' are defined as new production of cells that resulted in a cell efficiency higher than any previous laboratory result.

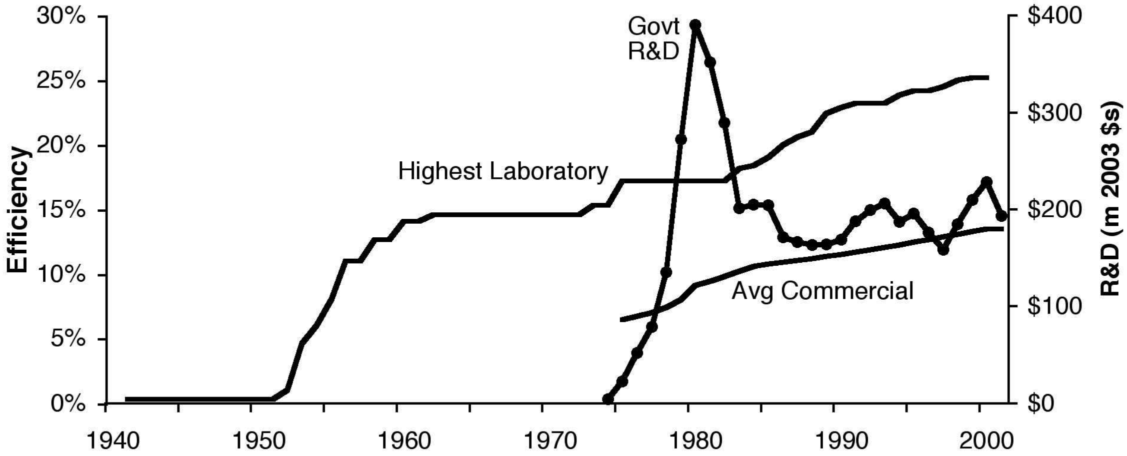

Figure 4.9: Public R&D and crystalline PV efficiency (highest laboratory cells and average commercial modules). Data: Christensen (1985); Maycock (1994, 2002); Grubb and Vigotti (1997); Menanteau (2000); Green *et al.* (2001); Nemet and Kammen (2007).

2005).[21] Experience may help firms generate ideas for incremental efficiency improvements. It may also play a role in facilitating the transition from producing efficient cells of a few watts in a laboratory to producing large modules that can operate reliably under ambient conditions. Still, if the underlying driver of changes in commercial efficiency is incorporating laboratory improvements into commercial manufacturing, then competing hypotheses such as R&D offer more compelling explanations of efficiency improvements than learning-by-doing.

---

[21] Although estimating the lags between R&D investments and their effects is difficult (Griliches, 1998).

4.4.3 Silicon cost

Reductions in the cost of purified silicon were a spillover benefit from manufacturing improvements in the microprocessor industry. During the study period, the PV industry accounted for less than 15% of the world market (Menanteau, 2000) for purified silicon. Since the PV industry, until recently, did not purify its own silicon, but instead purchased silicon from producers whose main customers are in the much larger microprocessor industry where purity standards were higher, experience in the PV industry was irrelevant to silicon cost reductions.

4.4.4 Other factors

Learning-by-doing and experience play more important roles in the following factors. However, these factors together only account for 10% of the overall change in cost.

Yield: Experience would have led to lower defect rates and the utilization of the entire wafer area.

Wafer size: Experience was probably important in enabling growing larger crystals and forming longer conductors from cell edges to electrical junctions.

Silicon consumption: Experience helped improve sawing techniques so that less crystal was lost as saw dust and thinner cells could be produced. The development of wire saws, a spillover technology from the radial tire industry, is less clearly related to experience.

Poly-crystalline share: Casting of rectangular multi-crystalline ingots was a new technology that only partially derives from experience with the Czochralski process for growing individual crystals.

4.5 Improving Policy Models

Learning derived from experience is only one of several explanations for the cost reductions in PV. Its role in enabling changes in the two most important factors identified in this study—plant size and module efficiency—is small compared to those of expected future demand, risk management, R&D, and knowledge spillovers. This weak relationship suggests careful consideration of the conditions under which one should rely on experience curves to predict technical change. Further, the importance of market dynamics identified in Period 1 advises extra caution when applying experience curves to technologies at early stages, such as might currently be considered for fuel cells, as well as carbon capture and sequestration. Below, the importance of firms' profit margins is discussed as an additional area to consider. The ways in which a bottom-up model such as this one might be used as a complement to experience curves to enhance our understanding of future technical improvements are also described. As an example, this model is applied in a simple scenario exercise to gauge the plausibility of future cost targets.

4.5.1 Incorporating market structure

The model results for Period 1, 1975–79, indicate that prices are not a reliable proxy for costs. Sensitivity analysis confirms that our price-based experience curve is sensitive to changes in margin. A plausible scenario based on historical data is that margins fell from 30–50% in the early years to near zero at the end of the study period. Such a shift would reduce the learning ratio by 0.03–0.05 and extend the crossover year by 8–15 years.[22]

Empirical data in this case study do not support three assumptions that are commonly made when applying the experience curve model using prices rather than costs: that margins are constant over time, that margins are close to zero with only minor perturbations, and that margins are often negative due to forward pricing. Indeed, earlier work pointed out that firms' recognition of the value of market domination, particularly during incipient commercialization, leads to unstable pricing behavior (BCG, 1972). An implication of the variation in the price–cost margin is that industry structure affects the learning rate. In the case of an industry such as PV that becomes more competitive over time, a price-based experience curve *over*-estimates the rate of technical progress.

One solution would be for future work to obtain real cost data where possible. An alternative would be to use an approach, such as that of Irwin and Klenow (1994), in which costs can be derived from prices and market shares using the assumptions in Cournot competition: that firm profit margins decrease as the number of firms in the market increases

---

[22] Using assumptions of 15% annual new capacity growth and a target module price of $0.30/Watt.

and that a single firm's profit margins increase with that firm's market share. However, comparisons of competing technologies are best made on the basis of prices, not costs, since prices reflect what a consumer faces in deciding whether and which technology to adopt. A more general approach would be to incorporate market dynamics into predictions of technological change. Industry concentration, market power, and changes in elasticity of demand affect prices. The HHI analysis above shows that concentration is not stable over time, especially if international trade is taken into account. The assumptions of perfect competition and that prices equal marginal cost are too strong in the early stages of the product life-cycle when the technology is improving rapidly, industry structure is unstable, and new types of customers are entering the market.

4.5.2 Technical factors and uncertainty

These results indicate that the confidence with which we use experience curves to predict technological change might be enhanced with analysis of the underlying technical and market dynamics. This type of approach is suggested by other studies that recommend multiple, complementary methods to inform policy decisions related to energy technology (Neij *et al.*, 2003; Taylor *et al.*, 2003). The combination of disaggregated technical factors and experience curves could inform policy decisions in three ways.

The explicit analysis of technical factors helps identify future barriers that could lead to discontinuities in the slope of the experience curve.[23] Assuming that some of these barriers may be surmountable, it may also help identify critical R&D areas. Identifying barriers might also allow us to predict, or at least gauge the probability of, discontinuities in the experience curve.

Additionally, the unraveling of technical factors provides an avenue for the investigation of how influences other than cumulative capacity, such as R&D and knowledge spillovers, contribute to technological change. For example, in the case of PV, firm-level analysis of the drivers behind the doubling in commercial efficiency over the period may enhance our understanding of the roles of R&D, cumulative capacity, and the interaction of the two. This approach would complement econometric investigations of the roles of these factors, such as that of Watanabe *et al.* (2003).

Finally, a model such as this one allows us to work backwards so that one can identify the level of technical improvement in each factor required for a given cost improvement. For example, if reducing the cost of PV by an additional factor of 10 became a goal, one could ask how large manufacturing plants would need to be to provide adequate economies of scale. With the resulting estimate for plant size, one could then assess whether individual plants are likely to ever reach that scale and the extent to which economies of scale would still exist for facilities that large. This type of analysis provides a basis for assessing the likelihood that such an improvement might occur, which could help estimate uncertainty in

---

[23] For example, the theoretical limit on the efficiency of single-junction silicon-based PV modules of approximately $\eta = 0.29$ constrains the cost reductions we can expect in the future from this generation of PV technology.

the pace of future cost reductions.

4.5.3 Scenarios of target costs

One might also use such a model to test the plausibility of long-term targets for PV cost reduction. Here, two cost targets are examined using the following assumptions;

- Efficiency improves from 13.5% in 2001 to 25% in 2030 (SEIA, 2004).
- Wafer thickness declines by 25% per decade, its historical rate.
- Scaling factor is -0.13, the mid-range of studies of large scale PV.
- A net increase of one additional manufacturing plant per year.
- No changes to the price of silicon or yield.

We first test the industry's roadmap goal of $1.00/W modules in 2050 (SEIA, 2004). Using the assumptions above, the model indicates that meeting such a goal would imply an industry growth rate of 11% for the next 45 years. At that point, 1.3 TW of PV modules would have been installed at a cost of $1.5 trillion. In 2050, each of 71 PV plants would be manufacturing 1.9 GW of modules annually.[24] In this scenario, 51% of the cost reduction comes from scale and 48% comes from efficiency improvements. These results are roughly

---

[24] A recent National Renewable Energy Laboratory study providing a detailed analysis of a 2.1 to 3.6 GW PV plant describes such a plant as feasible (Keshner and Arya, 2004).

similar to projections for large scale PV discussed by Schaeffer *et al.* (2004) (46% and 31% respectively).

Others claim that \$1.00/W modules would be prohibitively expensive once PV accounts for more than 5 to 10% of electricity generation. At such scale, the costs of electricity transmission and storage required to provide reliable service to an increasingly urbanizing world population would be so large that the cost of PV modules will have to be a minor component of the cost of PV-intensive energy systems. Under this line of reasoning, modules that cost \$0.10/W in 2050 might be a goal. The model suggests that this goal is not possible given the assumptions above and an additional constraint that installed PV cannot exceed 30 TW in 2050.[25] In an extremely high-growth scenario in which PV capacity does grow to 30 TW in 2050, this model predicts that module costs would only fall to \$0.63/Watt. Projected efficiency improvements, thinner wafers, and economies of scale are insufficient to bring the cost of crystalline PV to \$0.10/W. If such a cost target is indeed required then other types of cost reductions, such as switching to other materials like thin-films and organics, will be necessary. Such a change would probably represent a shift to a new technological paradigm (Dosi, 1982) and might be best understood using a model of overlapping technological generations (Irwin and Klenow, 1994), rather than a single learning curve.

---

[25] 30 TW is a high end estimate for total world energy demand in 2050.

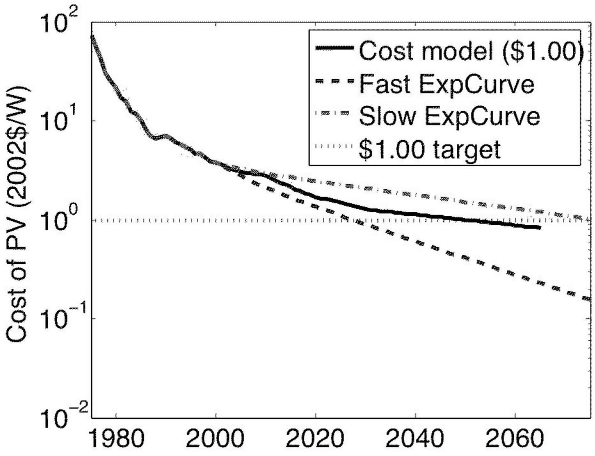

Figure 4.10: Scenarios comparing cost model, experience curves, and $1.00/W target price.

A similar scenario using experience curves provides a different outcome. A simple extension of the historical (1975-2001) learning rate, 0.23, using an assumption of 11% growth, would deliver $1.00/W modules in 2027 and $0.10/W in 2086 (Fig. 4.10). However, choosing which time period to use for calculating the learning rate expected in the future substantially affects the outcome. For example, a more conservative learning rate, 0.10, that might be projected using more recent trends, would delay $1.00/W modules from 2027 until 2076. The experience curve does not necessarily produce a faster or slower result than the technical factors model. It does however produce radically different outcomes as a result of apparently inconsequential choices, such as the period over which the learning rate is calculated.

Finally, future work on PV might be expanded to consider not only capital cost but the cost of PV electricity produced. In assessing experience in wind power, Dannemand

Andersen (2004) concluded that the cost of electricity is a more comprehensive measure of technological improvement than capital cost because technological competitiveness is ultimately based on decisions concerning electricity cost. Such an approach requires additional data that may be much more difficult to obtain. It also requires including factors such as interest rates whose level is exogenously determined but which are influential as they have varied by a factor of three over this study period. Using data on module and balance of system prices, system lifetimes, capacity factors, and interest rates, an experience curve for PV electricity is plotted in Fig. 4.11 and is compared to its primary technological competitor, retail electricity rates. Further work might consider what additional dynamics might need to be included to explain change in the PV cost of electricity curve, for example, the role of learning by doing among system installers. Also, the introduction of real-time pricing in electricity markets would also be important to consider because during the peak sunshine hours of the day, PV electricity would compete with rates substantially higher than the average rates (Borenstein, 2005).

4.5.4 Implications for modeling and policy

The inclusion of experience curves in models that optimize and simulate the costs of climate policy has enhanced their realism. Given the vast set of results showing that energy technologies improve over time, incorporating experience curves represents a substantial improvement over omitting them and implicitly assuming a learning rate of zero. But the

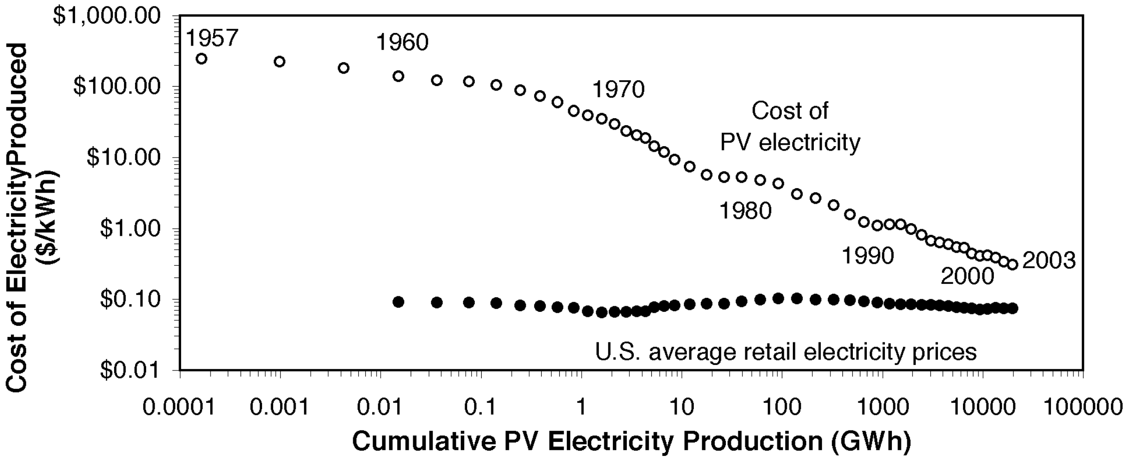

Figure 4.11: U.S. electricity prices and levelized cost of electricity produced from PV modules. Data: module and BOS prices (Wolf, 1974; Maycock, 2002; Strategies-Unlimited, 2003), lifetime (see Fig. 4.5), interest rates (Census, 2005), retail electricity prices (EIA, 2004).

results of this study indicate that, at least for the case of PV, a broader set of influences than experience alone contributed to the rapid cost reductions in the past; one implication is that experience curves *over*-estimate the technical improvements that should be expected to accrue from deployment alone. As a result, these findings support the efforts by modelers to explore ways of incorporating explanatory variables other than cumulative capacity. Future models will need to take into account the effects of factors such as public and private R&D, knowledge spillovers, technological opportunity, and market dynamics to more realistically inform decisions about large investments in future energy technologies.

If innovation is central to making the cost of climate policy affordable and market failures require government support for innovation, then these results suggest that an efficient policy is unlikely to be one that simply relies on ‘riding down the learning curve.’ Future work might examine the benefits of a shift in focus toward the design of a set of policy instruments that create incentives for firms to make investments in cost-reducing activities, while acknowledging that payoffs are inherently uncertain and may take several years to be realized.

Appendix 4.A Code for PV Cost Model

Matlab code:

```
% Greg Nemet, PV Cost Model

clear all;

graphs = 0; % set to 1 to plot data

format compact


%% Input data for PV 1975-2001

[pvdata] = xlsread('pv75-01.xls');

[t k] = size(pvdata);


year            = pvdata(:,1); % year

actpx           = pvdata(:,2); % actual module prices in 2002 $/Watt

eff             = pvdata(:,3); % module efficiency W out/ W in

plsize          = pvdata(:,4); % avg production in MWs per plant

yield           = pvdata(:,5); % usable modules/all modules

wafer           = pvdata(:,6); % area of wafer cross-section in cm^2

sicost          = pvdata(:,7)/1000; % $/g for purified si in 2002$s

siconsum        = pvdata(:,8); % grams of si used per watt of module

polyshr         = pvdata(:,9); % market share for poly-crystalline

cumprod         = pvdata(:,10); % cumulative annual production

labels = ['year    ';'actpx   ';'eff     ';'plsize  ';'yield   ';...

          'wafer   ';'sicost  ';'siconsum';'polyshr ';'cumprod ';];
```

```
% other parameters

b = -0.18  ; % scaling factor for economies of scale

p = 0.90    ; % cost of poly-crystalline/cost of mono-crystalline

pw = 0.4   ; % post-wafer processing as % of total manf. cost

fw = 0.1   ; % Percent of post-wafer costs that are fixed per wafer


%% Sensitivity Analysis

% plsize =[ ];


%% Calculate annual change in cost due to each factor

changes = NaN.*ones(t,8);

for i = 2:t

   changes(i,1) = actpx(i-1)*((eff(i-1)/eff(i))-1);

   changes(i,2) = actpx(i-1)*(((plsize(i)/plsize(i-1))^b) -1 );

   changes(i,3) = actpx(i-1)*((yield(i-1)/yield(i))-1);

   changes(i,4) = actpx(i-1)*((wafer(i-1)/wafer(i)) - 1)*pw*fw;

   changes(i,5) = sicost(i)*siconsum(i-1) - sicost(i-1)*siconsum(i-1);

   changes(i,6) = sicost(i-1)*siconsum(i) - sicost(i-1)*siconsum(i-1);

   polycost(i-1,1) = p*(actpx(i-1)/(1 - (1 - p)*polyshr(i-1)));

   changes(i,7) = (polyshr(i)-polyshr(i-1))*(polycost(i-1) - actpx(i-1));


   predchg      = sum(changes(:,1:7),2);    % predicted values

   actchg(i,1)  = actpx(i) - actpx(i-1);    % actual price change

   changes(i,8) = actchg(i) - predchg(i);   % unexplained change
```

```
end


%% Summary of percent contributions from each factor

change      = sum(changes(2:t,:));

changep1    = sum(changes(2:5,:));

changep2    = sum(changes(6:t,:))

contrib = sum(changes(2:t,:))./sum(actchg(2:t)) ;    % all periods

contribp1 = sum(changes(2:5,:))./sum(actchg(2:5));   % Period 1: 1975-1979

contribp2 = sum(changes(6:t,:))./sum(actchg(6:t));  % Period 2: 1980-2001



%% plot data

if graphs == 1

    figure(1)

    for i=2:k

        subplot(3,3,i-1)

        scatter(year,pvdata(:,i),1)

        axis tight

        ylabel(labels(i,1:8))

    end

    print -dpdf pvdata.pdf;

end
```

