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A STRUCTURALIST ASSESSMENT OF TECHNOLOGY POLICIES – TAKING SCHUMPETER SERIOUSLY ON POLICY

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A STRUCTURALIST ASSESSMENT OF TECHNOLOGY POLICIES – TAKING SCHUMPETER SERIOUSLY ON POLICY

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EXECUTIVE SUMMARY

This monograph evaluates a selection of government policies designed to influence technological change. Public policy can seek to accelerate or retard technological change, using incentives or disincentives that are either generally applied or narrowly focussed. Policy can also seek to exert its influence indirectly by altering such structural variables as the concentration of industry, the amount of foreign investment, the location of firms and the education system.

At the level of abstraction at which economists commonly operate, the distinctions between policies, programs and projects are ignored, and the general term *policy* is used. Our analysis requires that the distinctions be highlighted in assessing policies and also in evaluating programs. Furthermore, within policies we distinguish three types. *Framework policies*, such as research and development (R&D) tax credits, provide general support for specific activities across the entire economy; they are usually single-instrument policies that do not discriminate between firms, industries or technologies. *Focussed policies* are designed to encourage the development of specific technologies such as nuclear power, and are usually so narrowly focussed that falling within the focus is both a necessary and sufficient condition for obtaining benefits under the policy. *Blanket policies*, such as the Industrial and Regional Development Program (IRDP), incorporate elements of both framework and focussed policies: like framework policies, they have broad-based objectives; like focussed policies, they use multiple instruments and have some form of assessment mechanism enabling administrators to tailor the assistance they provide.

We study the policy advice that follows from two distinct theoretical paradigms. *Neoclassical theory* provides a precise set of objectives for policies and programs, and a means of assessing whether or not they have contributed gains. What we call *structuralist-evolutionary theory* starts from different assumptions about the behaviour of the economy, and it reaches different conclusions about both the role of policy and the substance of policy/program evaluation. We find that these differences account for striking divergences in views about the efficacy of various policies and programs.

The two types of theory also suggest different criteria for assessing incrementality. Because they treat structure and institutions as “black boxes,” neoclassical theories assess incrementality solely by the effects on technological change, usually measured by changes in R&D expenditure. Because the structuralist decomposition stresses the relation between technology and the underlying structure through which it operates, the incrementality criterion allows for policies that alter structural relations without necessarily affecting the level of R&D expenditure or inducing specific technological changes.

Neoclassical theory recommends policies that give generalized support to all R&D over policies that are more selective, while structuralist theories suggest a place for both generalized and selective policies.

Neoclassical theory provides criteria for optimal policies while the structuralist model suggests that irreducible elements of judgment are required for all policy decisions — judgments over which reasonable people may simply have to agree to disagree.

The direct approach of measuring the outputs of blanket policies is seldom easy and often impossible. As an alternative assessment procedure, we turn to the criteria derived from Lipsey and Carlaw’s 1996 study of the conditions that favour success or failure in focussed programs, and we use these criteria in our current assessment of the design and operation of blanket policies and programs. First, we examine the available assessments made by others. These concentrate mainly, but not exclusively, on outputs. Second, we compare the design of the policy or program in question with Lipsey’s and Carlaw’s design and operation criteria, using them to judge the potential for success or failure. Third, where there is agreement between the judgments reached under both of the above procedures, we conclude that there is a strong case for either success or failure. Fourth, if the judgments resulting from the two procedures disagree, we seek to reconcile the differences by comparing the theoretical perspectives adopted by ourselves and the other assessors. The differing judgments often arise from the different assumptions that characterize the theories employed.

In the cases of the Defence Industry Productivity Program (DIPP) in Chapter 2 and of the Industrial Research Assistance Program (IRAP) in Chapter 4, our assessments are favourable. Where the assessments of other evaluators differ from ours, we find two main causes. The first is that different incrementality criteria are used, with our assessments allowing for a broader set of objectives. The second is that neoclassical theorists prefer framework policies and reject policies with elements of focus suggested by structuralist theory.

In Chapter 3, we agree with the other assessors in rating the IRDP and its predecessors as failures. Our criteria allow us, however, to focus on specific structural interactions and design characteristics as causes of failure. These causes are either ignored or downplayed by those using other theoretical perspectives.

We conclude that, in contrast to neoclassical theory's strong and exclusive recommendation in favour of framework policies, the ideal structuralist policy set must have the single aim of encouraging technological advances but should use multiple policies and programs to achieve that aim. Framework policies provide the general push. Focussed policies cover particular spots where market failures are large and specific. A few blanket policies can be cautiously applied when a relatively broad-based single need is identified and clearly communicated to the administrators. But before such middle-range policies are used, very careful study is needed — much fuller and more careful study than has typically gone into the design of past policies and programs that were often hastily put together in response to political pressures. Before spending millions of dollars on any new blanket policy, a few tens of thousands should be spent on clearly defining its goals, selection criteria and administrative structure. In principle, this advice is easier to follow than the neoclassical advice of searching out the optimum level of R&D and instituting neutral policies to achieve it. It may, however, be no less difficult to follow in practice.