Appendix 4.B PV Time-Series Data

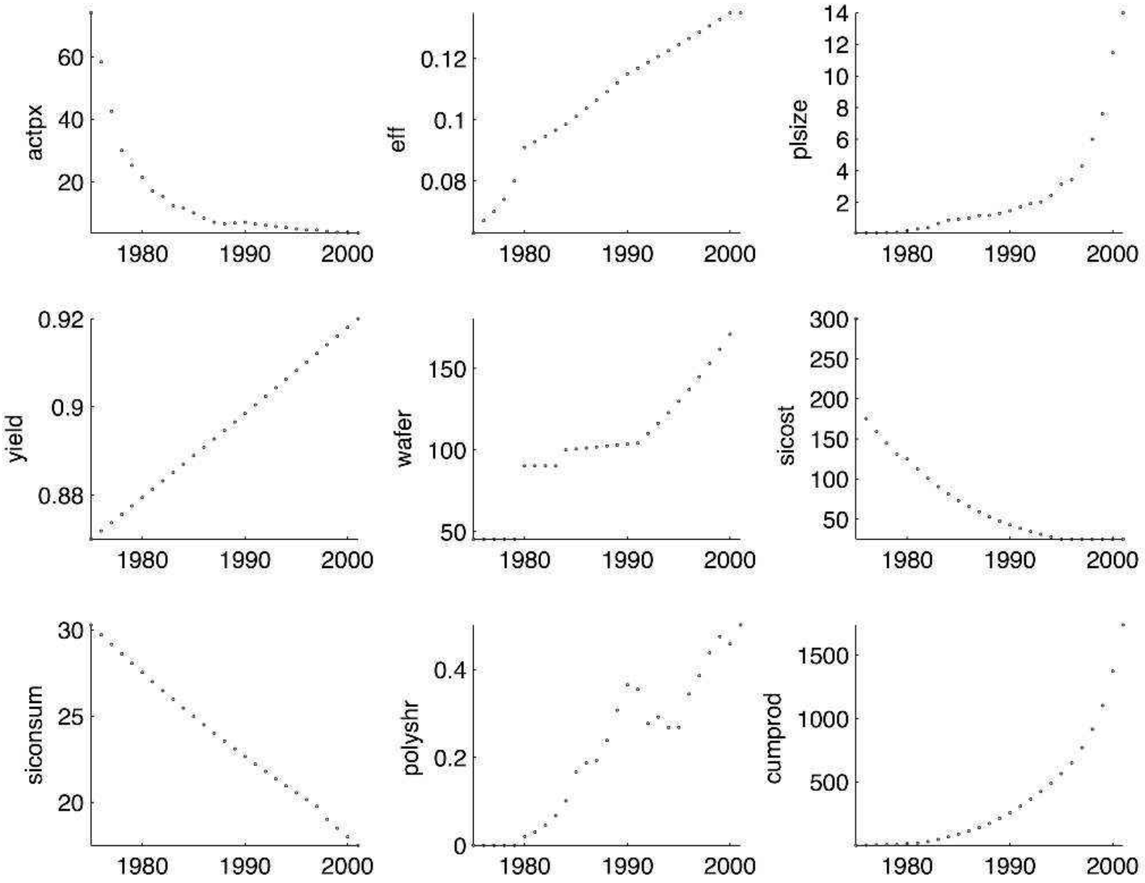

Figure 4.12: Summary of time series data assembled for this study.

Chapter 5

Beyond Diffusion: Net Radiative Forcing from Widespread Deployment of PV

Following Chapter 4, this chapter maintains the focus on understanding the mechanisms within the innovation process itself. The specific area of concern here is with the far end of the innovation process, as shown in Fig. 1.5. Widespread diffusion is the ultimate fulfillment of the innovation process. The early stages are intermediate steps and represent potential, but the environmental benefits of low-carbon energy technologies only accrue when they are used—and used widely—so that they displace more carbon-intensive energy sources. This chapter uses a simple model to explore one important aspect of what might happen at very large scale diffusion in the case of PV.

5.1 An Unintended Consequence?

Demand for energy continues to grow as population and personal resource consumption increase. From the perspective of the climate change problem, PV is an attractive way to meet this demand because PV systems produce energy without emitting greenhouse gases (GhGs). Moreover, systems are getting cheaper and installations are growing quickly (see Fig. 4.2). The potential available resource is large. But PV can only contribute to stabilizing the climate if it is makes a meaningful contribution to world energy supply and displaces carbon-intensive forms of energy production. To do that multiple terawatts (TW) of PV need to be deployed.

However history shows that adverse societal effects can emerge from apparently beneficial technologies when they are deployed at very large scale (Ruttan, 2001). In this vein, other studies have looked at the possible impacts of widespread PV, including: the use of toxic chemicals in the PV production process (Fthenakis and Moskowitz, 2000), the availability of raw materials (Andersson and Azar, 1998), and the extent to which PV is a net producer of energy over its lifecyle (Alsema and Nieuwlaar, 2000). An additional concern, which has been expressed but not as rigorously evaluated, is that covering a large portion of the earth's land area with light-absorbing solar panels will reduce the reflectivity of the earth's surface, thereby increasing radiative forcing and ultimately having a positive effect on global surface temperatures (Ausubel, 2001; Seitz, 2005). This chapter weighs the merits of this concern.

In this chapter, I created scenarios of PV deployment, based on assumptions used by the Intergovernmental Panel on Climate Change (IPCC), projected energy consumption, and logistic functions found in the literature on technological change. I then calculated land area covered using data on trends in the electrical conversion efficiency of PV panels. The effect on the earth's albedo was calculated using estimates of the reflectivity of various land covers and of PV panels, as well as assumptions about where the panels would be installed. I used historic trends in the carbon intensity of the world's energy supply to estimate the emissions avoided by the substitution of PV for fossil fuels. I then compared the positive radiative forcing caused by the change in albedo to the negative radiative forcing due to offsetting fossil fuel emissions. Finally, I performed sensitivity analysis and used the results to estimate the value of several possible technical improvements for enhancing the value of PV as a climate stabilization option.

5.2 Radiative Forcing due to PV Albedo Effect

This section estimates the effect of widespread use of PV on the earth's albedo and the extent of the resulting radiative forcing.

5.2.1 Amount of PV Installed

To address this highly uncertain factor, I construct two scenarios. In the first, I draw on results of an analysis by Nakicenovic and Riahi (2002) of the assumptions on technology deployment used by the IPCC in its Special Report on Emissions Scenarios (SRES)(Nakicenovic *et al.*, 2000) and Third Assessment Report (TAR)(Metz *et al.*, 2001). For this meta-scenario, I use the median value of PV deployment across these 34 emissions scenarios.[1] At this median level for 2100, PV would supply 6% of world energy use.

For the second scenario, I construct a "high-diffusion" path, which represents a likely upper bound on PV diffusion in 2100. I rely on the IPCC scenarios described above to arrive at world energy demand in 2100 and assume that PV accounts for 50% of world energy supply.

So far, these two scenarios describe end-points for 2100, as well as a few values along the way. To describe a path over time I turn to the literature on technology diffusion. Dating back to Rogers (1958) and Griliches (1960), a wide array of empirical case studies have found that new technologies tend to diffuse into widespread use according to a logistic function (Mansfield, 1961; Fisher and Pry, 1971).[2] Adoption of technology tends to be slow early on when reliability is unproven and only early adopters risk using the new device.

---

[1] I convert the median values calculated by Nakicenovic and Riahi (2002) from annual exajoules of energy produced into terawatts (TW) of installed capacity using a capacity factor for PV modules of 20%, equivalent to about five hours of peak sunlight per day (Stackhouse and Whitlock, 2005).

[2] Cumulative diffusion can be described according to a logistic function because the population of early adopters, intermediate adopters, and laggards is normally distributed.

Diffusion accelerates as initial problems are worked out and complementary innovations enable widespread adoption. Finally, diffusion slows as substitutes emerge and the market reaches saturation. This function takes the form

$$Q_t = \frac{Q_{max}}{1 + e^{-(a+b(t-t_0))}} + k \tag{5.1}$$

where: Q_t is the installed capacity of PV at time, t; a is the constant of integration used to position the curve horizontally on the time scale; b is the rate of growth coefficient; k is the amount of PV installed at time, $t = 0$; and Q_{max} is the difference between the saturation level and k.

I fit two logistic curves to historical data, as well as the points in the scenarios described above, to create a PV *diffusion curve* (Figure 5.1). The lower line shows the amount of PV installed using the median of the IPCC scenarios and the upper line shows terawatts installed in the high-diffusion scenario, in which PV supplies 50% of world energy demand in 2100.

5.2.2 Land Area Covered by PV

The area of the earth's surface covered by PV panels, A_t is related to the amount of PV deployed (Q_t) in units of Watts of output at peak capacity, electrical efficiency of the panels

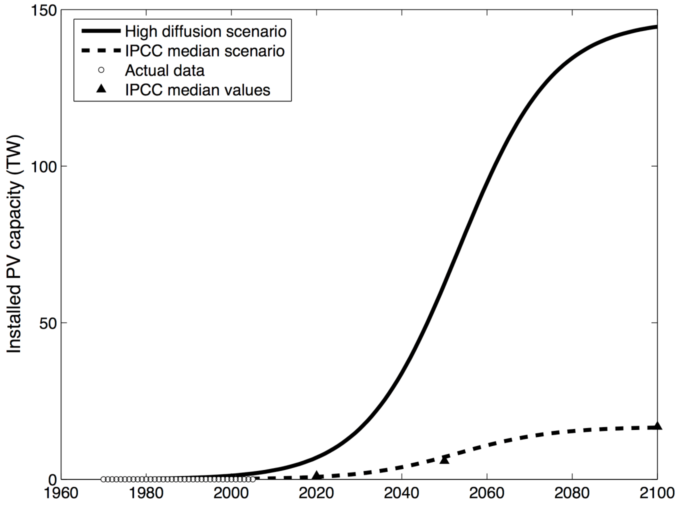

Figure 5.1: Technology diffusion curves for PV. Logistic function based on median values across emissions scenarios used by IPCC (lower) and scenario in which PV accounts for half of world energy supply in 2100 (upper).

(η_t), and peak incoming solar radiation (I_s), according to the following:

$$A_t = \frac{Q_t}{\eta_t I_s} \tag{5.2}$$

I create a time series for the electrical conversion efficiency of PV panels using historic trends (Nemet, 2006), industry targets (SEIA, 2004), and theoretical physical limits (the time series for η is Figure 5.6 in the Appendix).

$$\eta = \frac{\mathrm{W}_{peak}}{\mathrm{W}_{in}} \tag{5.3}$$

I assume peak incoming solar radiation (I_s) is 878 W/m^2. This is the annual average incoming solar radiation for 7 selected cities (see Table 5.3 in the Appendix) (Stackhouse

Table 5.1: Land area covered by PV panels (10^6 km^2).

Scenario:	2020	2050	2100
1) IPCC median	0.01	0.03	0.07
2) High-diffusion	0.04	0.28	0.60

and Whitlock, 2005).

$$I_s = \mathrm{W}_{in} = 878\,\mathrm{W/m^2} \tag{5.4}$$

I use the diffusion function in section 5.2.1 (eq. 5.1) and equation 5.2, to calculate the land area covered by PV panels (see Table 5.1). In the first scenario, using the mean of the IPCC scenarios, PV covers 0.07 million km^2 in 2100. This area is approximately equivalent to a 260 km × 260 km square and is about the size of Ireland. The land area covered in 2100 under the high-diffusion scenario is 0.60 million km^2, equivalent to a 770 km × 770 km square, a little larger than Madagascar.

5.2.3 Change in the Earth's Albedo

Photovoltaics use anti-reflective coatings and textured surfaces to maximize the incoming solar radiation absorbed by the cell, such that the 30% reflectivity value for raw silicon can be reduced to 1% in laboratory settings (Green, 1998). Poor siting and orientations can make real-world reflectivity values as high as 10% (Balenzategui and Chenlo, 2005). For this analysis, I assume that the reflectivity of solar cells (R_{pv}) is 5% (see Figure 5.2).

Although most PV systems today are on rooftops, the increasing use of energy services and urbanization of the world's population expected over this century imply that in many cases, the areal density of energy consumption (in W/m^2) will exceed that of incoming solar radiation once it has been converted into electricity. As a result, I assume half of the PV installed is on rooftops, a quarter in agricultural areas, or "grasslands", and a quarter in deserts. I represent these three types of land cover using the index, $c = 1$ to 3. I apply these proportions to the total land area covered by PV from above, to calculate the area covered in each type of landscape ($A_{c,t}$). I use the following albedo values:

$$\alpha_{\text{rooftop}} = 0.10,\ \alpha_{\text{grassland}} = 0.20,\ \alpha_{\text{desert}} = 0.25.$$

I calculate the effect of installed PV panels on the reflectivity of the surface of the earth (R_s) at each time, t using:

$$\triangle R_{s,t} = \sum_{c=1}^{3} \frac{A_{c,t}}{A_s} \left(R_{pv} - \alpha_c\right) \tag{5.5}$$

where A_{s} is the area of the surface of the earth, 510 million km^2. The annual impact on overall albedo of the planet (α) of a change in surface reflectivity can be approximated using:

$$\triangle \alpha_t = 0.28 \triangle R_{s,t} \tag{5.6}$$

Using, the high-diffusion scenario for PV, I calculate an approximate change in the earth's albedo of -0.0000367 or -3.67$\times 10^{-5}$.

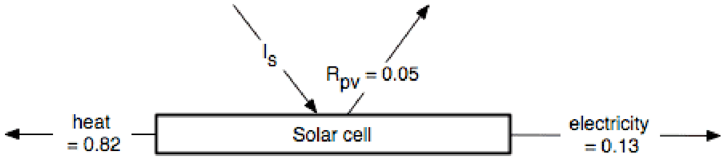

Figure 5.2: Energy flows in a solar cell. Values represent percentage of incoming solar radiation (I$_s$) in each flow.

5.2.4 Radiative Forcing from Albedo Change

The change in the earth's albedo can be converted into radiative forcing (RF) using the simplified energy balance equation:

$$\sigma T_s^4 = \frac{(n+1)\,\Omega\,(1-\alpha)}{4} - (F_c + 1.5F_e + 1.7F_s + +nF_w) \tag{5.7}$$

which for evaluating $\triangle\alpha$ reduces to:

$$RF_t = -\frac{(n+1)\,\Omega\triangle\alpha_t}{4} \tag{5.8}$$

where $n = 2$ radiation thicknesses, and $\Omega = 1368\,\mathrm{W/m^2}$.

Using, the high-diffusion scenario for PV, I calculate a radiative forcing of 0.0377 W/m^2 in 2100. Compare this ∼0.04 W/m^2 level to the radiative forcing caused by anthropogenic GhG emissions since pre-industrial times of 2.9 W/m^2, and to the albedo effect of previous land use change of between -0.4 and +0.2 W/m^2 (Houghton *et al.*, 2001). The radiative forcing in the first scenario is 0.004 W/m^2.

5.3 Radiative Forcing from Substitution of PV for Fossil Fuels

From a climate change perspective, the benefit of PV is that it can substitute for carbon-intensive energy supply. Here I calculate the total emissions of carbon avoided in each of the PV diffusion scenarios. The energy produced from PV systems (E_t) is a function of the amount deployed (Q_t), capacity factor (CF), and seconds in a year ($p = 3.15 \times 10^7$):

$$\mathrm{E_t} = Q_t CFp \tag{5.9}$$

The world energy system today emits GhGs with a carbon intensity of 17.1 T(C)/EJ (EIA, 2005). Today's level is about a third lower than it was 150 years ago (Grübler, 1998). For simplicity, I assume de-carbonization of the residual world energy supply, that excluding PV, continues linearly at its historical rate (see Figure 5.7 in the Appendix).

I calculate the carbon emissions offset by PV by multiplying energy produced (E_t) by carbon intensity (G_t). The emissions can be integrated from the initial deployment of PV until the end of the century to calculate total carbon emissions offset by PV (C).

$$C = \int_{1970}^{2100} G_t E_t \partial t \tag{5.10}$$

Figure 5.3 shows the annual emissions of carbon avoided per year for each scenario

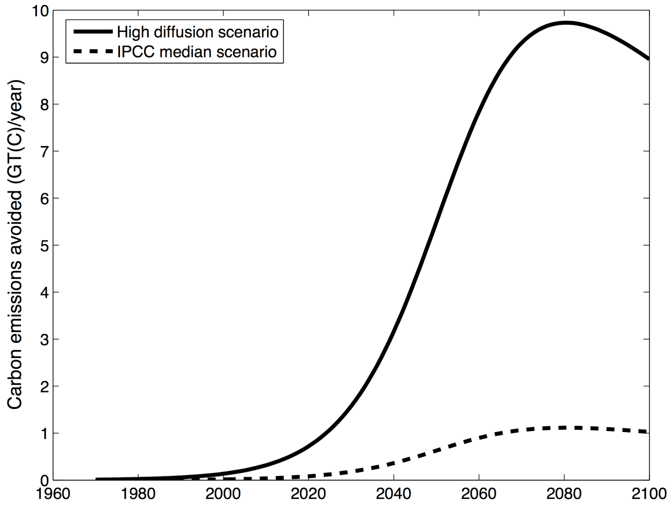

Figure 5.3: Carbon emissions avoided due to diffusion of PV (GT(C)/year). Lower curve is IPCC median scenario and upper curve is high-diffusion scenario.

of PV diffusion. PV offsets 528 GT of carbon emissions over the entire time period in the high diffusion scenario and 61 GT(C) in the IPCC median scenario.

528 GT is about two-thirds of the stock of CO_2 in the atmosphere today. If I assume half goes into oceans over the century, then atmospheric concentrations will rise by a third from today's level. The 100 ppm increase in atmospheric concentrations from 1750 to 2004 translates into 216 GT(C) and has resulted in 1.63 W/m^2 of radiative forcing (Houghton *et al.*, 2001). So if the relationship between carbon added and radiative forcing is approximately linear, the radiative forcing avoided due to the deployment of PV is about 2.0 W/m^2.

5.4 Net Effect and Sensitivity Analysis

A comparison of the results of sections 5.2.4 and 5.3 shows that the fossil fuel substitution effect is a factor of 53 times larger than the albedo effect. Figure 5.4 compares the radiative forcing due to albedo and fossil fuel substitution for each scenario. Historic radiative forcing for land use change and GhG emissions from 1750 to present are shown as a comparison (Houghton *et al.*, 2001). Next, uncertainty in the model parameters is estimated and the sensitivity of the ratio between the two radiative forcings to this uncertainty is assessed.[3] Figure 5.5 shows that, for the values evaluated, the range of the fossil fuel offset to albedo ratio, around the original value of 53, is 37 to 97. The following sections show how uncertainty in the main parameters is estimated.

5.4.1 PV system efficiency

The energy conversion efficiency of PV modules is assumed to increase to 28% by 2100. It is plausible, possibly through the widespread use of concentrators, that the average efficiency of commercial cells could reach 35%. I also test the scenario in which average efficiency in 2100 only reaches that of the highest commercial systems available today, 21%.

---

[3] No probabilistic assumptions have been made about the distribution of uncertainty in the parameters. As a result, the sensitivity analysis should be interpreted as indicating to which parameters the model is most sensitive, not as confidence intervals for statistical significance.

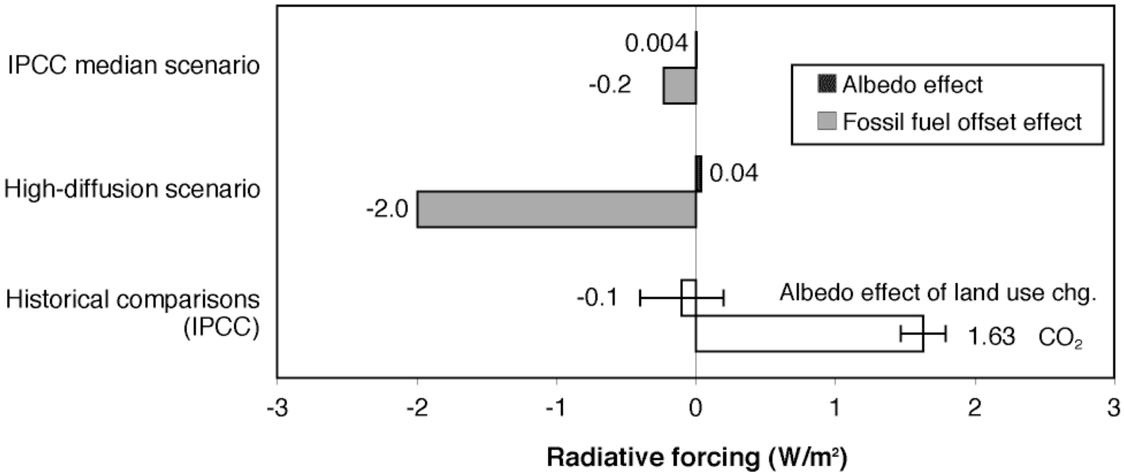

Figure 5.4: Comparison of radiative forcing due to albedo and fossil fuel substitution effects. Historic values for land use change and GhG emissions provided as a comparison.

5.4.2 Incoming solar radiation

The model uses an I_s based the average observed for seven large metropolitan areas worldwide. Here I test the impacts of assuming all the PV is installed in areas with incoming solar radiation similar to that of the sunniest metropolitan area (1190 W/m^2) and then for the least sunny area (622 W/m^2).

5.4.3 Albedo of ground cover

The model uses albedo values for ground cover of: $\alpha_{\mathrm{rooftop}} = 0.10$, $\alpha_{\mathrm{grassland}} = 0.20$, $\alpha_{\mathrm{desert}} = 0.25$. I test the sensitivity of the model to using values 25% higher and lower than the original values.

5.4.4 Ground cover type

The model assumes half of the PV installed is on rooftops, a quarter in agricultural areas, or "grasslands", and a quarter in deserts. If urban density is less of a constraint that originally imagined, one could assume that 75% is installed on rooftops, and the remainder split between croplands and desserts. At the other end of the range, it is plausible that land use and energy density constraints in populated areas mean that half is installed in desserts, and a quarter in the other two.

5.4.5 Reflectivity of PV panels

The model assumes that PV systems reflect 5% of incoming solar radiation, $R_{pv} = 5\%$. It was noted above that laboratory reflectivities can be as low as 1% and that those in poorly-sited field installations can be as high as 10%. Here I assess the impact on radiative forcing of assuming all PV is installed at the laboratory best, and then assuming all is installed like the poorly-sited systems.

5.4.6 De-carbonization

The model assumes that the carbon intensity of the world energy system (excluding PV) declines at its historical rate. It is conceivable, for example through the exploitation of abundant coal resources, that de-carbonization could cease and the carbon intensity of the world energy system in 2100 will be the same as it is today. Alternatively, one could

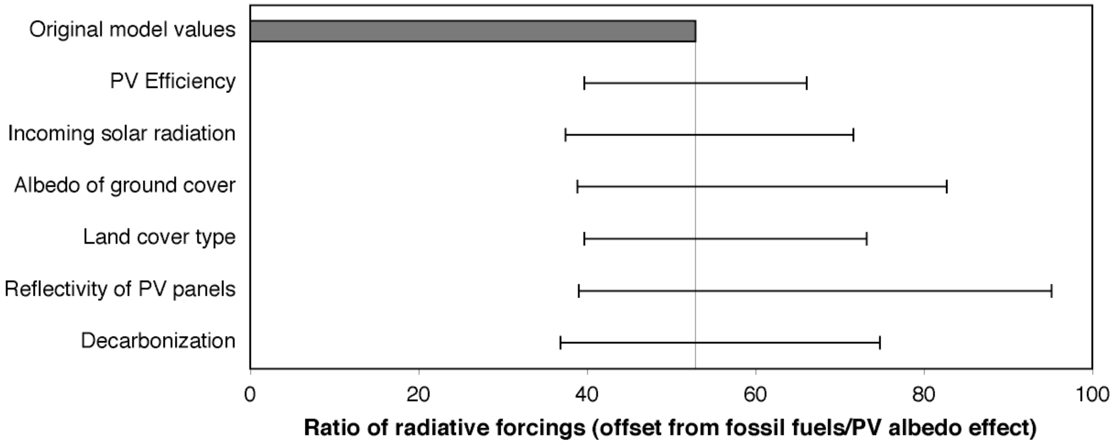

Figure 5.5: Sensitivity of radiative forcing ratio to uncertainty in parameters.

imagine de-carbonization accelerating such that in 2100 it reaches half the projected level it would have reached growing at its historical rate.

5.4.7 Simultaneous effects

When the uncertainty in each parameter is assessed individually, the sensitivity analysis supports the robustness of our conclusion that *the albedo effect is not substantial relative to the fossil fuels offset*. The size of the offset effect is never less than 37 times as large as the albedo effect (Figure 5.5).

But is it possible that the albedo effect might still be a concern if some of these uncertainties arrive in adverse combinations? For example, one unlikely but internally-consistent scenario is that all of the levels of all the uncertain parameters turn out to be at the least favorable end of their ranges; electrical conversion efficiencies are low, module reflectance values are low, systems are installed at locations that are not very sunny and above surfaces that are highly reflective, and the power production which PV substitutes for includes large amounts of renewables, nuclear, or other low-carbon sources of energy production. Even in this extremely unfavorable scenario, the fossil fuel offset is still 9.1 times as large as the albedo effect and PV remains an effective climate change mitigation option.

5.5 Improving the Effectiveness of PV for Climate Stabilization

The sensitivity analysis shows that PV remains an attractive climate change mitigation option even when the albedo effect reaches its maximum value within the range of uncertainty assessed in this chapter. Yet the uncertainties discussed above are not purely exogenous factors; they can be influenced by the actions of policy-makers, companies, and households. As a result, a second implication of this sensitivity analysis is that it points to future technical improvements in PV design and installations that can enhance the value of PV as a tool for climate change mitigation. Table 5.2 shows the effects on radiative forcing of five important improvements in PV system design and implementation. For example, row 2 of Table 5.2 indicates that changes in the design of PV modules that increase the light reflected from 5% of incoming solar radiation to 10%—such as coatings that selectively

Table 5.2: Effect of technical improvements on radiative forcing from PV

Improvement	Original Value	Improved Value	$\triangle$ Rad. Forcing (W/m^2)	% $\triangle$ Rad. Forcing Effect
PV efficiency	13%	28%	-0.04	-2.3%
PV reflectance	5%	10%	-0.04	-1.9%
Solar radiation (W/m^2)	622	1109	-0.05	-2.7%
Land cover (α)	0.2	0.1	-0.07	-3.8%
Energy substituted	Gas	Coal	—	-67.9%

reflect wavelengths below the semi-conductor material's bandgap—would improve PV's climate mitigation performance by 1.9%.

5.5.1 Module design

Improving module efficiency. Studies that focus on reducing PV's cost per unit of energy sometimes claim that we should be indifferent between a set of technical improvements that halves the cost of producing PV material, and one that doubles the efficiency of modules. Both create the same benefit in terms of reducing cost per unit of electrical output. But this analysis suggests that improving efficiency can have benefits beyond reducing cost. Ceteris paribus, a doubling of efficiency will halve the land area covered by PV panels thus halving the radiative forcing from PV's effect on the earth's albedo.

Increasing reflectance. PV modules are typically covered with coatings and surface textures that maximize the amount of light absorbed by the cell. However, it is feasible, that surfaces could be designed so that they more selectively reflect wavelengths that are unlikely to be converted into electricity by the semi-conductor material used in the cell.

5.5.2 Installations

Location. Installing systems in areas where incoming solar radiation is highest reduces the area of land required for each unit of electricity produced. Less land covered reduces the albedo effect.

Land cover type. Installing dark-colored PV panels on top of the land or structures with lowest reflectance values minimizes the albedo effect. For example, installations on low-albedo roofs are preferable to those on high-albedo surfaces such as deserts.

5.5.3 Substituted energy production

This chapter shows that the fossil fuel substitution effect is much larger than the albedo effect. PV maximizes its value as a climate change mitigation tool when it displaces the most-carbon intensive types of energy production. At one extreme, PV that displaces coal—the most carbon-intensive energy source—can have a large effect on reducing radiative forcing from carbon emissions. At the other, PV that displaces zero-carbon energy production, such as other renewables or nuclear power displaces no emissions; in that case, the albedo effect would make its net contribution to radiative forcing positive. The switch from gas to coal, shown in Table 5.2 shows how important the type of energy displaced is to total radiative forcing avoided.

5.5.4 Targeting improvements in PV beyond cost reductions

For the most part, efforts to improve the viability of photovoltaics have sought to find ways to reduce the cost of PV systems per unit of electricity generated (SEIA, 2004; Lewis, 2005). Cost reductions are certainly critical to the widespread deployment of PV. And a comparison of the two scenarios in Figure 5.4 shows that widespread deployment has a large effect on avoided radiative forcing. But cost per energy produced is really an inverse proxy for PV's climate-related value, rather than a precise metric, because cost does not comprehensively capture its usefulness as a tool for mitigating climate change. In addition to its benefits, PV has small adverse effects on climate stability. In particular, the carbon intensity of the energy production being displaced by PV is by far the most important factor affecting the net radiative forcing from PV.[4] The other technical changes shown in Table 5.2 also have substantial, albeit smaller, beneficial effects on climate. In addition, they have the advantage of perhaps requiring more modest changes to the energy system as a whole.

This chapter has found that the climatic benefits from substitution of PV for fossil fuels far outweigh the unfavorable effects due to the change in the earth's albedo. Still, by pursuing a path of technology development and deployment that minimizes the albedo effects of widespread deployment of PV, its value as a tool for climate change mitigation can be enhanced. Long-term monitoring of the directions of technical progress, the characteristics of installation sites, and the carbon intensity of the energy sources being displaced is im-

---

[4] Although, the actual power plants being displaced are difficult to identify and obtaining the gains from this improvement entails a substantial, perhaps unrealistic, degree of control over the entire energy system.

portant if we are to take full advantage of the potential of PV as a climate change mitigation option.

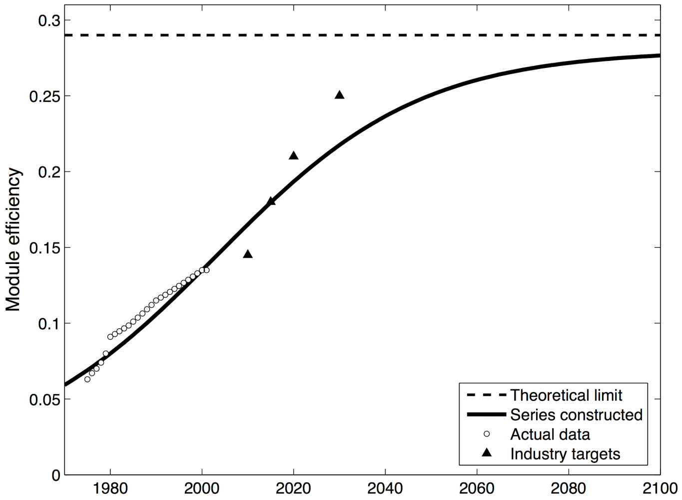

Figure 5.6: Time-series for the electrical conversion efficiency of PV modules.

Appendix 5.A PV, Solar, and Energy Data

5.A.1 Efficiency of PV panels

Figure 5.6 shows that the average commercial efficiency doubled from 6.5% in 1975 to 13% in 2001 (see Table **??**). The dashed line shows the theoretical limit on the efficiency of single-junction silicon-based PV of 29%. The solid curve is a logistic function fitted to historical data, the industry targets, and the theoretical limit.

5.A.2 Incoming solar radiation

Table 5.3 shows daily solar radiation data for seven large cities for each month of the year and averaged over the course of a year (Stackhouse and Whitlock, 2005). The average daily incoming energy is converted into a peak daily flow of energy using the assumption that

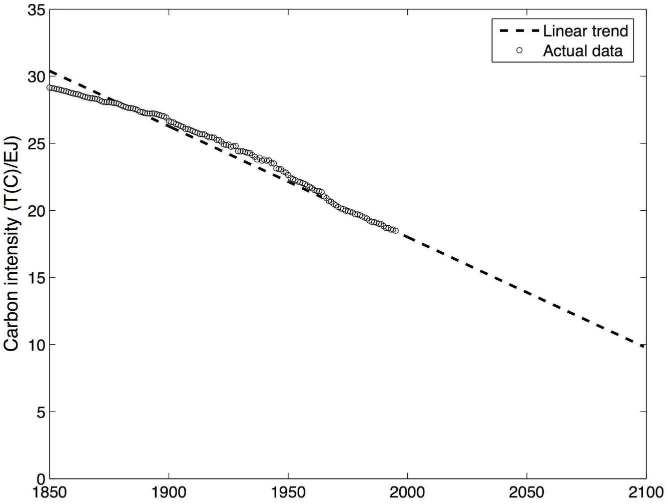

Figure 5.7: Carbon intensity of world energy use (tons of carbon per exajoule).

PV systems have a capacity factor of 20%. The peak incoming solar radiation averaged across the seven cities is 878 W/m^2, which is the value used in the model.

5.A.3 Carbon intensity of energy use

Figure 5.7 shows that the world has reduced the carbon intensity (carbon emitted per unit of energy produced) of energy use over time. In these calculations, I fit a linear function to the 150-year data series (Grübler, 1998) to estimate carbon intensity of world energy use, excluding PV, from 2005 until 2100.

Table 5.3: Solar radiation for seven selected cities

	10-year Average Daily Radiation on Horizontal Surface (kWh/m^2/day)													Peak I$_s$
City	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Avg.	(W/m^2)
Delhi, India	3.6	4.4	5.5	6.4	7.0	6.4	5.3	5.0	5.3	5.1	4.2	3.4	5.1	1024
Nairobi, Kenya	5.9	6.2	6.2	5.5	5.2	4.8	4.9	5.2	5.9	5.8	5.5	5.6	5.5	1109
Rio de Janeiro, Brazil	5.3	5.3	4.9	4.2	3.5	3.4	3.5	4.0	3.9	4.6	5.1	5.0	4.4	878
Sacramento, USA	2.1	3.3	4.5	6.1	7.3	7.8	7.5	6.6	5.3	3.9	2.6	1.9	4.9	981
Shanghai, China	2.7	3.1	3.4	4.6	5.1	4.8	5.3	5.1	3.9	3.6	3.1	2.7	3.9	788
Stuttgart, Germany	1.1	1.8	2.9	4.0	5.0	5.0	5.5	4.6	3.3	2.0	1.2	0.9	3.1	622
Tokyo, Japan	2.7	3.5	4.0	4.9	5.0	4.3	4.3	4.5	3.3	3.0	2.8	2.5	3.7	747
7-city average	3.3	4.0	4.5	5.1	5.4	5.2	5.2	5.0	4.4	4.0	3.5	3.1	4.4	878

Chapter 6

Summary and Implications for Policy

This chapter provides a summary of the results of the four studies and points to some specific policy implications of these findings. It then offers some broader observations about the full set of studies. It closes with a discussion of the public policy challenge of creating incentives for investments in innovation using long-term targets.

6.1 Case-Specific Findings and Policy Implications

6.1.1 Summary of empirical results

Chapter 2, on energy R&D, assessed the trends, effectiveness, and future prospects for the technology-push side of energy technology policy. R&D investment has been decreasing—most notably in the private sector. Across a variety of technologies, R&D spending has been well correlated with one widely-used measure of inventive output, patenting activity. A simple model of the insurance value of energy R&D—to address risks associated with air pollution, climate change, and supply disruptions—was used to arrive at a benchmark target of increasing spending levels to 5 and 10 times higher than current levels. A comparison of past R&D programs showed that scaling up energy R&D to these levels over ten years would fit well within the range established by the major technology push programs of the past sixty years.

The case study of wind power in California (Ch. 3) was used to assess the effectiveness of demand-side technology policies in inducing innovation at various stages of the innovation process. In this case, demand-pull policies effectively stimulated both diffusion of the technology and important post-adoption improvements. However, patenting activity appears to have declined precipitously just as policy-led demand for wind power increased. Explanations for this apparent lack of responsiveness to demand-pull focus on policy uncertainty, lags in investment payoffs, and trade-offs associated with the convergence on a dominant design.

In the case of photovoltaics (Ch. 4), empirical data were assembled to populate a simple engineering-based model identifying the most important factors affecting the cost of PV over the past three decades. The results indicate that learning from experience only weakly explains change in the most important cost-reducing factors—plant size, module efficiency, and the cost of silicon. They also suggest that the characterization of technological change in integrated assessment models would benefit from the inclusion of a broader set of explanatory variables, beyond those captured in experience curves.

A key aspect we need to consider for any large change to the energy system is that widespread diffusion of apparently-beneficial technologies can have unintended social consequences. The fourth study (Ch. 5) addressed the question: might terawatt-scale installation of photovoltaics lower the earth's reflectivity and contribute to warming of the atmosphere? There is a small warming impact, but the substitution of PV for fossil fuels dominates this albedo effect. The radiative forcing avoided by substituting PV for fossil fuels is a factor of 50 larger than the radiative forcing caused by PV's effect on the earth's albedo.

6.1.2 Case-specific policy implications of empirical findings

The results of the four studies have implications for policy in each of the specific cases. The most basic implication of the work on energy R&D is that policy makers who are concerned with future energy technologies should be aware that energy R&D investment starts from a low baseline; investment in energy R&D is low relative to that in other technology fields, low relative to the levels of the past, and low relative to rough estimates of what would be needed to address energy related risks in the future. Energy technology policy needs to be made with this basic fact in mind. Policies that address this lack of investment should be made with the understanding that the reasons for this under-investment have to do with weak appropriability conditions and lack of social concern about the risks associated with low levels of investment (Runci *et al.*, 2006). Comparison of the estimated levels needed with the major programs of the past provides a set of precedents for rapid scale up to levels more commensurate with the societal problems at stake. And recent coverage of this issue—including this data—in the popular press (Revkin, 2006) and in recent ballot measures (McPherson, 2006) may indicate that the low level of social concern is changing. Still, energy policy making in general must acknowledge that technology development builds on a legacy of over a decade of low levels of investment.

The case of wind power provides reason for encouragement. A set of strong demand-pull policies stimulated billions of dollars in private investment. California became the overwhelmingly dominant wind power market for more than a decade. The technology became cheaper and more reliable. The gains from learning-by-doing appear to have been substantial. Yet this case also provides an important reason for caution. Policy-led demand appears to have focused technology development on a single technological trajectory and helped establish a dominant design that has persisted for decades. While the incremental changes within this dominant design have been impressive, the rather modest contribution to total energy supply after three decades makes it unclear that these improvements have been sufficient to justify the billions of dollars of public funds invested. If ultimately, non-incremental improvements are necessary for widespread deployment of wind power, then the precipitous decline in inventive activity along alternative technological trajectories may prove costly. This raises questions about how long public support should be used to support variety, and whether the need for non-incremental change can be identified ex ante.