1. TECHNOLOGY POLICY: BASIC CONCEPTS

This monograph evaluates a selection of government policies designed to influence technological change. Most technological change is initiated by private-sector agents. It includes the generation and commercialization of new technological knowledge, ranging from incremental improvements in existing technologies to the introduction of major new technologies. It also includes the diffusion of technological knowledge throughout the economy. Since most new technologies enter the world in crude form and are improved and changed as their use extends across firms, industries and different applications, the processes of diffusion and innovation are intertwined. Public policy can seek to influence technical change by attempting to either accelerate or retard it, using incentives or disincentives that are either applied generally or narrowly focussed. Policy can also seek to exert its influence indirectly by altering such structural variables as the concentration of industry, the amount of foreign investment, the location of firms and the education system. At different times and places, policies designed to influence technological change have been given various names and administered under different administrative structures. We cover all such policies under the generic heading "technology policies."

We first investigate the background and the rationale for technology policies. We then consider the policy advice that follows from two distinct economic paradigms: the neoclassical and what we call the structuralist-evolutionary. We argue that, although not always diametrically opposed, the two paradigms do lead to major conflicts in policy advice, and these conflicts can be resolved only by choosing between them. As a result, both policy assessment and program evaluation is strongly contingent on the economic theory with which one approaches the job. Neoclassical theory provides a precise set of objectives for policies and programs, and a means of assessing whether or not they have contributed gains. What we call structuralist-evolutionary theories start from different assumptions about the behaviour of the economy, and reach different conclusions about both the role for policy and the substance of program evaluation. These differences account for some striking divergences in views about the efficacy of various policies and programs — differences that we shall find to be critical in several of our later chapters. For example, the neoclassical model shows certain policies to be optimal while the structuralist model suggests that irreducible elements of judgment are required for all policy decisions — judgments over which reasonable people may simply have to agree to disagree. Furthermore, the neoclassical model leads to a preference for policies that give generalized support to all research and development (R&D) over policies that are more selective, while structuralist theories suggest a place for both generalized and selective policies. The two paradigms also suggest different criteria for assessing incrementality. We conclude this chapter with some discussion of how the various policies can be assessed, proposing criteria that can be used to assess programs and projects when direct information is not available on their achievements.

I. POLICIES, PROGRAMS AND PROJECTS

A. Definitions

A *policy* is some stated objective, which may be specific, such as developing a nuclear power industry, or general, such as encouraging technological change. A *program* defines the set of instruments and the administrative apparatus that give effect to the policy. For example, the program to develop the CANDU reactor was intended to give effect to the policy of developing a nuclear power capability in Canada. Finally, a *project* defines a specific task that is part of a program. An example is the construction of a CANDU power plant.

At the level of abstraction at which economists commonly operate, the distinctions between these three are not made and the general term *policy* is used. Our analysis requires that the distinctions be highlighted, both in assessing policies and in evaluating programs.

B. Types of Policy

We distinguish three major types of technology policy, as follows:

- *Framework policies* provide general support for one specific activity across all of the economy. In practice (and usually in principle) they are single-instrument policies. They do not discriminate between firms, industries or technologies. They do not judge the viability of recipient firms or the projects in which they are engaged. Instead, to be engaged in the covered activity is both a necessary and sufficient condition for obtaining benefits under the policy. Examples are patent protection for the owners of intellectual property and support for R&D (which includes R&D subsidies and tax credits).

- *Focussed policies* are designed to encourage the development of specific technologies such as nuclear power, products such as unmanned undersea craft, or particular types of R&D such as pre-commercial research. They are usually so narrowly focussed that falling within the focus is both a necessary and sufficient condition for receiving benefits under the policy.

- *Blanket policies* incorporate elements of both framework and focussed policies. On the one hand, they typically have broad-based objectives similar to framework polices. On the other hand, they typically use multiple instruments and have some form of assessment mechanism that enables the administrators to tailor the assistance they provide, at least to some degree. For example, assistance may be provided only to companies deemed to be financially viable or to projects deemed to have a good chance of commercial success.

Thus, being engaged in the covered activities is a necessary but not a sufficient condition for receiving benefits under the policy. Sufficient conditions vary with the policy and the instrument, but must be met to gain benefits. Sometimes these conditions are laid down quite explicitly by the rules and regulations of particular programs; sometimes substantial discretion is left to administrators in deciding whether a firm's specific activity fulfils the sufficient conditions. An example of a blanket policy is the Industrial and Regional Development Program (IRDP), which is discussed in Chapter 3. This program had the broad objective of encouraging both industrial and regional development. It had many instruments, such as cost-sharing grants on current and capital expenditures and support for consulting activities, including market analysis. Support under the program depended, among other things, on client firms being assessed as financially viable.

In practice, all of these policies have usually sought to influence innovation by changing the costs and benefits of R&D, but blanket policies have also sometimes been used in attempts to alter some of the economy's structural characteristics (what in this text we call the facilitating structure) and thus to influence innovation indirectly.

C. Assessing Policies and Evaluating Programs

Among other things, assessing a policy requires asking whether its objectives are acceptable, either because they are consistent with the prescriptions of a theoretical model or because they are regarded as desirable for any number of other reasons.