The rather weak role that learning-by-doing plays in the quantification of the sources of cost reductions in PV implies that there may be avenues for enhancing technology policies by relying less on ‘riding down the learning curve’ and more on creating incentives for firms to make investments in the types of cost-reducing activities quantified in this study. Simply subsidizing demand in order to attain cost reductions is a blunt tool given the severe omitted variable bias that exists in learning curves. Policy makers need to understand that R&D has played a critical role in achieving key technical improvements, such as electrical efficiency improvements, and that it may need to play such a role in the future as well. Given the low levels of R&D investment noted above, government can play a role. Similarly, inter-sectoral spillovers have also helped drive down costs in the past and may also in the future, suggesting that efforts to support collaborative research across industries may bear fruit. Finally, acknowledging that a large share of the cost reductions arose from large and risky investments, which took years to payoff, implies that expectations about future demand, and thus about future policy, are critical to future cost reductions.

The primary policy implication of the work on the albedo effect of PV is that PV is an effective climate mitigation option, even when the adverse impact of its effect on albedo is taken in to account. This result is insensitive to uncertainty in the underlying data, so policy makers can be assured that opposition to PV due to its impact on the earth’s reflectivity is not based on valid concerns. A secondary policy implication is that the effectiveness of PV as a climate change mitigation option varies widely based on the specifics of installations, technology characteristics, and what mix of energy sources it displaces. While reducing the cost of PV is an important social goal, it does not fully capture the social value of PV for climate change. Policy makers will need to pay closer attention to how PV is deployed, not just its cost. Ultimately, evaluating technologies based on their "net climate impact" will provide a more comprehensive measure of their social value.[1]

6.2 Dampened Incentives

When the results of these cases are considered together, one issue that emerges prominently is the dampening of incentives to innovate that arises from persistently uncertain expectations about future policy.

6.2.1 Purposive and risky investments drove innovation

The improvements in technologies observed in this project required purposive—and risky—investments by a variety of actors. As one entrepreneur in the solar industry has put it, "These improvements didn't just happen—they took hard work."

A look at the last three decades shows that, for many technologies, cost reductions and

---

[1] The notion of the 'marginal cost of carbon abatement' is useful measure for evaluating technology options to address climate change precisely because it takes into account both the cost of a non-emitting technology and the amount of emissions it displaces (Pearce, 1991; Cline, 1992; Fischer and Morgenstern, 2003).

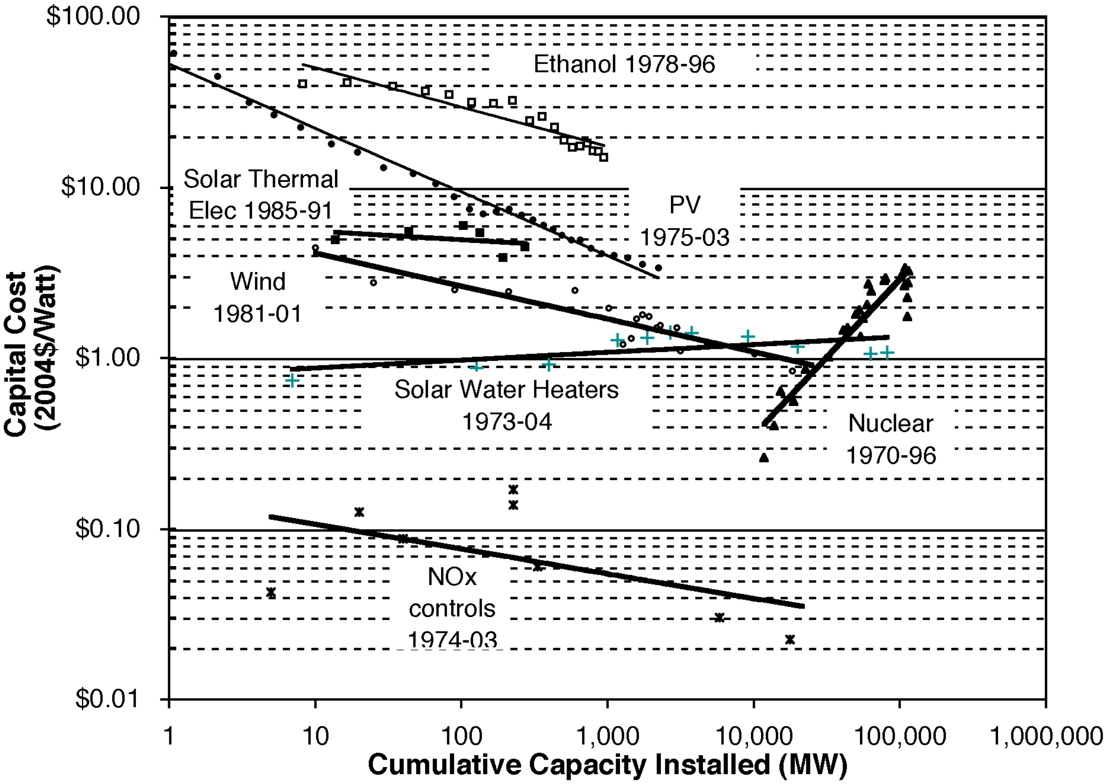

Figure 6.1: Experience curves for energy technologies. Data: Kahn (1991); Goldemberg *et al.* (2004); Taylor and Nemet (2006).

performance improvements have been substantial. Cost reductions can be observed in an array of case studies; Fig. 6.1 shows that the costs per watt of generation capacity have declined substantially for photovoltaics, wind power, solar thermal electric systems, NO_x controls, and ethanol production.[2] Performance improvements have also been observed for pre-commercial technologies, for example, nuclear fusion reactors (Fig. 6.2).

Having looked at the government's technology-push and demand-pull interventions, as

---

[2] Note that the data for ethanol is in units of dollars per gallon, rather than dollars per Watt. For insight into why the cost of nuclear power increased, see Hultman *et al.* (2007).

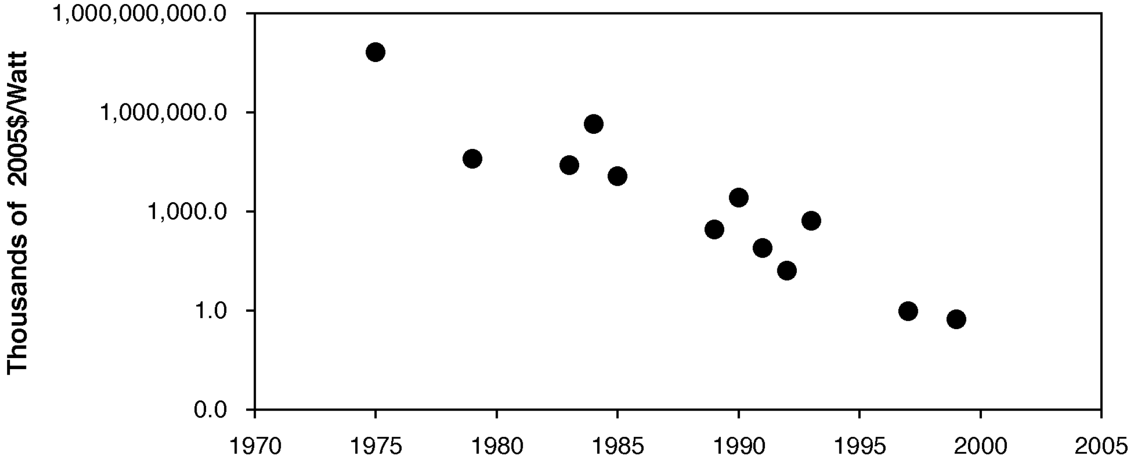

Figure 6.2: Cumulative R&D spending per watt of fusion power output (thousands of 2005$ per watt). Power output from experiments with Tokamak fusion reactors compared to fusion R&D investment. Data: McLean (2002); Nemet and Kammen (2007).

well as the decomposition of the drivers of change in the innovation process itself, it is difficult to make the case that innovation occurred as a fortuitously autonomous process that was simply the result of the passage time.[3] Rather, the technologies analyzed here improved as a result of purposive investments by a variety of actors. PV producers reduced costs by taking advantage of economies of scale; they built large manufacturing facilities that enabled them to spread fixed costs over a large number of units and justify the introduction of automation, including the development of specialized machinery (Neuhoff *et al.*, 2007). The capacity factor of wind power in California improved as wind farm developers invested billions to purchase, install, and maintain their turbines and other equipment.

---

[3] For an elaboration of that argument, see the discussion of characterizing technical change using the concept of autonomous energy efficiency improvement (AEEI), as surveyed in Grubb *et al.* (2002).

The turbines in particular became cheaper as their manufacturers experimented with new designs that they later introduced to the wind farm developers as new products. Government distribution of R&D funding to national laboratories, universities, and commercial organizations has been central to both "use-inspired basic research" (Stokes, 1997) and the development and demonstration of new energy technologies. For example, publicly funded R&D activities have been central for the development of record-breaking photovoltaic cells (see Section 4.4.2), as well as improvements in nuclear power (Cohen and Noll, 1991), solar thermal electric systems (Taylor and Nemet, 2006), and longer-term bets such as nuclear fusion (Fig. 6.2).[4] These investments, both public and private, produced outcomes that led to cheaper and better technologies. It seems unlikely that the observed improvements would have occurred without such investments, and that instead the passage of time and the general accumulation of new knowledge would have produced them.

The success of each of these investments was uncertain; investors in innovation, whether taxpayers, financiers, venture capitalists, or entrepreneurs, took risks. In some cases, the risk was primarily technical; public programs to fund nuclear fusion research are made without knowing whether a full-scale nuclear fusion reaction will ever produce significantly more usable energy than is used to produce the reaction. In others, the risk has more to do with uncertainties about expected future demand. For example, a chemical company, Wacker A.G., has invested $200m in a plant to produce solar grade purified silicon. It

---

[4] This is not to say that each of these investments earned returns that eventually exceeded their costs or that they were appropriate uses of private and public funds.

will complete construction of the plant without knowing if it will be able to sell enough material to solar cell manufacturers to operate that plant at a capacity that will justify the construction costs. Reliability is uncertain as well; solar thermal concentrators appear to work well in laboratory and pilot settings, but have not been built at utility scale since the mid-1980s. In order to secure a long-term power purchase agreement from an electrical utility, a company like Stirling Energy Systems will have to build many megawatts of installations and operate them for several years to demonstrate that its technology is reliable. In many cases, these risks converge. In 1985, Sunpower decided to develop a very high efficiency photovoltaic technology. But it could not know if it would ever achieve a greater than 20% electrical conversion efficiency for a commercial module, nor whether that device would function reliably in the field for twenty years, nor whether demand would be large enough to justify the two decades of investment. Innovation depends heavily on the willingness of a variety of actors to manage technical and market risk so that they can make the investments necessary to improve technologies.

6.2.2 Lags from investments to payoffs can be long

These investments are risky not only due to market and technical uncertainty, but also because the payoffs to innovation sometimes occur well after the initial investments are made. The innovation literature has repeatedly observed that the flows of new knowledge in the innovation process "take time" (Rosenberg, 1994), and that "lags are long" (Fagerberg *et al.*, 2004) as new technologies must be adapted to engage with the larger technological system. In low-carbon energy technologies, the time that elapses between the investment in an innovative activity and when a firm receives the payoff from that investment varies widely—from less than a year to many years. Payoffs can occur quickly; the levelized costs of solar thermal electric systems dropped by a factor of two within two years of building the first utility scale plant in the mid-1980s (Lotker, 1991). In other cases, the time to payoff takes longer. A photovoltaic manufacturing facility, which takes two to three years to make operational, may take ten to fifteen years to pay itself off. It has taken five to ten years for laboratory breakthroughs—such as buried contacts and selectively-reflective surfaces—to become fully integrated into commercially available PV products (Green, 2005). In the case of wind power, gains from learning-by-doing occurred within a year or two, whereas the turbines themselves took several years to pay back, if they did at all. The very large ($>$1MW) wind turbines that were experimented with in the 1970s, after it was correctly predicted that cost competitiveness would require megawatt scale, were only commercially available twenty years later, in the late 1990s. As the innovation literature suggests, lags are inherent in the innovation process. However, the long lifetimes of equipment in the energy sector (Knapp, 1999) imply that investment capital may need to be even more patient in this sector for innovative potential to be realized. Lags that lengthen the time-to-payoff exacerbate the risks associated with uncertainty about future demand and technical viability (Dixit and Pindyck, 1994). The heterogeneity in investment-payoff lags means that uncertainty affects various technologies, firms, and parts of the innovation process differently.

6.2.3 Payoffs rely heavily on policy

For low-carbon energy technologies, the payoffs to investments rely heavily on policy. Except for the immediate-term and for critical situations (Borenstein, 2002), the demand for production from individual energy technologies is highly elastic. Given the range of generation costs observed for carbon-free energy technologies during the course of this study, except in small niches, the unsubsidized supply curve does not intersect the unsubsidized demand curve for these technologies.[5]

Many, but certainly not all, of the appealing characteristics of carbon-free energy technologies involve the avoidance of externalities, for example, producing energy without GhG emissions and other air pollutants. The value of these benefits are not included in prices and the government is ultimately responsible for assigning value to these socially desirable attributes. Expected future demand for such a technology will vary based on the extent to which the government intervenes, either through increasing the cost of competing technologies—e.g. through a carbon tax—or reducing the cost of carbon-free technologies directly—e.g. through a buy-down program. In either type of government program, expectations about future demand will depend heavily on expectations about the government

---

[5] Although a central feature of the history of these technologies is that the supply curve shifts downward over time due to technological change (see Fig. 6.1).

role. And because lags between investments and payoffs are long, expectations about the government role five, ten, or more years in the future will determine investments. In the case of especially long-lived technologies, such as coal power plants, expectations about the stringency of policy decades in the future affect investment decisions today (Sekar *et al.*, 2006; Barringer and Sorkin, 2007). The history of energy technology policy shows that the strength of incentives depend not just upon whether and when the government imposes a policy instrument, such as a carbon cap and trade system, but also upon crucial implementation details such as grandfathering, bankability, safety valves, and the timing of targets.

6.2.4 Policy volatility has persisted

This reliance on expectations about government actions years, and even decades, in the future is a problem because the history of energy policy in the U.S. has been dramatically volatile, both for demand-pull and technology push instruments.

On the demand side, policy instruments applied to energy supply technologies only rarely exist for more than a couple of years. In the case of wind power, a wide array of incentives were put in place over thirty years (Fig. 3.1) and yet the average duration of these incentive programs was only 3.1 years. Investment tax credits expired and had to be renewed. The production tax credit was repeatedly reauthorized every two years; the reauthorization depended on last minute congressional deliberations, leading to surges of investment activity at the beginning and end of each period.

As for technology-push, overall energy R&D funding levels increased by a factor of three within five years and then were cut in half over the next five years. New programs have ramped up quickly. Congressional appropriations debates and the introduction of 'earmarks' have led to the sudden cancellation of existing programs (Sissine, 2006). For example, the U.S. solar thermal electric power program was recently cut so that it now operates on a budget of less than \$2 million per year. In fact, funding levels have been volatile across a wide array of energy R&D programs. The histogram in Fig. 6.3 shows the annual change in funding levels for all 70 energy R&D programs in the past thirty years. The high frequencies at the left end of the distribution represent programs that were cancelled while those to the right represent the launch of new programs.[6] This figure shows that, even excluding the new and cancelled programs, in 72% of cases, annual program budgets either grew or were cut by more than 5%. And more than half the time, annual program budgets rose or fell by more than 10%.[7] The history of U.S. energy R&D funding is not characterized by stable budgets, but by changes that are much larger than annual changes in economic activity and overall research spending.

---

[6] The column at extreme right represents programs that increased by more than 100%.

[7] These amounts are based on changes in constant dollar terms so the effect of the general economy-wide change in price levels is removed.

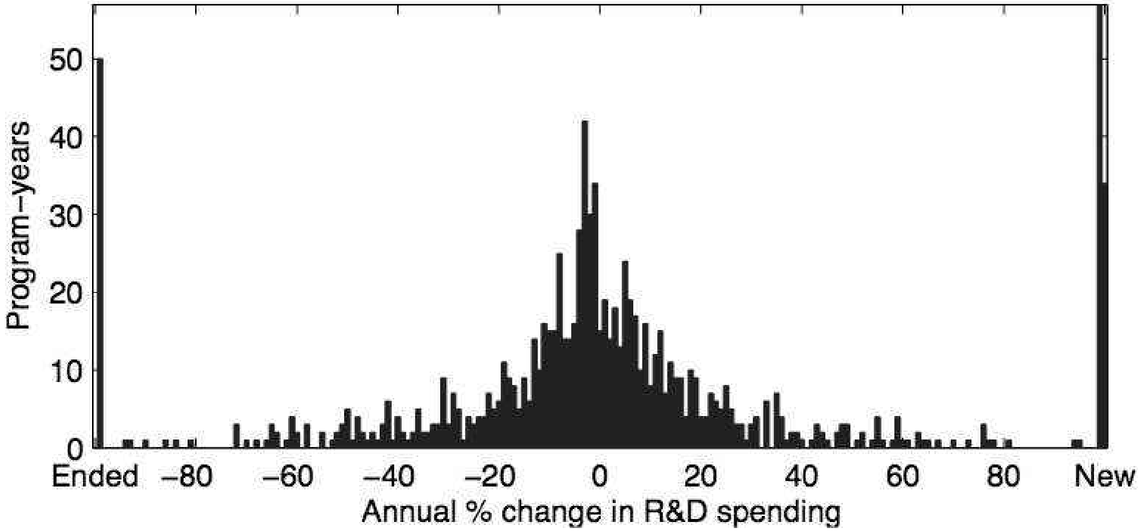

Figure 6.3: Annual percentage change in funding for 70 federally-sponsored energy R&D programs, 1978-2008. Changes are based on constant dollars. Data: Gallagher *et al.* (2007)

6.2.5 Policy uncertainty dampens incentives for innovators

Uncertainty in expectations about future policies increases the risk in investing in innovation for clean energy technologies. As observed in several cases in this dissertation, the lags between investments in innovation and the payoffs for private actors can last several years. Because externalities are pervasive in the clean energy sector, these distant payoffs rely heavily on the status of future government policies. But if expectations about the level—or existence—of these policy instruments several years in the future are uncertain, then firms will discount the value of these future policies and under-invest in innovation. Because technology development is in itself a risky endeavor, private sector energy companies will only respond to policies that are "Credible, long-lasting, and have a reasonable degree of stability" (Mowery, 2006). Anecdotally, multiple venture capital investors in California have made the claim that they typically risk adjust the value of future policy to zero, because as one of them put it:

> "We want to invest in companies that will be profitable regardless of policy. . . what the government giveth, it can taketh away."

For this investor, policy uncertainty apparently eliminated the policy-driven incentives for investing in new technology. If this view reflects a general sentiment, it is particularly concerning since venture capital is normally considered to be the most important source of private sector financing for early stage investments in new technologies (Barry *et al.*, 1990; Gompers and Lerner, 2001). If these investors are unwilling to take risks on new technologies that depend on future policies, then who will? Will low-carbon-related innovation only be pursued to the extent to that it holds promise for becoming better and cheaper than existing carbon-intensive technologies in absolute terms, that is without consideration of its ability to remediate environmental externalities and of the policies that might internalize them?

On their surface, recent policies in California appear to have avoided the adverse affects of volatility by setting a series of longer term goals. For example, the California Renewable Portfolio Standard, passed in 2002, set a target fifteen years in the future (Sher, 2002).[8] In

---

[8] The California Energy Commission and California Public Utilities Commission increased the stringency of these targets in 2006 so that the standard currently mandates that 20% of retail electricity sales must be obtained from renewable fuels by 2010, and 33% by 2020, with required annual increases of 2% from 2003 onward.

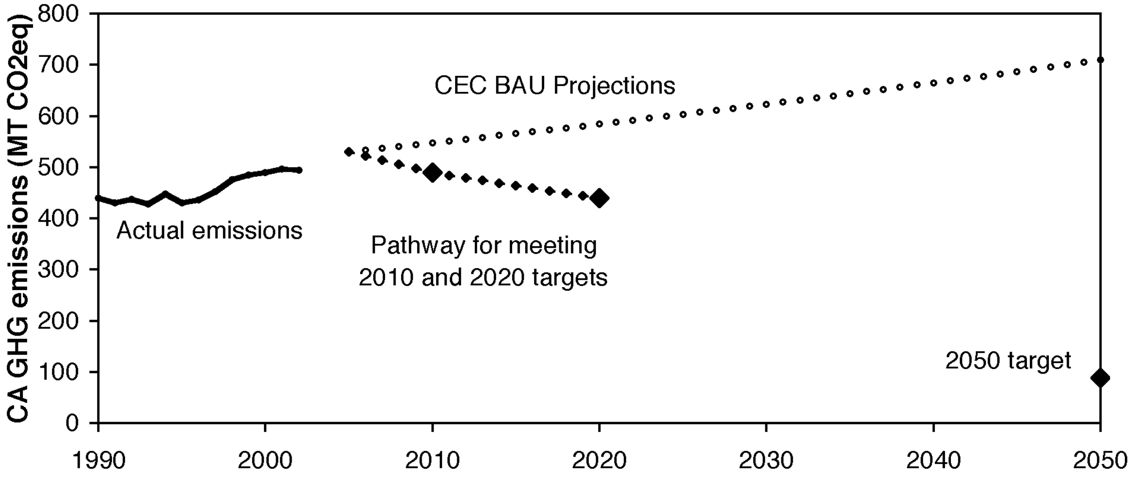

Figure 6.4: Greenhouse gas emissions in California: Actual, business-as-usual projections, and targets under AB32. Data: Bemis and Allen (2005).

2006, the Governor signed SB-1, the "Million Solar Homes Bill", which set a target for three gigawatts of solar generating capacity in the state by 2017 (Murray, 2002).[9] Similarly, the state's Global Warming Solutions Act (Assembly Bill 32), which the legislature passed in 2006, sets greenhouse-gas reduction targets for 2020 (Nunez, 2006), and the Governor's Executive Order includes a target for 2050 (Schwarzenegger, 2005) (Fig. 6.4).[10]

The adoption of long term targets into law is encouraging. But they do not eliminate cause for concern. First, the strength of the incentives created by such policies depend not only on the targets themselves but also on the crucial details concerning implementation. For example, investor owned utilities (IOUs) in California have so far fallen short of

---

[9] Part of this goal will be met through the California Solar Initiative, a $2.9 billion set of performance-based incentives for installation of new solar systems from 2007 to 2017 (Peevey and Malcolm, 2006).

[10] Assembly Bill 32 mandates that the state implement programs such that emissions are reduced to 1990 levels by 2020, and the Executive Order announced a target of 80% below 1990 levels by 2050.

their renewables obligations in 2003, 2004, and 2005 under the state's Renewable Portfolio Standard (RPS) (CPUC, 2007). Moreover, meeting the 2010 target of 20% renewables is not assured. In 2005, renewables as a portion of retail sales were 12% for Pacific Gas and Electric (PG&E), 18% for Southern California Edison (SCE), and 5% for San Diego Gas and Electric (SDG&E). These IOUs are unlikely to meet the 2010 target of 20% renewables given the sum of all existing procurement contracts, even if all pending contracts are signed, all currently short-listed bids are accepted, and all expiring contracts are re-signed (Fig. 6.5)(CPUC, 2007).[11] Assuming all these "probable" contracts end up providing power in 2010, these utilities together will fall short of the target by 2.5 terawatt-hours, or about 1.2 gigawatts of renewables capacity assuming they operate at 25% capacity factor.

One might expect that this apparently aggressive target would prompt the development and adoption of new technologies which, while possibly unproven or expensive, would enable the IOUs to meet the target. But the details of the RPS regulations contain another option. Utilities can simply pay a penalty:

> "The CPUC has set a penalty of 5 cents per kilowatt hour in the event that an investor owned utility does not meet its obligation, with an overall penalty cap of $25 million per utility annually" (CPUC, 2003).[12]

---

[11] That it takes "2-5 years for new projects to come on-line" suggests that developing completely new projects in time will be difficult. "Probable" bids in Fig. 6.5 includes all pending contracts, short-list bids, and expiring contracts.

[12] Note also that load serving entities 'Are allowed to carry, for up to three years, procurement deficits greater than 25% of that year's incremental procurement target without penalty if they have demonstrated to the CPUC an allowable reason for noncompliance, four of which are: (1) Insufficient response to the RPS solicitation, (2) Contracts already executed will provide future deliveries sufficient to satisfy current

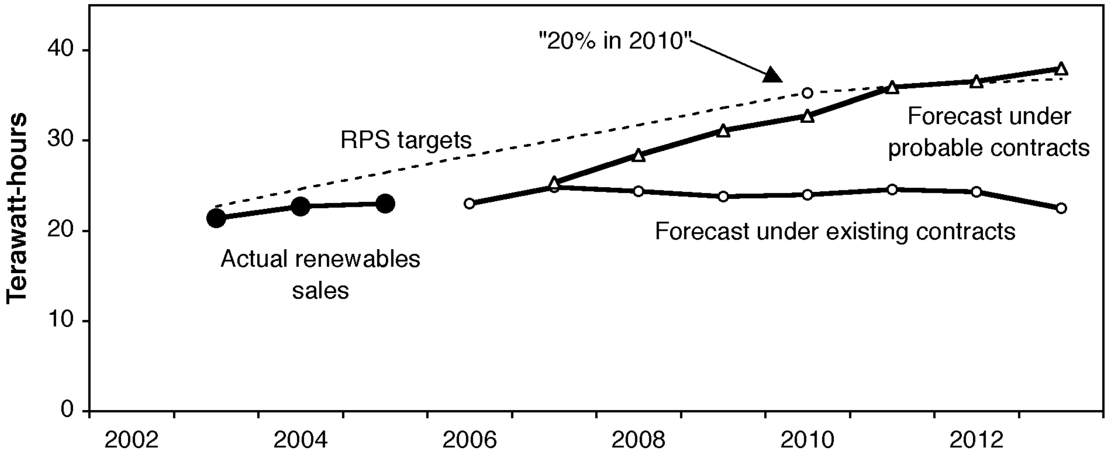

Figure 6.5: Retail sales of renewable power among California investor owned utilities: RPS targets, actual sales, and forecast power under existing and future procurement contracts. Data: CPUC (2007).

As the best sites for renewables are used up, i.e. the windiest and sunniest sites with access to transmission capacity, the stock of inexpensive projects is dwindling and marginal cost of renewables capacity is rising. Improvements that reduce the cost of renewables can address these rising marginal costs, but the penalty should eliminate consideration of any new technology that will cost more than 5 cents over non-renewable sources.[13] Moreover, $25 million per year is a small price to pay relative to the cost of developing, building and operating renewables technologies.[14] The RPS has spurred the IOUs to procure a large amount

---

year deficits, (3) Inadequate public goods funds to cover above-market renewable contract costs, (4) Seller non-performance" (CPUC, 2003).

[13] One alternative possibility is that if IOUs can transfer the investment in expensive renewables to their rate base, then they can avoid both the cost of the expensive technology and the penalties. However, they may encounter limits to how much cost rate-payers may be willing to bear.

[14] This amount is especially small considering that the annual revenues of PG&E, SCE, and SDG&E in 2006 were $13 billion, $10 billion. and $12 billion respectively (PG&E, 2007; SCE, 2007; SDG&E, 2007).

of renewable electricity CPUC (2007). But the modest penalties for non-compliance limit the incentives to technologies that are already close to being competitive.[15]

This dampening of the incentives created by out-clauses in future targets exists also for economy-wide programs addressing greenhouse-gas emissions, such as a proposed cap and trade program or carbon tax. In this case, some have suggested that the adoption of “safety valves”—limits on how high the price of carbon can go—would safeguard the macro-economy from excessively onerous costs and would make more stringent climate policy palatable to affected parties (Jacoby and Ellerman, 2004). Canada has already implemented such a program as part of its decision to ratify the Kyoto Protocol; in that case, a cap of $15/ton of carbon was offered as a concession to carbon-intensive business interests (Schmidt, 2002). Recent draft greenhouse-gas legislation in the Senate Committee on Energy and Natural Resources proposes a safety valve by limiting the price of carbon emissions credits to $7/ton in 2012 (Bingaman and Specter, 2007). With its proposed 5%/year increases thereafter, the cap in 2050 could reach approximately $15/year in year 2007 dollars, assuming 3%/year inflation. Again, this feature of the implementation of a target would reduce the incentives for investing in new technologies that might enable very large reductions in emissions, but whose costs are expected to be above the price cap. For example, recent estimates of the cost of carbon capture and sequestration (CCS) from coal power plants are in the range of $150–200/ton (Anderson and Newell, 2004). A price cap

---

[15] This tradeoff between adopting a new technology and paying a penalty is developed formally by Kemp (1998) in an analysis of the effect of taxes on the decisions by firms to adopt water treatment technology.

on carbon that is an order of magnitude lower may severely restrict the incentive for firms to build new coal plants, e.g. with gasification, which are amenable to sequestration later, never mind investing in improving the CCS technology directly. The desirability of the cap is debatable, but one should not expect a regime of stringent, but *avoidable* targets to stimulate investment in technologies that only pay off at high carbon prices. While a price cap on carbon limits the impact of carbon regulation on the economy, relying on it as the primary source of incentives also limits our ability to reduce emissions later if the impacts become more severe than expected, unless additional and complementary technology development policies are implemented.

Second, even if the implementation details are strict, the longevity of the targets themselves may not be credible (Montgomery and Smith, 2005). In previous cases, governments have backed down from ambitious long term targets once influential stake-holders protested that they faced an unfair burden in meeting them. For example, after passing a mandate in 1990 that 10% of new vehicles sold in 2003 must have no emissions, the California Air Resources Board abandoned the Zero Emissions Vehicles (ZEV) Mandate once automobile manufacturers sued the state (Dixon *et al.*, 2002; Shaheen *et al.*, 2002). Subsequently, the ZEV mandate shifted to a much less stringent set of incentives. This case may be especially pernicious, in terms of creating incentives for innovation, because it suggests that policies can change not only due to the vagaries of political priorities, but also because potential innovators decide that lobbying and litigating to soften government imposed targets may be a more effective use of their resources than investing in innovation to meet them.

The shift in policy-making towards longer term targets is encouraging for avoiding the adverse effects on innovation of policy uncertainty. Still, setting long term targets by itself is insufficient. Implementation details and credibility in the longevity of the targets themselves are crucial for providing signals to investors in innovation for technologies that are not currently close to being competitive, but may be needed as options to meet long term targets.

6.3 Trading off Flexibility and Commitment

So what should be the policy response, if any, to historically-reinforced perceptions of policy volatility. One could argue that policy makers should endeavor to increase their commitment to future policies (Blyth and Yang, 2006); they should design programs to last for several years; they should resist changing existing programs; they should communicate clear expectations to firms so that firms reduce their risk that policies might change; and they should tighten up non-compliance penalties.

But are such efforts to make ironclad, long-term commitments feasible? The history of policy making in the energy sector is characterized by iteration, initiative, and learning (Ruttan, 2001). Often, policy makers experiment with new policy instruments and early failures are abandoned. For various reasons, energy policy in the U.S. is best characterized by bricolage of diverse initiatives, rather than a strategically designed set of programs

(Laird, 2001). Further, administrations last only a few years; majorities shift. It may be risky to shift to a policy design regime whose effectiveness relies on long-term policy stability.[16]

And even if it is politically feasible to make long-term commitments credible, would it be desirable? Knowledge about the climate problem is dynamic; there is social learning and our understanding of the problem is continually improving, even if our certainty about it is not (Bierbaum *et al.*, 2007). Specifically, scientific assessments of climate sensitivity and of the impacts of a warmer atmosphere are evolving—compare Houghton *et al.* (2001) and Alley *et al.* (2007). Also, the costs of polices depend heavily on future developments in the pace and direction of technological change, population, and economic growth, all of which are highly uncertain (Nakicenovic *et al.*, 2000). The magnitude of the uncertainties involved in the climate problem and the potential that social learning may change them over time, implies that long-term policies will benefit from flexibility (Kelly and Kolstad, 1999). For example, we might discover that low-carbon innovation is occurring more rapidly than anticipated so that the costs of mitigating climate change are likely to be lower than anticipated, suggesting that tightening emissions targets would be desirable. Conversely, we might learn that the expected damages due to climate change are unlikely to be as severe as anticipated. In that case, shifting to less stringent targets would be desirable. Or we

---

[16] While beyond the scope of this discussion, the weakness of international political institutions makes setting credible long-term targets—with enforceable penalties for non-compliance—even more challenging in a *global* context. Application of targets at the international level raises further concerns about the wisdom of relying on them (Victor *et al.*, 2005).

may find that feedback effects, many of which are not currently well characterized by integrated assessment models, are likely to be larger than we currently expect (Torn and Harte, 2006), so that more stringent emissions limits will be needed to avoid the possibility of unacceptably high damages. Further, as the exercise in Chapter 5 points out, we may need to monitor the development of technological solutions too, as we learn more about their overall impact on the climate problem. Less volatile policy may have desirable effects, but maximizing the stability and credibility of long-term targets is not an obvious solution because the costs of tying the hands of future policy makers may outweigh the benefits of reducing policy risk.

Managing this tradeoff between the benefits of commitment and of flexibility will be crucial to the success of climate policy, particularly with respect to innovation. One response to this tradeoff is to raise the barriers to amending long-term climate policy so that it becomes difficult, but not impossible, to change. Such a regime might resemble the U.S. Constitution, which was intentionally designed to be mutable, but also to require broad agreement to adjust. Targets could be revisited on an infrequent basis, e.g. every five years, and updated to incorporate new information about the possible damages and the costs of preventing, or coping with, these impacts. Such a design might deliver the ‘reasonable degree of stability’ that private actors require, while allowing for the incorporation of insights from social learning about both expected future damages and the costs of avoiding them.

The effectiveness of such a scheme would depend on policy makers’ abilities to make changes to long term targets based on the acquisition of new information about the problem, while resisting making changes due to the emergence of new competing political demands. It seems plausible that raising the barriers to changing long-term targets, once they have been set, would discourage changes that are based on short-sighted, perhaps politically expedient reasons.

Another response is to assume that, to a substantial extent, policy uncertainty is inevitable. Even if technical and scientific uncertainty can be addressed by revisiting targets on a regular basis, there are still limits to the discretion that politicians can exercise over future policy, and future politicians. It may be impossible for political leaders to commit their successors to policies decades in the future. So if continuation and credibility over many years is impossible, then we may need to take some degree of policy volatility into account, rather than optimistically ignore it. If we do, we are likely to find that incentives that rely on the details of policy instruments many years in the future are dampened to a much larger extent than are incentives linked to nearer-term policies. From that perspective, one possibility is that long-term emission targets and pricing of carbon emissions may benefit from being complemented with policy instruments whose incentives are robust to uncertainty about future policy. For example, in the case of tax credits for R&D, direct sponsorship of R&D spending, and support for cost-shared demonstration projects, the financial transfer from government to innovator occurs much closer to the innovator's decision to commit resources to a project; it probably occurs within a year. Firms eligible for the credit do not need to discount the probability of actually receiving the incentive due to policy uncertainty.[17]

A second possibility would be to set intermediate technical or market targets for new technologies; providing prizes for reaching theses targets may complement carbon prices and provide incentives for technologies with distant payoffs. For example, the government might act as a sponsor and set a group of technical thresholds; firms reaching these thresholds would be eligible to receive a prize. Rather than having to demonstrate commercial viability, the firm would only need to demonstrate proof of concept in a laboratory setting.[18] With the long development and diffusion times for energy technologies, rewarding intermediate steps may provide a means to reduce the time to payoff and thus reduce policy uncertainty. The general aim in designing such schemes would be to reduce policy risk for innovators, not completely, but to a more manageable level than currently specified intermediate-term emissions targets provide.

6.4 Demand for Institutional Innovation

Climate change will require transforming the world's energy system over the next several decades. Technical improvements to deliver affordable and reliable low-carbon energy

---

[17] Like any industry, these firms still face market and technical risk in developing their product.

[18] In this scheme, the government acts as the selection mechanism, rather than the market. While this practice is employed in military procurement, it would require increased technical capacity by the government administrators in the energy area. For more on the advantages and disadvantages of creating rewards for intermediate steps and final products see Scotchmer (2005).

production and end-use technologies will be central to this project. The history of technological innovation provides ample evidence of the resourcefulness of human ingenuity and of the potential for profound change to large systems on a multi-decadal time scale. And while still nascent relative to other fields, the literature on the economics of innovation has begun to provide a shared understanding of how the innovation process functions and how its effects diffuse through society. However, the empirical basis for understanding how *governments* can guide the rate and direction of the innovation process over several decades is much weaker; relevant precedents to analyze are scarce and their pertinence to the climate problem is limited. Moreover, this concluding chapter has discussed how existing and proposed incentive mechanisms suffer from a variety of flaws that make them potentially inadequate for motivating the magnitude of investments in innovation required. Generating insights—and ultimately devising a shared framework—for understanding how the government can provide adequate incentives for climate-relevant innovation over many decades is clearly needed. More practically, while technical innovation is certainly central, the climate problem is creating an emerging demand for *institutional* innovation. A viable model for providing incentives for innovation in low-carbon energy technologies over a multi-decade time scale has not yet emerged. Experimentation, monitoring, incorporation of social learning, and capacity building within governments are important aspects of designing policy that can provide the incentives for innovation that are required to address the climate innovation policy problem.

Bibliography

AAAS. Analysis of the outyear projections for R&D in the FY 2005 budget. In *forthcoming in AAAS Report XXIX: Research and Development FY 2005*. AAAS, Washington, DC, 2004.

AAAS. R&D funding in the FY2007 DOE budget. Report, American Association for the Advancement of Science, February 2006.

W. J. Abernathy. *The Productivity Dilemma*. The Johns Hopkins Press, Baltimore, MD, 1978.

Paul S. Adler and Kim B. Clark. Behind the learning curve: A sketch of the learning process. *Management Science*, 37(3):267–281, 1991.

Armen Alchian. Reliability of progress curves in airframe production. *Econometrica*, 31(4):679–693, 1963.

John A. Alic, David C. Mowery, and Edward S. Rubin. U.S. technology and innovation policies: Lessons for climate change. Report, Pew Center on Global Climate Change, 2003.

Richard Alley, Terje Berntsen, Nathaniel L. Bindoff, and Others. Climate change 2007, the physical science basis: Contribution of working group I to the fourth assessment report. Summary for policymakers, Intergovernmental Panel on Climate Change, February 2007.

E. A. Alsema and E. Nieuwlaar. Energy viability of photovoltaic systems. *Energy Policy*, 28(14):999–1010, 2000.

Paul Ames, Jan Sliva, and Aoife White. The New York Times: Europe's leaders agree to cut greenhouse gases, 9 March 2007.

Soren Anderson and Richard Newell. Prospects for carbon capture and storage technologies. *Annual Review of Environment and Resources*, 29(1):109–142, 2004.

B.A. Andersson and C. Azar. Material constraints for thin-film solar cells. *Energy*, 23(5):407–411, 1998.

Phil Angelides. State treasurer Phil Angelides launches Green Wave environmental investment initiative. Technical report, Office of the State Treasurer of California, February 3 2004.