In evaluating any program, we first need to distinguish three stages, shown in Figure 1:

Figure 1
The Three Stages of a Program

"Design" in Figure 1 includes the rules of the game, the design of the delivery system, the institutional context and the relevant characteristics of the administrators, including technical and administrative expertise and mind set. Mind set is particularly important because policies that are identical in all other respects often produce different results depending on who administers them. It can matter, for example, whether a specific technology-enhancing policy is administered by a tax department, a science and technology department, a regional development department or a welfare department.

Much can be predicted about the operation of a program from knowledge of its design. However, since policy and program innovation is replete with uncertainties similar to those that affect technological innovation, one can never be sure how a new program design will work until it has been implemented.

During the early stages of a new program, its design and implementation are all there is to go on for an evaluation. Experience of past successes and failures of other programs can be a good guide to program evaluation at this stage. Once it has been in operation for some time, we would like to assess the program's performance. Has it been what was expected? Has it met a stringent standard of excellence or even optimality? Would other policies have achieved the desired results more efficiently?

II. Two generic models

In this section, we present two generic classes of models — the neoclassical, and what we call the structuralist-evolutionary — comparing and contrasting some of their key defining characteristics. The well-known neoclassical model yields a unique optimal equilibrium, departures from which are due to market failures. In contrast, structuralist-evolutionary models typically contain no unique equilibrium and thus cannot be used to derive a set of scientifically determined, welfare-maximizing policies. The choice between the two types of model is not merely a matter of taste and convenience. Instead it is a choice between two basic views of how the world works. Each of the models is based on formal assumptions appropriate to one of the two worldviews. As we shall see in the next section, each suggests some opposing views on appropriate policies and programs.

A. Neoclassical Theories

Analyses of market successes, market failures and the rationale for interventionist government policies are usually developed using neoclassical models of the type formalized by Arrow and Debreu. We consider six of the defining characteristics of such models.

1. Maximizing behaviour

All agents are assumed to maximize. This assumption requires, among other things, that all situations that depart from perfect foresight can be treated as *problems in risk*. Optimal decisions can be taken by scrutinizing all possible outcomes of any choice, assigning probabilities to each and then choosing the alternative that has the highest expected value associated with its outcome. Two individuals with the same endowments and tastes, faced with the same choice between two alternative courses of action and possessing the same full set of relevant information, must make the same choice.

2. Unique equilibrium

Standard neoclassical models feature a unique, competitive, welfare-maximizing equilibrium. The static equilibrium is characterized by constant tastes and technology. When technology is changing, the equilibrium concept is a dynamic, steady-state, optimal-growth path. In standard policy analysis, the many conditions needed to rule out the possibilities of non-existence of equilibrium, or of multiple equilibriums, are usually assumed to be fulfilled.

3. Technology kept behind the scenes

Typically, the details of technology are not explicitly modelled. Instead, the influence of technology is captured by the form of the relevant production functions, which determine the output flows resulting from given input flows. The hypothesis of maximization under conditions of risk implies that any new unit of capital expenditure — on any item of physical capital or on R&D — has the same marginal expected value no matter where it occurs. For this reason, a policy that encourages an extra unit of investment has the same payoff whatever form that investment takes and wherever it occurs. This powerful conclusion underlies much neoclassical policy advice.

4. Technological change seen only by its results

Because technology is not explicitly modelled, the process and the structure of technological change is observable only by its results. These may be seen in four ways. First, the form of the production function itself may alter, as in the case when a multiplicative constant increases in value. Second, the nature of the inputs themselves may alter. For example, in aggregate, neoclassical, Solow-type models of exogenous growth, technological change (no matter what its structure) has the effect of increasing the number of units of effective labour that is provided by each physical unit of labour input (e.g., each person-year). Third, the effects of technological change may be observed through the Solow residual, usually referred to as total factor productivity (TFP). In this case, the insertion of given inputs into a production function that predicted output in the past now causes it to underpredict actual output. Fourth, if physical capital is measured so as to include the value of new embodied technological change, while human capital is measured so as to include the value of disembodied new knowledge, the effect of technological change is observed not in the Solow residual but as larger measured inputs of physical and human capital.

5. Competition seen only as an end product

In equilibrium-based neoclassical models, competition as dynamic rivalrous behaviour is replaced by the type of competition that lies behind a competitive equilibrium. Competition then refers not to a process but to an end state. If the competition is “perfect,” the resulting unique equilibrium is optimal in the sense that no social improvement can be obtained by departing from it.[1] Departures from this optimal equilibrium are due to market failures, the removal of which fully defines the tasks for policy.

6. No explicit economic structure

Typical neoclassical models contain no explicit modelling of the economic structure or its institutions in the sense that they are defined later in this chapter (what we call the facilitating structure). Many neoclassical economists have been interested in institutions and have modelled such aspects of structure as the location of industry and the internal management of firms. Nevertheless, in the general-equilibrium, Arrow-Debreu-type models on which the neoclassical policy prescriptions that concern us are usually based, no attention is given to any of the characteristics of structure and institutions which experience, and our theory, suggest are important.