Apollo-Alliance. The Apollo jobs report: Good jobs and energy independence, new energy for America. Report, The Apollo Alliance, January 2004.

Linda Argote and Dennis Epple. Learning curves in manufacturing. *Science*, 247(4945):920–924, 1990.

Kenneth Arrow. The economic implications of learning by doing. *The Review of Economic Studies*, 29(3):155–173, 1962.

Kenneth Arrow. Welfare and the allocation of resources for invention. In *The rate and direction of inventive activity: economic and social factors*. Princeton University Press, 1962.

W. Brian Arthur. Competing technologies, increasing returns, and lock-in by historical events. *The Economic Journal*, 99:116–131, 1989.

W. Brian Arthur. Out-of-equilibrium economics and agent-based modeling. In K. Judd and L. Tesfatsion, editors, *Handbook of Computational Economics, Vol. 2: Agent-Based Computational Economics*. Elsevier/North-Holland, 2006.

K. Astrand and L. Neij. An assessment of governmental wind power programmes in sweden–using a systems approach. *Energy Policy*, 34(3):277–296, 2006.

Jesse H. Ausubel. Some ways to lessen worries about climate change. *The Electricity Journal*, 14(1):24–33, 2001.

AWEA. The economics of wind energy. Fact sheet, American Wind Energy Association, February 2004.

AWEA. Windpower outlook 2005. Technical report, American Wind Energy Association, 2005.

AWEA. Windpower outlook 2006. Technical report, American Wind Energy Association, 2006.

Christian Azar and Hadi Dowlatabadi. A review of technical change in assessment of climate policy. *Annual Review of Energy and the Environment*, 24(1):513–544, 1999.

B. H. Bahk and M. Gort. Decomposing learning by doing in new plants. *Journal of Political Economy*, 101(4):561–583, 1993.

J. L. Balenzategui and F. Chenlo. Measurement and analysis of angular response of bare and encapsulated silicon solar cells. *Solar Energy Materials and Solar Cells*, 86(1):53–83, 2005.

N. Baloff. Learning curve - some controversial issues. *Journal of Industrial Economics*, 14(3):275–282, 1966.

Felicity Barringer and Andrew Ross Sorkin. In big buyout, utility to limit new coal plants, 25 February 2007.

C. B. Barry, C. J. Muscarella, J. W. Peavy, and M. R. Vetsuypens. The role of venture capital in the creation of public companies - evidence from the going-public process. *Journal of Financial Economics*, 27(2):447–471, 1990.

BCG. *Perspectives on Experience*. The Boston Consulting Group, Boston, 1972.

BEA. Gross domestic product (GDP) by industry. Technical report, Bureau of Economic Analysis, December 2004.

Gerry Bemis and Jennifer Allen. Inventory of california greenhouse gas emissions and sinks: 1990 to 2002 update. Staff paper, California Energy Commission, June 2005.

M. J. Benner and M. L. Tushman. Exploitation, exploration, and process management: The productivity dilemma revisited. *Academy of Management Review*, 28(2):238–256, 2003.

J. I. Bernstein and M. I. Nadiri. Interindustry R&D spillovers, rates of return, and production in high-tech industries. *American Economic Review*, 78(2):429–434, 1988.

Jeffrey Bernstein and M. Ishaq Nadiri. Product demand, cost of production, spillovers, and the social rate of return to R&D. Working Paper 3625, National Bureau of Economic Research, 1991.

Mark A. Bernstein, Jay Griffin, and Robert Lempert. Impacts on U.S. energy expenditures of increasing renewable energy use. Technical Report TR-384, The RAND Corporation, 2006.

J. Bessen. Patents and the diffusion of technical information. *Economics Letters*, 86(1):121–128, 2005.

R. H. Bezdek and R. M. Wendling. The U.S. energy subsidy scorecard. *Issues in Science and Technology*, 22(3):83–85, 2006.

Rosina Bierbaum, Ulisses Confalonieri, Jacques Dubois, Alexander Ginzburg, Peter H. Gleick, and Others. Confronting climate change: Avoiding the unmanageable and managing the unavoidable. The report of the scientific expert group, Scientific Expert Group on Climate Change and Sustainable Development, February 2007.

Jeff Bingaman and Arlen Specter. Global warming documents: Bingaman-Specter discussion draft, 22 January 2007.

Fatih Birol. *World Energy Investment Outlook*. International Energy Agency, Paris, 2003.

William Blyth and Ming Yang. Impact of climate change policy uncertainty in power investment. Working Paper LTO/2006/02, International Energy Agency, October 2006.

Severin Borenstein. The trouble with electricity markets: Understanding California's restructuring disaster. *Journal of Economic Perspectives*, 16(1):191–211, 2002.

Severin Borenstein. Valuing the time-varying electricity production of solar photovoltaic cells. Working Paper CSEM-WP-142, U.C. Energy Institute, Center for the Study of Energy Markets, March 2005.

T. M. Bruton and J. M. Woodock. Multi-megawatt upscaling of silicon and thin film solar cell and module manufacturing (MUSIC FM). Technical Report CT94 0008, European Commission Project RENA, 1997.

T. M. Bruton. General trends about photovoltaics based on crystalline silicon. *Solar Energy Materials and Solar Cells*, 72(1-4):3–10, 2002.

Paolo Buonanno, Carlo Carraro, and Marzio Galeotti. Endogenous induced technical change and the costs of kyoto. *Resource and Energy Economics*, 25(1):11–34, 2003.

Dallas Burtraw, David A. Evans, Alan Krupnick, Karen Palmer, and Russell Toth. Economics of pollution trading for SO_2 and NO_x. *Annual Review of Environment and Resources*, 30(1):253–289, 2005.

Vannevar Bush. As we may think. *The Atlantic Monthly*, 1945.

Vannevar Bush. Science the endless frontier: A report to the President by Vannevar Bush, director of the office of scientific research and development. Technical report, United States Government Printing Office, July 1945.

John G. Carlson. Cubic learning curves: Precision tool for labor estimating. *Manufacturing Engineering and Management*, 71(5):22–25, 1973.

CEC. Policy report on AB 1890 renewables funding. Report P500-97-002, California Energy Commission, March 1997.

CEC. 1983-2005 California electricity generation. Report, California Energy Commission, 2006.

Census. *U.S. Census Bureau: Statistical Abstract of the United States*. US Government Printing Office, Washington, DC, 2005.

Richard Cheney. National energy policy. Report, National Energy Policy Development Group, Office of the Vice President, May 16 2001.

S. R. Chidamber and H. B. Kon. A research retrospective of innovation inception and success - the technology-push, demand-pull question. *International Journal of Technology Management*, 9(1):94–112, 1994.

Elmer Christensen. *Electricity from photovoltaic solar cells: Flat-Plate Solar Array Project of the U.S. Department of Energy's National Photovoltaics Program, 10 years of progress. JPL publication: 400-279*. Prepared by the Jet Propulsion Laboratory for the U.S. Dept. of Energy through an agreement with the National Aeronautics and Space Administration, Pasadena, CA, 1985.

William R. Cline. *The Economics of Global Warming*. The Institute for International Economics, Washington, D.C., 1992.

W. M. Cohen and D. A. Levinthal. Absorptive-capacity - a new perspective on learning and innovation. *Administrative Science Quarterly*, 35(1):128–152, 1990.

Linda R. Cohen and Roger G. Noll. *The Technology Pork Barrel*. Brookings, Washington, 1991.

Rita R. Colwell. *America's Investment in the Future*. National Science Foundation, Washington, DC, 2000.

Patrick Conley. Experience curves as a planning tool. *IEEE Spectrum*, 7(6):63–68, 1970.

David W. Conover. U.S. climate change technology program strategic plan. Draft for public comment, Executive Office of the President, September 2005.

Joseph Cortwright and Heike Meyer. Signs of life: The growth of biotechnology centers in the u.s. Report, Brookings, 2002.

Dennis Costello and Paul Rappaport. The technological and economic development of photovoltaics. *Annual Review of Energy*, 5(1):335–356, 1980.

CPUC. Decision 03-06-071, order initiating implementation of the Senate Bill 1078 Renewable Portfolio Standard Program, 2003.

CPUC. Progress of the California Renewable Portfolio Standard as required by the supplemental report of the 2006 Budget Act. Report to the legislature, California Public Utilities Commission, January 2007.

Kristina B. Dahlin and Dean M. Behrens. When is an invention really radical?: Defining and measuring technological radicalness. *Research Policy*, 34(5):717–737, 2005.

Kristina Dahlin, Margaret Taylor, and Mark Fichman. Today's Edisons or weekend hobbyists: Technical merit and success of inventions by independent inventors. *Research Policy*, 33:1167–1183, 2004.

Per Dannemand Andersen. Sources of experience: theoretical considerations and empirical observations from danish wind energy technology. *International Journal of Energy Technology and Policy*, 2(1/2):33–51, 2004.

Paul A. David, Bronwyn H. Hall, and Andrew A. Toole. Is public R&D a complement or substitute for private R&D? a review of the econometric evidence. *Research Policy*, 29(4-5):497–529, 2000.

Paul A. David. The landscape and the machine: Technical interrelatedness, land tenure, and the mechanization of the corn harvest in Victorian Britain. In D. McColskey, editor, *Essays on a Mature Economy: Britain after 1840*. Cambridge University Press, Cambridge, U.K., 1971.

Graham A. Davis and Brandon Owens. Optimizing the level of renewable electric R&D expenditures using real options analysis. *Energy Policy*, 31:1589–1608, 2003.

Avinash K. Dixit and Robert S. Pindyck. *Investment under uncertainty*. Princeton University Press, Princeton, NJ, 1994.

Lloyd Dixon, Isaac R. Porche, and Jonathan Kulick. Driving emissions to zero: Are the benefits of California's zero emission vehicle program worth the costs? Technical report, RAND, 2002.

DOJ. Horizontal merger guidelines. Guidelines document, U.S. Department of Justice and the Federal Trade Commission, April 8 1997.

J.J. Dooley. Unintended consequences: Energy R&D in a deregulated energy market. *Energy Policy*, 26(7):547–555, 1998.

G. Dosi, L. Marengo, and C. Pasquali. How much should society fuel the greed of innovators?: On the relations between appropriability, opportunities and rates of innovation. *Research Policy*, 35(8):1110–1121, 2006.

Giovanni Dosi. Technological paradigms and technological trajectories: A suggested interpretation of the determinants and directions of technical change. *Research Policy*, 11(3):147–162, 1982.

Giovanni Dosi. Sources, procedures, and microeconomic effects of innovation. *Journal of Economic Literature*, 26(3):1120–1171, 1988.

David Driessen. Does emissions trading encourage innovation? *Environmental Law Reporter*, 32, 2003.

R.D. Duke and D.M. Kammen. The economics of energy market transformation initiatives. *The Energy Journal*, 20(4):15–64, 1999.

John M. Dutton and Annie Thomas. Treating progress functions as a managerial opportunity. *Academy of Management Review*, 9(2):235–247, 1984.

Ottmar Edenhofer, Kai Lessmann, Claudia Kemfert, Michael Grubb, and Jonathan Köhler. Induced technological change—exploring its implications for the economics of atmospheric stabilization: Synthesis report from the innovation modeling comparison project. *The Energy Journal*, 27(Special Issue on Endogenous Technological Change and the Economics of Atmospheric Stabilisation):57–108, 2006.

Jae Edmonds and Steven J. Smith. The technology of two degrees. Working paper, Pacific Northwest National Laboratory, Joint Global Change Research Institute, at the University of Maryland, 2006.

James A. Edmonds, John Clarke, James Dooley, Son H. Kim, and Steven J. Smith. Modeling greenhouse gas energy technology responses to climate change. *Energy*, 29(9-10):1529–1536, 2004.

EIA. Annual energy review 2003: Table 8.10 average retail prices of electricity, 1960-2003. Report, Energy Information Administration, 2004.

EIA. International energy outlook 2005. Technical report, U.S. Dept. of Energy, Energy Information Administration, July 2005.

A. Denny Ellerman and David Harrison. Emissions trading in the U.S. experience, lessons, and considerations for greenhouse gases. Report, Pew Center on Global Climate Change, May 2003.

Kerry Emanuel. Increasing destructiveness of tropical cyclones over the past 30 years. *Nature*, 436(7051):686–688, 2005.

EPA. Inventory of U.S. greenhouse gas emissions and sinks: 1990 to 2003. Technical Report EPA 430-R-05-003, U.S. Environmental Protection Agency, April 2005.

EPACT. The energy policy act of 2005, Public Law 58 of the 109th U.S. Congress, August 8 2005.

Robert E. Evenson, Paul E. Waggoner, and Vernon W. Ruttan. Economic benefits from research: An example from agriculture. *Science*, 205:1101–1107, 1979.

Jan Fagerberg, David C. Mowery, and Richard R. Nelson (eds.). *The Oxford Handbook of Innovation*. Oxford University Press, New York, 2004.

Carolyn Fischer and Richard D. Morgenstern. Carbon abatement costs: Why the wide range of estimates? Discussion paper RFF DP 03-42, Resources for the Future, September 2003.

Carolyn Fischer. Emissions pricing, spillovers, and public investment in environmentally friendly technologies. Discussion paper RFF DP 04-02, Resources for the Future, February 2004.

J. C. Fisher and R. H. Pry. Simple substitution model of technological change. *Technological Forecasting and Social Change*, 3(1):75–88, 1971.

Donna Fossum, Lawrence S. Painter, Elisa Eiseman, Emile Ettedgui, and David M. Adamson. Vital assets: Federal investment in research and development at the nation's universities and colleges. Report, RAND, 2004.

Marvin Frankel. Obsolescence and technological change in a maturing economy. *The American Economic Review*, 45(3):296–319, 1955.

L. Frantzis, E. Jones, C. Lee, M. Wood, and P. Wormser. Opportunities for cost reductions in photovoltaic modules. In *16th European Photovoltaic Solar Energy Conference*, pages 2100–2103, Glasgow, 2000.

Chris Freeman and Francisco Louca. *As time goes by: from the industrial revolutions to the information revolution*. Oxford University Press, Oxford, 2001.

Chris Freeman and C. Perez. Structural crises of adjustment, business cycles, and investement behavior. In Giovanni Dosi, Chris Freeman, Richard Nelson, G. Silverberg, and Luc Soete, editors, *Technical change and economic theory*. Pinter, London, 1988.

Christopher Freeman and Luc Soete. *The Economics of Industrial Innovation*. The MIT Press, Cambridge, MA, 1997.

Chris Freeman. *The economics of industrial innovation*. The MIT Press, Cambridge, MA, 1974.

Chris Freeman. The economics of technical change. *Cambridge Journal of Economics*, 18(5):463–514, 1994.

Thomas Friedman. The New York Times: A warning from the garden, 19 January 2007.

V. M. Fthenakis and P. D. Moskowitz. Photovoltaics: Environmental, health and safety issues and perspectives. *Progress in Photovoltaics*, 8(1):27–38, 2000.

Kelly Sims Gallagher, John P. Holdren, and Ambuj D. Sagar. Energy-technology innovation. *Annual Review of Environment and Resources*, 31(1):193–237, 2006.

Kelly Sims Gallagher, Ambuj D. Sagar, D. Segal, P. deSa, and John P. Holdren. Doe budget authority for energy research, development, and demonstration database. Technical report, Energy Technology Innovation Project, John F. Kennedy School of Government, Harvard University, 2007.

GAO. Key challenges remain for developing and deploying advanced energy technologies to meet future needs. Report to Congressional Requesters GAO-07-106, U.S. Government Accountability Office, December 2006.

R. Garcia and R. Calantone. A critical look at technological innovation typology and innovativeness terminology: a literature review. *Journal of Product Innovation Management*, 19(2):110–132, 2002.

R. Garud and P. Karnoe. Bricolage versus breakthrough: distributed and embedded agency in technology entrepreneurship. *Research Policy*, 32(2):277–300, 2003.

M. Ghannam, S. Sivoththaman, J. Poortmans, J. Szlufcik, J. Nijs, R. Mertens, and R. Van Overstraeten. Trends in industrial silicon solar cell processes. *Solar Energy*, 59(1-3):101–110, 1997.

Amal K. Ghosh. Solar cells: Present and future technology and cost. In R.F. Meyer, editor, *Long Term Energy Resources*, volume II, pages 761–771, Montreal, 1979. Pitman.

Paul Gipe. *Wind energy comes of age*. Wiley, New York, 1995.

Adolf Goetzberger, Christopher Hebling, and Hans-Werner Schock. Photovoltaic materials, history, status and outlook. *Materials Science and Engineering*, R 40:1–46, 2003.

J. Goldemberg, S. T. Coelho, P. M. Nastari, and O. Lucon. Ethanol learning curve - the brazilian experience. *Biomass and Bioenergy*, 26(3):301–304, 2004.

Paul Gompers and Josh Lerner. The venture capital revolution. *Journal of Economic Perspectives*, 15(2):145–168, 2001.

Austan Goolsbee. Does government R&D policy mainly benefit scientists and engineers? *American Economic Review*, 88(2):298–302, 1998.

A. Goto and K. Suzuki. R&D capital, rate of return on R&D investment and spillover of R&D in japanese manufacturing-industries. *Review of Economics and Statistics*, 71(4):555–564, 1989.

Lawrence H. Goulder. Induced technological change and climate policy. Technical report, Pew Center on Global Climate Change, October 2004.

M. A. Green, J. Zhao, A. Wang, and S. R. Wenham. Progress and outlook for high-efficiency crystalline silicon solar cells. *Solar Energy Materials and Solar Cells*, 65(1-4):9–16, 2001.

Martin A. Green. *Solar Cells: Operating Principles, Technology and System Applications*. University of New South Wales, Kensington, Australia, 1998.

Martin A. Green. Silicon photovoltaic modules: a brief history of the first 50 years. *Progress in Photovoltaics: Research and Applications*, 13(5):447–455, 2005.

D.S. Greenberg. HINDSIGHT - DOD study examines return on investment in research. *Science*, 154(3751):872, 1966.

Zvi Griliches. Hybrid corn: An exploration in the economics of technological change. *Econometrica, Journal of the Econometric Society*, 25(4):501–522, 1957.

Zvi Griliches. Hybrid corn and the economics of innovation. *Science*, 132(3422):275–280, 1960.

Zvi Griliches. R&D and productivity: Measurement issues and econometric results. *Science*, 237(4810):31–35, 1987.

Zvi Griliches. Patent statistics as economic indicators: A survey. *Journal of Economics Literature*, 28:1661–1707, 1990.

Zvi Griliches. The search for R&D spillovers. *Scandinavian Journal of Economics*, 94:29–42, 1992.

Zvi Griliches. *R&D and productivity: the econometric evidence*. University of Chicago Press, Chicago, 1998.

Michael Grubb and Roberto Vigotti. *Renewable Energy Strategies for Europe, Volume II*. Earthscan, London, 1997.

Michael Grubb, Jonathan Köhler, and Dennis Anderson. Induced technical change in energy and environmental modeling: Analytic approaches and policy implications. *Annual Review of Energy and the Environment*, 27:271–308, 2002.

M. Grubb. The skeptical environmentalist - measuring the real state of the world. *Science*, 294(5545):1285–1287, 2001.

Harald Gruber. Trade policy and learning by doing: the case of semiconductors. *Research Policy*, 25:723–739, 1996.

Arnulf Grübler, Nebojsa Nakicenovic, and David G. Victor. Dynamics of energy technologies and global change. *Energy Policy*, 27:247–280, 1999.

Arnulf Grübler, Nebojsa Nakicenovic, and David G. Victor. Modeling technological change: Implications for the global environment. *Annual Review of Energy and the Environment*, 24:545–569, 1999. Review.

Arnulf Grübler. *Technology and Global Change*. Cambridge University Press, Cambridge, 1998.

L. Guey-Lee. Renewable energy 1998, issues and trends: Renewable electricity purchases: History and recent developments. Technical report, U.S. Department of Energy, Energy Information Administration, 1999.

G. Hall and S. Howell. The experience curve from the economist's perspective. *Strategic Management Journal*, 6(3):197–212, 1985.

Bronwyn Hall and Jacques Mairesse. Empirical studies of innovation in the knowledge-driven economy. *Economics of Innovation and New Technology*, 15:289–299, 2006.

Bronwyn H. Hall, Adam B. Jaffe, and Manuel Trajtenberg. The NBER patent citation data file: Lessons, insights and methodological tools. Working paper, NBER, 2001.

Bronwyn H. Hall, Adam B. Jaffe, and Manuel Trajtenberg. Market value and patent citations. *Rand Journal of Economics*, 36(1):16–38, 2005.

Ben Hall. The Financial Times: U.K. plans binding emission targets, 13 March 2007.

W. Michael Hanemann and Alexander E. Farrell. Managing greenhouse gas emissions in California. Technical report, The California Climate Change Center at UC Berkeley, January 2006.

Dietmar Harhoff, Francis Narin, F.M. Scherer, and Katrin Vopel. Citation frequency and the value of patented inventions. *The Review of Economics and Statistics*, 81(3):511–515, 1999.

Katharine Hayhoe, Daniel Cayanc, Christopher B. Field, Peter C. Frumhoff, Edwin P. Maurer, Norman L. Miller, Susanne C. Moser, Stephen H. Schneider, Kimberly Nicholas Cahill, Elsa E. Cleland, Larry Dale, Ray Drapek, R. Michael Hanemann, Laurence S. Kalkstein, James Lenihan, Claire K. Lunch, Ronald P. Neilson, Scott C. Sheridan, and Julia H. Vervillee. Emissions pathways, climate change, and impacts on California. *Proceedings of the National Academies of Sciences*, 101(34):12422–12427, 2004.

Bracken Hendricks. Testimony to the House Committee on Resources, oversight hearing on energy supply and the american consumer, executive director, Apollo Alliance, February 12 2004.

O.C. Herfindahl. *Concentration in the US Steel Industry*. PhD thesis, Columbia University, 1950.

Matthias Heymann. Signs of hubris : The shaping of wind technology styles in Germany, Denmark, and the United States, 1940-1990. *Technology and Culture*, 39(4):641–670, 1998.

John Richard Hicks. *The theory of wages*. P. Smith, 1932.

Richard F. Hirsch. *Power Loss: The Origins of Deregulation and Restructuring in the American Electric Utility System*. The MIT Press, Cambridge, MA, 1999.

Albert O. Hirschman. *National power and the structure of foreign trade*. University of California Press, Berkeley and Los Angeles, 1945.

M. I. Hoffert, K. Caldeira, G. Benford, D. R. Criswell, C. Green, H. Herzog, A. K. Jain, H. S. Kheshgi, K. S. Lackner, J. S. Lewis, H. D. Lightfoot, W. Manheimer, J. C. Mankins, M. E. Mauel, L. J. Perkins, M. E. Schlesinger, T. Volk, and T. M. L. Wigley. Advanced technology paths to global climate stability: Energy for a greenhouse planet. *Science*, 298(5595):981–987, 2002.

M. I. Hoffert, K. Caldeira, G. Benford, T. Volk, D. R. Criswell, C. Green, H. Herzog, A. K. Jain, H. S. Kheshgi, K. S. Lackner, J. S. Lewis, H. D. Lightfoot, W. Manheimer, J. C. Mankins, M. E. Mauel, L. J. Perkins, M. E. Schlesinger, T. Volk, and T. M. L. Wigley. Planning for future energy resources - response. *Science*, 300(5619):582–584, 2003.

John P. Holdren, William K. Reilly, John W. Rowe, and Philip R. Sharp. Ending the energy stalemate: A bipartisan strategy to meet americas energy challenges. Report, The National Commission on Energy Policy, December 2004.

John P. Holdren. The energy innovation imperative: Addressing oil dependence, climate change, and other 21st century energy challenges. *Innovations*, 2006.

J.T. Houghton, Y. Ding, et al. *IPCC Third Assessment Report, Climate Change 2001: The Scientific Basis*. Cambridge University Press, Cambridge, 2001.

George P. Huber. Organizational learning: The contributing processes and the literatures. *Organization Science*, 2(1):88–115, 1991.

Nathan E. Hultman, Jonathan G. Koomey, and Daniel M. Kammen. Viewpoint: What history can teach us about the future costs of U.S. nuclear power? *Environmental Science and Technology*, 41(9):2088–2093, 2007.

IEA. Wind energy annual report. Report, International Energy Agency, 2002.

IEA. International energy agency, energy technology R&D statistics, 2005.

IIT. Technology in retrospect and critical events in science (project TRACES). Technical report, Illinois Institute of Technology (IIT) - National Science Foundation, 1968.

Douglas A. Irwin and Peter J. Klenow. Learning-by-doing spillovers in the semiconductor industry. *Journal of Political Economy*, 102(6):1200–1227, 1994.

Daniel J. Jacob. The greenhouse effect. In *Introduction to Atmospheric Chemistry*, page 136. Princeton University Press, Princeton, NJ, 1999.

Mark Z. Jacobson and Gilbert M. Masters. Energy: Exploiting wind versus coal. *Science*, 293(5534):1438–, 2001.

Staffan Jacobsson and Volkmar Lauber. The politics and policy of energy system transformation–explaining the german diffusion of renewable energy technology. *Energy Policy*, 34(3):256–276, 2006.

H. D. Jacoby and A. D. Ellerman. The safety valve and climate policy. *Energy Policy*, 32(4):481–491, 2004.

Arnulf Jaeger-Waldau. PV status report 2004: Research, solar cell production and market implementation of photovoltaics. Technical Report EUR 21390 EN, European Commission, DG JRC, Institute for Environment and Sustainability, Renewable Energies Unit, October 2004.

Adam B Jaffe and Manuel Trajtenberg. *Patents, citations, and innovations: a window on the knowledge economy*. MIT Press, Cambridge, Mass., 2002.

Adam B. Jaffe, Richard G. Newell, and Robert N. Stavins. Environmental policy and technological change. *Environmental and Resource Economics*, 22(1 - 2):41–70, 2002.

Adam B. Jaffe, Richard G. Newell, and Robert N. Stavins. A tale of two market failures: Technology and environmental policy. *Ecological Economics*, 54(2-3):164–174, 2005.

Michael Jefferson. Energy technologies for the 21st century. Report, World Energy Council, August 2001.

Charles I. Jones and John C. Williams. Measuring the social return to R&D. *The Quarterly Journal of Economics*, pages 1119–1135, 1998.

JPEA. Shipping statistics of PV cells and modules, FY2001. Newsletter No. 02-03, Japanese Photovoltaic Energy Association (JPEA), July 2002.

M. Junginger, A. Faaij, and W.C. Turkenburg. Global experience curves for wind farms. *Energy Policy*, 33:133–150, 2005.

Edward Kahn. *Electric Utility Planning and Regulation*. American Council for an Energy Efficient Economy, Washington, DC, 1991.

Nicholas Kaldor. The irrelevance of equilibrium economics. *The Economic Journal*, 82:1237–1255, 1972.

Daniel M. Kammen and Gregory F. Nemet. Reversing the incredible shrinking U.S. energy R&D budget. *Issues in Science and Technology*, 22(1):84–88, 2005.

Daniel M. Kammen. The future of university nuclear science and engineering programs. In *U.S. House of Representatives Science Committee, sub-committee on Energy*, Washington, D.C., 2003.

Daniel M. Kammen. Department of energy's plan for climate change technology programs. In *U.S. House of Representatives Science Committee, sub-committee on Energy*, Washington, D.C., 2006.

D. L. Kelly and C. D. Kolstad. Bayesian learning, growth, and pollution. *Journal of Economic Dynamics and Control*, 23(4):491–518, 1999.

Rene Kemp. *Environmental policy and technical change: a comparison of the technological impact of policy instruments*. Edward Elgar, Cheltenham, UK, 1997.

Rene Kemp. The diffusion of biological waste-water treatment plants in the dutch food and beverage industry. *Environmental and Resource Economics*, 12(1):113–136, 1998.

Rene Kemp. Technology and environmental policy: Innovation effects of past policies and suggestions for improvement. In Sung Chul Chung, editor, *OECD Proceedings: Innnovation and the Environment*, page 35. OECD, Paris, 2000.

Donald Kennedy. Climate change and climate science. *Science*, 304(5677):1565, 2004.

M.S. Keshner and R. Arya. Study of the potential cost reductions resulting from super-large-scale manufacturing of PV modules. Subcontractor Report NREL/SR-520-36846, National Renewable Energy Laboratory, October 2004.

G. Klaassen, A. Miketa, K. Larsen, and T. Sundqvist. The impact of R&D on innovation for wind energy in Denmark, Germany and the United Kingdom. *Ecological Economics*, 54(2-3):227–240, 2005.

Alfred Kleinknecht and Bart Verspagen. Demand and innovation: Schmookler re-examined. *Research Policy*, 19(4):387–394, 1990.

A. K. Klevorick, R. C. Levin, R. R. Nelson, and S. G. Winter. On the sources and significance of interindustry differences in technological opportunities. *Research Policy*, 24(2):185–205, 1995.

S.J. Kline and Nate Rosenberg. An overview of innovation. In R. Landau and N. Rosenberg, editors, *The Positive Sum Strategy*. Academy of Engineering Press, Washington, 1986.

K. E. Knapp. Exploring energy technology substitution for reducing atmospheric carbon emissions. *Energy Journal*, 20(2):121–143, 1999.

S. Kortum and J. Lerner. Does venture capital spur innovation? *Rand Journal of Economics*, 31:674–692, 2000.

Nikolaos Kouvaritakis, Antonio Soria, and Stephane Isoard. Modelling energy technology dynamics: methodology for adaptive expectations models with learning by doing and learning by searching. *International Journal of Global Energy Issues*, 14(1-4):104 – 115, 2000.

Thomas Kuhn. Annual report of the U.S. shareholder-owned electric utility industry. Annual report, Edison Electric Institute, 2004.

Frank N. Laird. *Solar Energy, Technology Policy, and Institutional Values*. Cambridge University Press, New York, 2001.

D. A. Levinthal and J. G. March. The myopia of learning. *Strategic Management Journal*, 14:95–112, 1993. Sp. Iss. SI.

B. Levitt and J. G. March. Organizational learning. *Annual Review of Sociology*, 14:319–340, 1988.

Nathan S. Lewis. Basic research needs for solar energy utilization. Report on the basic energy sciences workshop on solar energy utilization, Department of Energy, April 18-21 2005.

Frank R. Lichtenberg. Energy prices and induced innovation. *Research Policy*, 15(2):67–75, 1986.

Roger G. Little and Michael J. Nowlan. Crystalline silicon photovoltaics: the hurdle for thin films. *Progress in Photovoltaics: Research and Applications*, 5(5):309–315, 1997.

Michael Lotker. Barriers to commercialization of large-scale solar electricity: Lessons learned from the LUZ experience. Contractor Report SAND91-7014, Sandia National Laboratories, November 1991.

Erik Strojer Madsen, Camilla Jensen, and Jorgen Drud Hansen. Scale in technology and learning by doing in the windmill industry. *Journal for International Business and Entrepreneurship Development*, 1(2), 2003.

Joel Makower, Ron Pernick, and Clint Wilder. Clean energy trends 2007. Technical report, Cleanedge, March 2007.

Franco Malerba. Learning by firms and incremental technical change. *The Economic Journal*, 102(413):845–859, 1992.

E. Mansfield, J. Rapoport, A. Romeo, S. Wagner, and G. Beardsley. Social and private rates of return from industrial innovations. *Quarterly Journal of Economics*, 91(2):221–240, 1977.

E. Mansfield. Technical change and the rate of imitation. *Econometrica*, 29(4):741–766, 1961.

Edwin Mansfield. Contribution of R&D to economic growth in the united states. *Science*, 175:477–486, 1972.

John H. Marburger. Science for the 21st century. Technical report, U.S. Office of Science and Technology Policy, July 2004.

Robert M. Margolis and Daniel M. Kammen. Evidence of under-investment in energy R&D in the united states and the impact of federal policy. *Energy Policy*, 27:575–584, 1999.

Robert M. Margolis and Daniel M. Kammen. Underinvestment: The energy R&D challenge. *Science*, 285:690–692, 1999.

Eric Martinot. Renewables 2005: Global status report. Report, Worldwatch Institute, November 2005.

Karl Marx. Chapter 15: Machinery and modern industry. In *Capital*. Harmondsworth, Middlesex, England, 1867.

Robert M. May. The scientific wealth of nations. *Science*, 275(5301):793, 1997.

Paul D. Maycock and Edward N. Stirewalt. *A Guide to the Photovoltaic Revolution*. Rodale Press, Emmaus, PA, 1985.

Paul D. Maycock. U.S.-Japanese competition for the world photovoltaic market. In Charles K. Ebinger and Ronald A. Morse, editors, *U.S.-Japanese Energy Relations: Cooperation and Competition*, pages 207–217. Westview Press, London, 1984.

Paul D. Maycock. International photovoltaic markets, developments, and trends forecast to 2010. In *Conference Record of the twenty-fourth IEEE Photovoltaic Specialists Conference*, volume 1, pages 694–699. IEEE, 1994.

Paul D. Maycock. Cost reduction in PV manufacturing: Impact on grid-connected and building-integrated markets. *Solar Energy Materials and Solar Cells*, 47:37–45, 1997.

Paul D. Maycock. The world photovoltaic market. Report, PV Energy Systems, January 2002.

Paul D. Maycock. PV market update. *Renewable Energy World*, 6(4):84–101, 2003.

Paul D. Maycock. PV news. Report, PV Energy Systems, 2005.

Alan McDonald and Leo Schrattenholzer. Learning rates for energy technologies. *Energy Policy*, 29:255–261, 2001.

Jon G. McGowan and Stephen R. Connors. Wind power: A turn of the century review. *Annual Review of Energy and the Environment*, 25(1):147–197, 2000.

Adam McLean. The ITER fusion reactor and its role in the development of a fusion power plant. *Radiation Protection Management*, 22(5):27–36, 2002.

Bruce McPherson. California proposition 87: Alternative energy. research, production, incentives. tax on california oil producers. initiative constitutional amendment and statute., 2006.

Ronald L. Meeks. Federal R&D funding by budget function: Fiscal years 2003-05. Report NSF 05-303, National Science Foundation, Division of Science Resources Statistics, 2004.

Philippe Menanteau. Learning from variety and competition between technological options for generating photovoltaic electricity. *Technological Forecasting and Social Change*, 63(1):63–80, 2000.

Sabine Messner. Endogenized technological learning in an energy systems model. *Journal of Evolutionary Economics*, 7(3):291–313, 1997.

Bert Metz, Ogunlade Davidson, Rob Swart, and Jiahua Pan. *Climate Change 2001: Mitigation, Contribution of Working Group III to the Third Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge University Press, Cambridge, UK, 2001.

Asami Miketa and Leo Schrattenholzer. Experiments with a methodology to model the role of R&D expenditures in energy technology learning processes; first results. *Energy Policy*, 32:1679–1692, 2004.

MIT. R&D scorecard 2002. *Technology Review*, 105(10):59, 2002.

Richard L. Mitchell, C. Edwin Witt, Richard King, and Douglas Ruby. PVMaT advances in the photovoltaic industry and the focus of future PV manufacturing R&D. In *Conference Record of the 29th IEEE Photovoltaic Specialists Conference*, pages 1444–1447, 2002.

W. David Montgomery and Anne E. Smith. Price, quantity, and technology strategies for climate change policy. Working paper, Charles River Associates, October 2005.

Michal C. Moore, Douglas J. Arent, and Douglas Norland. R&D advancement, technology diffusion, and impact on evaluation of public R&D. *Energy Policy*, 35(3):1464–1473, 2007.

Robert M. Moore. Czochralski silicon solar cell modules: Present cost and future prospects. *Solar Cells*, 5:313–329, 1982.

M. Granger Morgan and Susan F. Tierney. Research support for the power industry. *Issues in Science and Technology*, 1998.

David Mowery and Nathan Rosenberg. The influence of market demand upon innovation: a critical review of some recent empirical studies. *Research Policy*, 8(2):102–153, 1979.

David C. Mowery and Nathan Rosenberg. *Technology and the Pursuit of Economic Growth*. Cambridge University Press, Cambridge, UK, 1989.

David C. Mowery and Nathan Rosenberg. *Paths of Innovation: Technological Change in 20th-Century America*. Cambridge University Press, Cambridge, 1998.

David C. Mowery. Economic-theory and government technology policy. *Policy Sciences*, 16(1):27–43, 1983.

David Mowery. The practice of technology policy. In Paul Stoneman, editor, *Handbook of the Economics of Innovation and Technological Change*, pages 513–557. Blackwell, Oxford, 1995.

David C. Mowery. Collaborative R&D: How effective is it? *Issues in Science and Technology*, 15(1):37–44, 1998.

David C. Mowery. Lessons from the history of federal R&D policy for an Energy ARPA. In *Testimony to the Committee on Science, U.S. House of Representatives*, Washington, D.C., 2006.

Kevin Murray. Senate Bill 1: The million solar roofs program, August 21 2002.

M. Ishaq Nadiri. Innovations and technological spillovers. Working Paper 4423, National Bureau of Economic Research, August 1993.

Nebojsa Nakicenovic and Keywan Riahi. An assessment of technological change across selected energy scenarios. Research Report RR-02-005, IIASA, May 2002.

Nebojsa Nakicenovic, Joseph Alcamo, Gerald Davis, Bert de Vries, Joergen Fenhann, and Stuart Gaffin. *Special Report on Emissions Scenarios, A Special Report of Working Group III of the Intergovernmental Panel on Climate Change*. Cambridge University Press, Cambridge, UK, 2000.

Lena Neij, Per Dannemand Andersen, and Michael Durstewitz. The use of experience curves for assessing energy policy programs. In *Proceedings of the EU/IEA Workshop on Experience Curves: A tool for energy policy analysis and design*, Paris, 2003.

Richard R. Nelson and Sidney G. Winter. In search of useful theory of innovation. *Research Policy*, 6(1):36–76, 1977.

Richard R. Nelson and Sidney G. Winter. *An Evolutionary Theory of Economic Change*. The Belknap Press of Harvard University Press, Cambridge, MA, 1982.

Richard R. Nelson. The simple economics of basic scientific-research. *Journal of Political Economy*, 67(3):297–306, 1959.

Richard R. Nelson. Modelling the connections in the cross section between technical progress and R&D intensity. *The Rand Journal of Economics*, 19(3):478–485, 1988.

Gregory F. Nemet and Daniel M. Kammen. U.S. energy research and development: Declining investment, increasing need, and the feasibility of expansion. *Energy Policy*, 35(1):746–755, 2007.

Gregory F. Nemet. Beyond the learning curve: factors influencing cost reductions in photovoltaics. *Energy Policy*, 34(17):3218–3232, 2006.

Karsten Neuhoff, Gregory Nemet, Misato Sato, and Katja Schumacher. The role of the supply chain in innovation: the example of photovoltaic cells. *Manuscript*, 2007.

Richard M. Nixon. State of the union address, 1974.

Vicki Norberg-Bohm. Stimulating green technological innovation: An analysis of alternative policy mechanisms. *Policy Sciences*, 32(1):13–38, 1999.