B. Structuralist-Evolutionary Theories

Theories that we call structuralist-evolutionary, or "structuralist" for short, are designed to make technology, structure and institutions explicit, and to study the *process* of growth-creating technological change. In these models, technological change is largely endogenous to the system in the sense that it responds to economic incentives.[2] The work of Nelson and Winter (1982) provides one of the major sources from which these models have sprung.

Although this class contains many distinct theories and models, most of them display the following six characteristics, which contrast sharply with those of the neoclassical model we have described. We devote more space to discussing these characteristics in the structuralist model because they are less well-known than their neoclassical counterparts.

1. Non-maximization

Structuralist theories typically abandon the neoclassical assumption of maximization under conditions of either certainty or risk. Structuralist modellers accept the evidence, amassed by students of technology, that uncertainty is pervasive in the process of endogenous technological change. Since innovation means doing something never done before, there is an element of genuine uncertainty in all innovative activity. It is often impossible even to enumerate in advance the full set of outcomes of a particular line of research. Time and money are often spent investigating specific research questions to discover whether the alley they lead up is blind or rich in potential. As a result, massive sums are sometimes spent with no positive results, while trivial expenditures sometimes produce results of great value. Furthermore, the search for one technological advance often produces different, unforeseen advances.

The existence of such uncertainties implies that agents will often be unable to assign probabilities to alternative future states in order to conduct risk analysis as conventionally defined. The assumption of

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[1] In a perfectly competitive optimum, there is no alteration that can make someone better off without simultaneously making someone worse off; all the possibilities for making everyone better off, either actually or potentially, are exhausted in any perfectly competitive equilibrium.

[2] In the macro growth literature, "endogenous growth" does not refer to the micro-economic behaviour that causes technology to change endogenously. Instead, it refers to an aggregate production function that has no less than constant returns to scale in the accumulating factors (whatever it may mean to have constant returns to the accumulation of knowledge).

rational maximizing behaviour is, therefore, replaced by an alternative assumption, such as groping in a purposeful, profit-seeking manner. Bounded rationality is often used to approximate these conditions. Whatever explicit theory of choice is used, the key implication of genuine uncertainty is that two individuals with the same endowments and tastes, faced with the same choice between two courses of action, and possessed of the same bounded set of relevant information, may make different choices. In effect, each is deciding to back different horses in a race with unknown odds. Under uncertainty, neither individual's choice can be said to be irrational.[3]

All of this applies to any actions designed to develop new technologies. For example, in making R&D expenditures directed at achieving a major technological advance, two similarly situated competing firms may decide to pursue different technological possibilities. Neither can be judged irrational in advance, although it will often be clear after the fact that one made a better judgment than the other.

2. No unique equilibrium

The assumption that firms seeking significant technological advances are groping in the face of uncertainty, rather than maximizing in the face of risk, has at least one serious implication: the absence of a unique, welfare-maximizing equilibrium. Some formulations of the resulting behaviour yield punctuated equilibriums: long, stable periods alternate with bursts of change, the timing and substance of which are not predictable in advance. Others yield multiple equilibriums, implying that historical accidents determine which equilibrium will be reached or approached at any one time. Still others yield no equilibrium but only perpetual change. In this case, although equilibrium theory is inapplicable, behaviour is still open to theoretical analysis seeking to understand the system's laws of motion.

3. Technology made explicit

A nation's technology is largely embodied in a complex set of interrelating capital goods.[4] In contrast to the neoclassical technological "black box," structuralist evolutionary theories explicitly take account of the detailed structure of technology. Below we mention some of the most important classes of technological interactions.

We start by defining a "main technology" as a physically distinct, stand-alone capital good that can be used to produce some useful output. When we look at each main technology, we see that complex linkages typically extend both inward and outward in fractal-like fashion.

Looking inward, we see that every main technology has differentiated parts with specific relationships between them, which we refer to as the technology's "make-up." For example, consider a computer. It is a main technology that is a compilation of a number of sub-technologies such as a motherboard, a semiconductor chip and special application cards such as Ethernet cards. It also requires a minimum set of software programs, a keyboard, a mouse and a monitor. Each of the sub-technologies can itself be broken down into constituent sub-sub-technologies, such as the material for the semiconductor (silicon)

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[3] Many structuralist-evolutionary theories also make use of the substantial body of evidence indicating that, even in well-defined situations involving only statistically measurable risk, agents do not maximize expected values but instead display "loss aversion" or "endowment effects." For discussion of these alternative motivations see, for example, Kahneman and Tversky (1979), Thaler (1980), Knetsch and Sinden (1987), and Tversky and Kahneman (1992).

[4] In Lipsey, Bekar and Carlaw (1998), we discuss the characteristics of technology in greater detail and give many more examples.

and the laser technology for etching it. Lower-level software languages are used to design higher-level, more user-friendly software. These lower-level languages require compilers to produce the higher-level language programs. The motherboard also requires its own special set of materials and components, including transistors, switches and circuits, which are in turn made of component parts. This fractal-like nature of the computer's make-up is typical of virtually all capital goods. It is also typical of consumer durables, such as automobiles and refrigerators, that deliver services for use in consumption.