Vicki Norberg-Bohm. Creating incentives for environmentally enhancing technological change: Lessons from 30 years of U.S. energy technology policy. *Technological Forecasting and Social Change*, 65(2):125–148, 2000.

William D. Nordhaus. Modeling induced innovation in climate change policy. In Arnulf Grubler, Nebojsa Nakicenovic, and William D. Nordhaus, editors, *Technological change and the environment*, pages 182–209. Resources for the Future, International Institute for Applied Systems Analysis, Washington, DC, 2002.

NRC. *Energy Research at DOE: Was It Worth It? Energy Efficiency and Fossil Energy Research 1978 to 2000.* National Academies Press, Washington, DC, 2001.

Fabian Nunez. Assembly Bill 32: the California global warming solutions act of 2006, September 27 2006.

NYT. Searches on Lexis-Nexis for the keyword “energy” in the abstracts and titles of articles appearing in the New York Times, 1970-2005, 2006.

Sumiye Okubo, Carol A. Robbins, Carol E. Moylan, Brian K. Sliker, Laura I. Schultz, and Lisa S. Mataloni. R&D satellite account: Preliminary estimates. Report, Bureau of Economic Analysis/National Science Foundation, September 2006.

Brian O’Neill, Arnulf Grübler, and Nebojsa Nakicenovic. Letters to the editor: Planning for future energy resources. *Science*, 300:581, 2003.

Steve Pacala and Robert Socolow. Stabilization wedges: Solving the climate problem for the next 50 years with current technologies. *Science*, 305:968–972, 2004.

Keith Pavitt. Sectoral patterns of technical change - towards a taxonomy and a theory. *Research Policy*, 13(6):343–373, 1984.

Steven Payson. Quality improvement versus cost reduction: A broader perspective on evolutionary economic change. *Technology Analysis and Strategic Management*, 10(1):69–88, 1998.

PCAST. Report to the president on federal energy research and development for the challenges of the twenty-first century. Technical report, Office of the President, 1997.

David Pearce. The role of carbon taxes in adjusting to global warming. *The Economic Journal*, 101(407):938–948, 1991.

Michael R. Peevey and Kim Malcolm. Interim order adopting policies and funding for the california solar initiative, 13 December 2006.

PG&E. Annual report for 2006. Investor relations report, Pacific Gas and Electric Corporation, 2007.

William A. Pizer and David Popp. Endogenizing technological change: Matching empirical evidence to modeling needs. *National Bureau of Economic Research Working Paper Series*, No. 13053, 2007.

William Pizer. The case for intensity targets. Discussion Paper RFF-DP-05-02, Resources for the Future, January 2005.

Nicholas Ponomarenko. Interview with primary patent examiner, U.S. patent and trademark office, January 26 2004.

David Popp. Induced innovation and energy prices. *American Economic Review*, 92(1):160–180, 2002.

David Popp. Pollution control innovations and the clean air act of 1990. *Journal of Policy Analysis and Management*, 22(4):641–660, 2003.

David Popp. Lessons from patents: Using patents to measure technological change in environmental models. *Ecological Economics*, 54(2-3):209–226, 2005.

PriceWaterhouseCoopers. Money tree survey. Report, PriceWaterhouseCoopers International Limited, 2001.

Rodrigo Prudencio. Nth power 2003 energy venture capital study. Technical report, Nth Power LLC, 2005.

Leonard Rapping. Learning and world war II production functions. *The Review of Economic Statistics*, 47(1):81–86, 1965.

Till Requate. Dynamic incentives by environmental policy instruments - a survey. *Ecological Economics*, 54(2-3):175–195, 2005.

Till Requate. Timing and commitment of environmental policy, adoption of new technology, and repercussions on R&D. *Environmental and Resource Economics*, 31(2):175–199, 2005. doi:10.1007/s10640-005-1770-x.

Andrew Revkin. The New York Times: Budgets falling in race to fight global warming, October 30 2006.

Richard G. Richels, Thomas Rutherford, Geoffrey Blanford, and Leon Clarke. Managing the transition to climate stabilization. Working Paper 07-01, AEI-Brookings Joint Center for Regulatory Studies, January 2007.

David J. Roessner. Government-industry relationships in technology commercialization: The case of photovoltaics. *Solar Cells*, 5(2):101–134, 1982.

Everett M. Rogers. Categorizing the adopters of agricultural practices. *Rural Sociology*, 23(4):345–354, 1958.

A. Rohatgi. Road to cost-effective crystalline silicon photovoltaics. In *Proceedings of 3rd World Conference on Photovoltaic Energy Conversion*, volume A, pages 829–834, Osaka, Japan, 2003.

Paul M. Romer. Endogenous technological change. *The Journal of Political Economy*, 98(5):S71–S102, 1990.

Nathan Rosenberg. Direction of technological change - inducement mechanisms and focusing devices. *Economic Development and Cultural Change*, 18(1):1–24, 1969.

Nathan Rosenberg. Science, invention and economic growth. *Economic Journal*, 84(333):90–108, 1974.

Nathan Rosenberg. The historiography of technical progress. In *Inside the Black Box: Technology and Economics*. Cambridge University Press, Cambridge, 1982.

Nathan Rosenberg. *Inside the Black Box: Technology and Economics*. Cambridge University Press, Cambridge, 1982.

Nathan Rosenberg. Why do firms do basic research (with their own money)? *Research Policy*, 19(2):165–174, 1990.

Nathan Rosenberg. *Exploring the Black Box: Technology, Economics, and History*. Cambridge University Press, Cambridge, 1994.

Roy Rothwell. Towards the fifth-generation innovation process. In J. Henry and D. Mayle, editors, *Managing Innovation and Chance*, pages 115–135. Sage, London, 2002.

Paul Runci, Leon Clarke, and James Dooley. Energy research. *Issues in Science and Technology*, 22(3), 2006.

Vernon W. Ruttan. *Technology, Growth, and Development: An Induced Innovation Perspective*. Oxford University Press, New York, 2001.

Vernon W. Ruttan. *Is war necessary for economic growth? military procurement and technology development*. Oxford University Press, Oxford, 2006.

Vernon W. Ruttan. Will government programs spur the next breakthrough? *Issues in Science and Technology*, 22(2):55–61, 2006.

Ambuj D. Sagar and Bob van der Zwaan. Technological innovation in the energy sector: R&D, deployment, and learning-by-doing. *Energy Policy*, 34(17):2601–2608, 2006.

Bjorn A. Sanden and Christian Azar. Near-term technology policies for long-term climate targets–economy wide versus technology specific approaches. *Energy Policy*, 33(12):1557–1576, 2005.

Enrico Santarelli and Roberta Piergiovanni. Analyzing literature-based innovation output indicators: the Italian experience. *Research Policy*, 25(5):689–711, 1996.

Dominique Sarti and Roland Einhaus. Silicon feedstock for the multi-crystalline photovoltaic industry. *Solar Energy Materials and Solar Cells*, 72(1-4):27–40, 2002.

Janet Laughlin Sawin. *The Role of Government in the Development of Renewable Energy Technologies: Wind Power in the United States, California, Denmark, and Germany, 1970–2000*. PhD thesis, Tufts University, 2001.

SCE. Annual report for 2006. Investor relations report, Southern California Edison, 2007.

G.J. Schaeffer, A.J. Seebregts, L.W.M. Beurskens, H.H.C. de Moor, E.A. Alsema, W. Sark, M. Durstewicz, M. Perrin, P. Boulanger, H. Laukamp, and C. Zuccaro. Learning from the sun; analysis of the use of experience curves for energy policy purposes: The case of photovoltaic power. final report of the PHOTEX project. Report ECN DEGO: ECN-C–04-035, ECN Renewable Energy in the Built Environment, 2004.

F. M. Scherer. Demand-pull and technological invention - Schmookler revisted. *Journal of Industrial Economics*, 30(3):225–237, 1982.

F. M. Scherer. Inter-industry technology flows in the United States. *Research Policy*, 11(4):227–245, 1982.

Lisa Schmidt. The Calgary Herald: Ottawa caps oil industry emissions: Carbon credits limited to $15 a tonne, 19 December 2002.

Jacob Schmookler. Economic sources of inventive activity. *Journal of Economic History*, 22(1):1–20, 1962.

Jacob Schmookler. *Invention and economic growth*. Harvard University Press, Cambridge, MA, 1966.

Robert N. Schock, William Fulkerson, Merwin L. Brown, Robert L. San Martin, David L. Greene, and Jae Edmonds. How much is energy research and development worth as insurance? *Annual Review of Energy and Environment*, 24:487–512, 1999.

Joseph A. Schumpeter. *The Theory of Economic Development*. Harvard University Press, Cambridge, MA, 1934.

Joseph A. Schumpeter. *Capitalism, socialism, and democracy*. Harper, New York, London, 2d ed. edition, 1947.

Arnold Schwarzenegger. Executive order S-3-05 by the Governor of the State of California, 2005.

Suzanne Scotchmer. Chapter 2: Investing in knowledge. In *Innovation and Incentives*. The MIT Press, Cambridge, MA, 2005.

SDG&E. Annual report for 2006. Investor relations report, Sempra Energy, 2007.

SEIA. Our solar power future: The photovoltaics industry roadmap through 2030 and beyond. Report, Solar Energy Industries Association, September 2004.

R. Seitz. More options offered for long-term energy solutions. *Physics Today*, 58(4):14–14, 2005.

Ram C. Sekar, John E. Parsons, Howard J. Herzog, and Henry D. Jacoby. Future carbon regulations and current investments in alternative coal-fired power plant technologies. *Energy Policy*, In Press, Corrected Proof, 2006.

S. A. Shaheen, J. Wright, and D. Sperling. California's zero-emission vehicle mandate - linking clean-fuel cars, carsharing, and station car strategies. Report 0361-1981, Transportation Research Board of the National Academies, 2002.

Byron Sher. Senate Bill 1078: The Renewables Portfolio Standard - chapter 516, statutes of 2002, September 27 2002.

C. W. Sherwin and R. S. Isenson. First interim report on Project HINDSIGHT. Interim Report AD 642-400, Clearinghouse for Federal Scientific and Technical Information, 30 June 1966.

C. W. Sherwin and R. S. Isenson. Project HINDSIGHT. *Science*, 156(3782):1571, 1967.

Eytan Sheshinski. Tests of the learning by doing hypothesis. *Review of Economics and Statistics*, 49(4):568–578, 1967.

Herbert A. Simon. Theories of decision-making in economics and behavioral science. *The American Economic Review*, 49(3):253–283, 1959.

Fred Sissine. DOE budget earmarks: A selective look at energy efficiency and renewable energy R&D programs. CRC report for congress, The Library of Congress: Congressional Research Service, 3 March 2006.

Adam Smith. Chapter 1: Of the division of labor. In *An Inquiry into the Nature and the Causes of the Wealth of Nations*. Methuen and Co., London, 1776.

Robert Solow. A contribution to the theory of economic growth. *Quarterly Journal of Economics*, 70(1):65–94, 1956.

Robert M. Solow. *Growth Theory: an Exposition*. Oxford University Press, Oxford, 2nd edition, 2000.

Paul W. Stackhouse and Charles H. Whitlock. Surface meteorology and solar energy data set. Database, NASA's Science Mission Directorate, Earth-Sun System Division, Applied Sciences Program, December 2005.

Nicholas Stern. *Stern Review on the economics of climate change*. Cambridge University Press, Cambridge, UK, 2006.

Donald E. Stokes. *Pasteur's quadrant: basic science and technological innovation*. Brookings Institution Press, Washington, D.C., 1997.

Strategies-Unlimited. Photovoltaic five-year market forecast, 2002-2007. Technical Report PM-52, Strategies Unlimited, March 2003.

Thomas Surek. Progress in U.S. photovoltaics: Looking back 30 years and looking ahead 20. In *3rd World Conference on Photovoltaic Energy Conversion*, Osaka, Japan, 2003.

L. Sveikauskas. Technological inputs and multifactor productivity growth. *Review of Economics and Statistics*, 63(2):275–282, 1981.

Richard M. Swanson. A vision for crystalline silicon photovoltaics. *Progress in Photovoltaics: Research and Applications*, 14(5):443–453, 2006.

M. Symko-Davies, R.L. Mitchell, C.E. Witt, H.P. Thomas, R. King, and D. Ruby. Decade of PV industry R&D advances in silicon module manufacturing. In *Record of the Twenty-Eighth IEEE Photovoltaic Specialists Conference, 2000. Conference*, pages 1460–1463, Anchorage, AK USA, 2000.

Margaret R. Taylor and Gregory F. Nemet. The interaction between policy and innovation in clean energy technologies. In *USAEE/IAEE/ASSA meetings: Current Issues in Energy Economics and Energy Modeling*, Boston, 2006.

Margaret R. Taylor, Edward S. Rubin, and David A. Hounshell. Effect of government actions on technological innovation for SO_2 control. *Environmental Science and Technology*, 37(20):4527–4534, 2003.

Margaret R. Taylor, Edward S. Rubin, and David A. Hounshell. Control of SO2 emissions from power plants: A case of induced technological innovation in the U.S. *Technological Forecasting and Social Change*, 72(6):697–718, 2005.

Margaret Taylor, Dorothy Thornton, Gregory Nemet, and Michael Colvin. Government actions and innovation in environmental technology for power production: The cases of selective catalytic reduction and wind power in california. PIER-EA Report CEC-500-2006-053, California Energy Commission, June 2006.

Margaret R. Taylor. *The Influence of Government Actions on Innovative Activities in the Development of Environmental Technologies to Control Sulfur Dioxide Emissions from Stationary Sources*. PhD thesis, Carnegie-Mellon University, 2001.

David J. Teece. Profiting from technological innovation - implications for integration, collaboration, licensing and public-policy. *Research Policy*, 15(6):285–305, 1986.

Nestor Terleckyj. Effects of R&D on the productivity growth of industries: An explanatory study. Report, National Planning Association, 1974.

Peter Thompson. How much did the liberty shipbuilders learn? new evidence for an old case study. *Journal of Political Economy*, 109(1):103–137, 2001.

Richard S. J. Tol. Estimates of the damage costs of climate change. part 1: Benchmark estimates. *Environmental and Resource Economics*, V21(1):47–73, 2002.

Margaret S. Torn and John Harte. Missing feedbacks, asymmetric uncertainties, and the underestimation of future warming. *Geophysical Research Letters*, 33(L10703), 2006.

U.S. The Constitution of the United States, 1790.

Abbott P. Usher. *A History of Mechanical Inventions*. Harvard University Press, Cambridge, MA, 1954.

USPTO. U.S. patent and trademark office: Manual of patent classification, 2005.

USPTO. The U.S. patent and trademark office bibliographic database, 5/11/06 2006.

Vijay V. Vaitheeswaran. *Power to the People: How the Coming Energy Revolution Will Transform an Industry, Change Our Lives, and Maybe Even Save the Planet*. Farrar, Straus and Giroux, 2003.

Bob van der Zwaan and Ari Rabl. The learning potential of photovoltaics: Implications for energy policy. *Energy Policy*, 32:1545–1554, 2004.

Daan P. van Soest and Erwin H. Bulte. Does the energy-efficiency paradox exist? technological progress and uncertainty. *Environmental and Resource Economics*, 18(1):101–112, 2001.

Raymond Vernon. International investment and international trade in product cycle. *Quarterly Journal of Economics*, 80(2):190–207, 1966.

David G. Victor, Joshua C. House, and Sarah Joy. Climate: Enhanced: A Madisonian approach to climate policy. *Science*, 309(5742):1820–1821, 2005.

V. Walsh. Invention and innovation in the chemical-industry - demand-pull or discovery-push. *Research Policy*, 13(4):211–234, 1984.

Chihiro Watanabe, Kouji Wakabayashi, and Toshinori Miyazawa. Industrial dynamism and the creation of a “virtuous cycle” between R&D, market growth and price reduction - the case of photovoltaic power generation (PV) development in Japan. *Technovation*, 20:299–312, 2000.

Chihiro Watanabe, Youichirou S. Tsuji, and Charla Griffy-Brown. Patent statistics: deciphering a ‘real’ versus a ‘pseudo’ proxy of innovation. *Technovation*, 21(12):783–790, 2001.

Chihiro Watanabe, A. Nagamatsu, and C. Griffy-Brown. Behavior of technology in reducing prices of innovative goods—an analysis of the governing factors of variance of PV module prices. *Technovation*, 23:423–436, 2003.

Clas-Otto Wene. Experience curves for technology policy. Technical report, International Energy Agency, 2000.

T. M. L. Wigley, R. Richels, and J. A. Edmonds. Economic and environmental choices in the stabilization of atmospheric CO_2 concentrations. *Nature*, 379:240 – 243, 1996.

Robert H. Williams and Gregory Tarzian. A benefit-cost analysis of accelerated development of photovoltaic technology. PU-CEES Report No. 281, The Center for Energy and Environmental Studies - Princeton University, October 1993.

Peter Woditsch and Wolfgang Koch. Solar grade silicon feedstock supply for PV industry. *Solar Energy Materials and Solar Cells*, 72(1-4):11–26, 2002.

John H. Wohlgemuth. Long term photovoltaic reliability. In *NCPV and Solar Program Review Meeting*, pages 179–182, Denver, CO, 2003.

Martin Wolf. Historic development of photovoltaic power generation. In Helmut R. Loesch, editor, *International Conference on Photovoltaic Power Generation*, pages 49–65, Hamburg, Germany, 1974. Deutsche Gesellschaft fuer Luft- und Raumfahrt.

Raymond M. Wolfe. Research and development in industry. Report, National Science Foundation, Division of Science Resources Statistics, 2004.

Raymond M. Wolfe. Research and development in industry: 2003. Report, National Science Foundation, Division of Science Resources Statistics, 15 August 2006.

WPRS. Results from the wind project performance reporting system (1985). Annual Report P500-85-013, California Energy Commission, August 1986.

WPRS. Results from the wind project performance reporting system (1986). Annual Report P500-87-019, California Energy Commission, August 1987.

WPRS. Results from the wind project performance reporting system (1987). Annual Report P500-88-005, California Energy Commission, August 1988.

WPRS. Results from the wind project performance reporting system (1988). Annual Report P500-89-010, California Energy Commission, August 1989.

WPRS. Results from the wind project performance reporting system (1989). Annual Report P500-90-010, California Energy Commission, August 1990.

WPRS. Results from the wind project performance reporting system (1990). Annual Report P500-91-010, California Energy Commission, August 1991.

WPRS. Results from the wind project performance reporting system (1991). Annual Report P500-92-002, California Energy Commission, August 1992.

WPRS. Results from the wind project performance reporting system (1992). Annual Report P500-93-002, California Energy Commission, August 1993.

WPRS. Results from the wind project performance reporting system (1993). Annual Report P500-95-001, California Energy Commission, August 1995.

WPRS. Results from the wind project performance reporting system (1994). Annual Report P500-95-003, California Energy Commission, August 1995.

WPRS. Results from the wind project performance reporting system (1995). Annual Report 500-97-003, California Energy Commission, August 1997.

WPRS. Results from the wind project performance reporting system (1996–1999). Annual Report 500-01-018, California Energy Commission, August 2001.

WPRS. Results from the wind project performance reporting system (2000–2001). Annual Report 500-02-034F, California Energy Commission, August 2002.

WPRS. Wind performance reporting system. Database, California Wind Energy Collaborative, California Energy Commission, August 2006.

T. P. Wright. Factors affecting the costs of airplanes. *Journal of the Aeronautical Sciences*, 3:122–128, 1936.

Chi-Jen Yang and Michael Oppenheimer. A “Manhattan project” for climate change? *Climatic Change*, V80(3):199–204, 2007.

Louis E. Yelle. The learning curve: Historical review and comprehensive survey. *Decision Science*, 10:302–328, 1979.