Looking outward, we see that main technologies are typically grouped into "technology systems," which we define as a set of two or more main technologies that co-operate to produce a range of related goods or services. They co-operate within one firm, between firms within one industry, between firms in closely linked sets of industries, and across industries that are unrelated from an engineering point of view. Furthermore, these structures overlap in the sense that a subset of the technologies used to produce product *A* is used, along with other technologies, to produce product *B,* and so on. In some cases the same technology may be used in the process technology as well as the product that embodies it, as when computers are used to manufacture other computers, or machine tools to manufacture machine tools.

Thus, the economy's overall technology is a set of interlocking embodied technologies. There is the fractal-like web of sub-technologies below any one main technology. There is an external structure of interrelations between capital goods in one industry, as when several capital goods co-operate to produce a final product. There are interrelations between industries, as when the output of one industry is used as a capital good in another. There are interrelations between industries producing unrelated outputs when they use some of the same process technologies.

4. Technological change made explicit

Tastes and technology are the given exogenous variables in all neoclassical micro-economic theories, particularly the Arrow-Debreu type of general equilibrium theories, on which many policy conclusions are based. Thus, technological changes are unexplained events. Structuralist theories accept the large body of evidence showing that technological change is a largely endogenous response to economic signals. A change in any one element of the complex technological structure discussed in the previous section creates incentives that induce further changes in technology throughout the system. These result in the dynamic complementarities that we discuss below.

Endogenous technological change – The process of technological change is largely the result of endogenous economic activity, often rooted in inter-firm competition. An abundance of empirical evidence suggests that competition in both product and process technologies is critical in many industries.[5] Such competition drives endogenous technological change. In manufacturing and many modern service industries, failing to keep up with one's opponents in new technologies is far more serious than choosing a wrong price or an inappropriate capacity. Indeed, competition in technological advancement dominates inter-firm competition over both the middle and the long term.

In the explicit modelling of technological change, an important role is played by such sources of non-convexities as once-for-all sunk costs of developing and acquiring technological knowledge, positive feedbacks from current market success to further R&D efforts, and complementary relations between various technologies. In a model with uncertainty, the resulting non-linearities can give rise to path-dependent processes that may select any one of several available equilibriums or lead to no equilibrium at all (whereas if a unique equilibrium exists, only a limited role is played by these forces).

[5] See, for example, Porter (1990) and Dertouzos, Lester and Solow (1989).

Types of R&D – The popular idea of research and development is of white-coated scientists and engineers operating in well-equipped research laboratories. This formal model of R&D is not wide of the mark for many university and government research facilities, as well as some large firms with full-fledged research laboratories. Most small and many medium-sized enterprises (SMEs), however, do research in a less formal way. Indeed, micro-improvements in products and processes that aggregate to significant macro productivity increases are often performed on the job by unknown and unsung people who work with specific capital goods. Even when SMEs consciously set out to improve existing products and processes or to develop new ones, the research is typically much less formal than in a laboratory. There is often a continuing two-way flow of information between R&D and production, and learning by doing occurs at all stages. In such cases, the performing of R&D and the production of known products by means of known techniques become intermixed, with no clear dividing line between them. Failure to appreciate the informal nature of much R&D can lead to considerable confusion in the delivery of some R&D support programs.

Complementarities – The concept of complementarity refers to the response to a change. When dealing with technological changes, we need to distinguish between two types of complementarities, which we call Hicksian and technological. The Hicksian concepts of complementarity and substitutability in production refer to the signs of the quantity responses to a change in an input price, with technology being given (in the form of a fixed production function). A Hicksian complementarity occurs, for example, when an innovation lowers the cost of an input, and more is demanded not only of that input but also of other complementary inputs.

Now consider an innovation in one technology, the full benefit of which cannot be reaped until many of the other technologies that are linked to it are re-engineered and the make-up of the capital goods embodying them are altered. We refer to these as "technological complementarities," defined as existing whenever a technological change in one item of capital requires a (non-marginal) redesign or reorganization of some of the other items that co-operate with it in its internal make-up and/or in the technology systems of which it is a part. The complex nature of technology discussed in section II.B.3 gives rise to myriad technological complementarities, and the history of technological change is full of examples. Here we quote one illustration from Lipsey, Bekar and Carlaw (1998*a*):

> When the electric engine replaced the steam engine in particular uses, such as powering factories, it was necessary to abandon the old steam engines and their entire power delivery systems. Eventually the whole layout of the factory, as well as much ancillary equipment, was restructured to make best use of the new power unit — a series of changes in both the main technologies and technology systems within the plant. *These changes could not be modelled as a change in the price of power in a production function designed to reflect the technological requirements of steam. Even a zero price of steam power would not have led to the radical redesign of the plant which was the major source of efficiency gain under electricity (Schurr, 1990 and David, 1991) and which depended on the unit drive which attached an efficient power delivery system to each machine, something which was impossible under steam.*

One implication of these complementarities is that a change in any one technology will typically induce changes in the physical make-up and the economic value of many other technologies, as well as in existing R&D programs. In the case of what are now called "general-purpose technologies," such as electricity and the computer, the changes are observed to pervade the entire economy.[6] Any one agent can capture only a small proportion of the changes in the value of other technologies that result from changing its own technology. This, combined with the complex structure of the economy's technology, produces the massive externalities typically associated with major technological changes.

[6] See Lipsey, Bekar and Carlaw, "What Needs to Be Explained," in Helpman (1998*a*), ch. 2.

Furthermore, as we saw in the electricity example quoted above, *the effects of technological complementarities cannot be modelled as changes in the prices of flows of factor services used in an unchanged production function*. All of the action is taking place in the structure of capital and the consequent changes will typically take the form of new factors of production, new products and new production functions. Neoclassical economics has no operative analytical way of dealing with these structural complementarities, which dominate the evidence concerning technological change.[7]

One thing that matters about all this is that any one technological innovation will lead to non-marginal changes in many other technologies, including the redesign of many capital goods and production processes, as well as the reconfiguration of many technology systems.[8] Since these induced technological changes take place under uncertainty, there does not exist a unique optimal set of technological responses to the original technological change. It therefore follows that a policy change causing a significant technological innovation will also give rise to many technological complementarities. This in turn implies that technology-enhancing policies are not neutral; their effects will inevitably be context-specific. Furthermore, because there is no unique optimal response to the initial policy-induced technological change, there is no unique, optimal technology-enhancing policy.

5. Competition viewed as a process

For all the reasons mentioned above, evolutionary theory is Darwinian in overall conception (if not necessarily in detail). Its view of competition is Austrian in that its concern is with rivalrous *behaviour* as a process, rather than with the final equilibrium *outcome* of such behaviour.[9] It also models more than the small part of the economy in which demanders and suppliers are price takers. Instead, rivalrous behaviour as a competitive process extends to oligopoly and other market situations where firms have significant market power but are also in direct (often intense) rivalry with other firms.

6. Structure made explicit

Structure, including institutions, is often made explicit in structuralist models so that its place in the process of technological change can be studied. Here we use a refinement of our own version of what can be called a *structuralist decomposition*. We presented earlier versions of this decomposition in Lipsey and Bekar (1995) and Lipsey and Carlaw (1996). This is an appreciative theory.[10] Its elements are summarized and contrasted with the aggregate neoclassical model in Figure 2, near the end of this chapter.

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[7] Milgrom and Roberts (1990) have developed a mathematical technique that may do some of this job, but it is not operational for our purposes.

[8] Rosenberg (1982), Ch. 3, provides numerous examples.

[9] For a contrast of the neoclassical and evolutionary views of competition, see, for example, Blaug (1997).

[10] A formal theory is, or can be, laid out in mathematical form with assumptions and deductions. Its advantages include precision; its disadvantages include coverage restrictions necessary to formalize all assumptions. Appreciative theory covers assumptions and hypotheses that cannot easily be laid out in mathematical form. Rigorous analysis can still be used to reach conclusions and criticize them. Its advantages include a broader coverage of behavioural assumptions and factual observations; its disadvantages include some lack of formality and precision. History and philosophy are examples of disciplines that use appreciative theorizing extensively.

The theory separates technology from the capital goods that embody it, making the latter a part of what we call the economy's "facilitating structure." It also separately specifies public policies and the policy structures designed to give them effect. At any particular time, the facilitating structure, in combination with primary inputs, produces economic performance. The introduction of any important new technology, or a radical improvement in an old technology, induces complex changes in the whole of this facilitating structure. Ultimately, the performance of the economy is determined by, among other things, the compatibility of technology with the facilitating and policy structures.

Technology is an idea set defined as follows:

- *Product technologies* are the specifications of the products that can be produced, where products refer to both intermediate and final goods as well as services.
- *Process technologies* are the specifications of the processes that are, or currently could be, employed to produce these goods and services.[11]

The stock of existing technological knowledge (both applied and fundamental) resides in firms, universities, government research laboratories, and similar production and research institutions, as well as in physical capital. To produce economic results, technology must be embodied in physical capital that workers are able to operate efficiently.

We saw earlier that each stand-alone main technology has a specific internal make-up of sub-technologies and sub-sub-technologies, while sets of main technologies co-operate in complex and overlapping technology systems. Furthermore, various technologies range in purpose, from very specific single-use technologies at one extreme to widely used general-purpose technologies at the other. Technologies tend to evolve over time along trajectories that increase their efficiency and their range of application.

The *facilitating structure* is the realization of technology and has the following elements:

- all physical capital;
- the human capital that is embodied in people;
- the organization of production facilities, including labour practices;
- the managerial and financial organization of firms;
- the physical location of industries;
- industrial concentration;
- all infrastructure;
- private financial institutions, and financial instruments.

The facilitating structure is the embodiment of technological and organizational knowledge. It is altered by decisions taken by agents in both the private and public sectors.

Public policy is the idea set covering the specification of the objectives of public policy as expressed in legislation, rules, regulations, procedures and precedents, as well as the specification of the means of achieving these objectives as expressed in the design and command structure of public-sector institutions, from the police force to government departments and international bodies. Public policy is made and changed by public-sector agents such as legislatures and courts.

The *policy structure* is the realization set that provides the means of achieving public policies. It is embodied in public-sector institutions and also includes the humans who staff these organizations and whose human capital embodies the knowledge related to the design and operation of public-sector

[11] Both product and process technologies include those directed at producing research results.

institutions (institutional competence). Note that the policy structure is parallel to technology and its embodiment in the relation between public policy and the facilitating structure.

Primary inputs of labour and raw materials are fed through the structure to produce the system's *economic performance*. This includes:

- aggregate gross domestic product (GDP), its growth rate, and its breakdown between sectors and between such broadly defined groupings as goods production and service production;
- gross national product (GNP) and its distribution between size and functional classes; and
- total employment and unemployment, and their distribution between such sub-groups as sectors and skill classes.

Economic performance is determined by the interaction between inputs and the existing facilitating structure. That structure is in turn influenced by technology and public policy. It follows that changes in technology typically have no effect on performance until they are embodied in the facilitating structure. Furthermore, the full effects on performance will not be felt until all the elements of the structure have been adjusted to fit the newly embodied technology.

The various elements of this model are interrelated, which implies many causal linkages between them. One of the most important of these runs from technology through structure to performance. A change in technology requires a change in structure to make it fully operational, and economic performance typically continues to change even after the new technology is in place as long as the structure is still adapting to it. As the discussion later in this chapter reveals, however, there are other relations between the elements shown in Figure 2. Each of these creates its own externalities and potentials for market failure, which are studied below.

III. POLICY THROUGH NEOCLASSICAL LENSES

We have already observed that, in the neoclassical model, any possible outcome can be given a probability distribution with a well-defined expected value. Maximizing agents equate the expected returns from a marginal expenditure everywhere in the economy, including all lines of R&D. Given all the other standard assumptions, a unique welfare-maximizing equilibrium exists. Departures from this equilibrium are caused by market failures, which take two general forms: externalities and non-convexities.

A. The Neoclassical Justification for Technology Policies

Market failures – We use the terms *externalities* and *spillovers* synonymously. They are non-priced effects of any economic activity that are felt by parties not directly participating in the activity. Following Romer, we define a two-by-two matrix by rivalrous/non-rivalrous and appropriable/non- appropriable.[12] Ordinary economic goods are rivalrous and appropriable. Pure public goods are non-rivalrous and non-appropriable. Common property resources are rivalrous but non-appropriable. Knowledge (created by R&D or by another process) is non-rivalrous and (at least partially) appropriable. Knowledge can, for

[12] A good is rivalrous if someone's use of it precludes anyone else's simultaneous use of it, and non-rivalrous if someone's use does not diminish the ability of anyone else to make use of it. A good is appropriable if its use can be controlled by agents, and non-appropriable if its use cannot be controlled.

example, be appropriated either through secrecy or through a property right conferred by an instrument such as a patent. Because knowledge is non-rivalrous, its creation typically confers externalities on those who did not create it.

Innovation is replete with non-convexities. R&D directed to a specific objective is a sunk cost that need not be repeated once the objective is achieved. Learning how to embody any piece of technology in a working piece of equipment is also a sunk cost, as is the acquiring of tacit knowledge about how to operate the equipment effectively. New technologies do not enter the world fully developed but instead go through a long evolutionary process, being improved as they diffuse through the economy. At each stage, however, the R&D and the accumulated experience do not need to be repeated to hold the technology at that level. Past R&D is thus a sunk cost of bringing the technology to its current level.

The neoclassical formulation – The neoclassical theoretical literature on the specific issue of technology spillovers usually takes Arrow as its starting point. On the basis of indivisibilities, appropriability and uncertainty, Arrow (1962*a*) argues that a positive spillover results from any new technological knowledge. Since R&D is the source of much new knowledge, the social return to R&D greatly exceeds the private return.

Arrow's basic insight has been employed in a number of models of production externalities. Griliches (1992) surveys the literature that attempts to measure R&D spillovers at the levels of the firm, industry and aggregate economy. He starts with Simon (1947) and progresses through Arrow (1962*b*) up to the present endogenous growth formulations. In the aggregate versions, knowledge is modelled as a homogeneous stock that can be added to incrementally. Knowledge externalities arise because one firm's productivity depends not only on its own R&D effort but also on the stock of general knowledge available to it. New knowledge is implicitly assumed to diffuse instantaneously and costlessly because the stock of knowledge is treated as just another input in the aggregate production function. This then displays increasing returns to scale. An example of how such model is employed to measure R&D spillovers is provided by Cabellero and Lyons (1991).

In such models, we can imagine a perfect patent system that gives each person a complete and enforceable property right to the knowledge that he or she creates. If we add the assumption of zero transaction costs, each creator of knowledge would act as a perfectly discriminating monopolist. Both the use of existing knowledge and the value of the resources devoted to knowledge creation would then be optimal.

In reality, property rights to knowledge are highly incomplete, leading to the prediction that knowledge-creating activities, such as R&D, will be below their optimal amount. It would, therefore, be welfare-enhancing to encourage these activities through public policy. Two instruments are often recommended. The first is more embracing and more enforceable patents that give more return to inventors and innovators. The second is direct support for R&D in the form of subsidies or tax relief. Notice that in the neoclassical model, in which the expected payoffs from all lines of R&D expenditure are equated at the margin, there is no distinction between the inputs into the advancement of technological knowledge and the output of the new technological knowledge. Increasing one increases the other. The policy prescription therefore does not differentiate between lowering the costs of generating new technological knowledge and raising the payoff to that knowledge.

Types of policy – In the aggregate neoclassical formulation, there is only one body of technological knowledge, with one private and social marginal product. In such a world there is no distinction between framework and focussed policies. To compare these, we need to disaggregate. When we do this we find that, if there are no externalities or other sources of market failure, the unaided price system yields an optimum allocation of resources between all lines of production, including the creation of knowledge through R&D. If the only source of externalities is the non-rivalrous nature of knowledge, R&D will be below its optimal rate and policies to encourage it will be potentially welfare-increasing. If the externalities are uniform across all lines of R&D, a generalized R&D subsidy is appropriate and, in principle, can restore a first-best optimum. In contrast, focussed policies (e.g., the support of research into the next generation of long-distance passenger aircraft) or blanket policies (e.g., special support for R&D undertaken by small firms) are judged to be non-optimal. The reason is that they selectively distort the price and profit signals that are generated by a perfectly competitive market. Although such policies may sometimes yield a positive net benefit, more benefit can always be achieved by devoting the same expenditure under a non-distorting framework policy.

B. Additionality/Incrementality

Everyone who evaluates policies and programs has to worry about what is called incrementality in North America and additionality in Europe.[13] Although we consider the issue at this point, what we say is also pertinent to our discussion of the structuralist approach to technology policy.

If an expenditure of government funds leads to no alteration in targeted behaviour, the funds have clearly been wasted. Assume, for example, that firms are given a subsidy to locate in a depressed area in order to increase employment there. If the only firms that accept the money are those that would have gone anyway, and they do exactly what they would have done without the subsidy, the money has been wasted. If, however, some firms go to the depressed area because of the subsidy, and/or others that would have gone anyway build larger facilities because of the subsidy, what we call "the broad test of incrementality" is passed. Something that the policy makers are trying to do has happened as a result of their expenditure of funds. This discussion illustrates that, in assessing broad incrementality, we need to know the aims of the policy and to be able to measure the relevant results. The neoclassical and the structuralist theories differ, not in their acceptance of broad incrementality as a necessary condition for success, but in the policy objectives that they recognize and hence the scope of the incrementality test that they apply.

Structuralists use the broad test because they accept a policy as having incremental effects even if it causes no direct change in technology, as long as it causes a targeted change in the facilitating structure that indirectly encourages technological change. For example, an R&D subsidy may be used as the carrot to induce firms to create research laboratories or to establish closer links with government and university research laboratories.

Those using the neoclassical approach typically confine the objectives of technology policy to achieving a targeted technological change (either in a specific technology or in the general rate of technical change). This approach leads to what we call the "narrow test of incrementality"; when applied to technology policy, this test requires that *a technology be developed or installed that would not have been developed or installed in the absence of the policy or program under consideration*. No other changes in structure are permitted to enter as policy objectives. (See, for example, Tarasofsky, 1984, and the Economic Council of Canada, 1983.)

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[13] The use of the term *incrementality* to describe the results of some technology policy is unfortunate because the same term is used to distinguish incremental from radical technical innovations. Incremental innovations are technologies that evolve out of their predecessors, while radical innovations are technologies that could not have done so. For example, the steam engine was a radical innovation that could not have evolved out of the water wheel that it eventually displaced, while the particular type of high-pressure steam engine that was placed in iron ships in the mid-19th century evolved incrementally out of Watt's original steam engine.

Using the example of an R&D subsidy, Tarasofsky suggests a quantification of narrow incrementality in the following way:

$$R = a_0 + \Sigma a_i V_i + a_g G$$

where R is the firm's expenditure on R&D that is financed from non-government sources, V_i is the vector of variables that determine the firm's R&D, a_i is a vector of corresponding coefficients, and G is the government's subsidy (or other program expenditure) meant to encourage the firm's R&D. There is no incrementality if the firm merely reduces R by one dollar for every one dollar of G. In this case, $a_g = -1$. If the firm undertakes no new expenditure from its own resources but uses the government funds, a_g is zero and incrementality equals the government expenditure. If the government program induces the firm to spend more of its own money, a_g exceeds zero. The larger is a_g, the greater is the incrementality. Many neoclassical versions are stricter than this narrow version. What we call "the test of ideal incrementality" is that *the policy be in some sense not simply a good use of government money but an optimal allocation of a given government expenditure*. Usher (1983) provides a four-part definition of what he calls an optimally incremental R&D subsidy:

- The government's contribution must be only that percentage of the costs of the project that *must* be provided to the client firm in order to induce it to undertake the targeted level of R&D investment.
- The project undertaken must be the least costly way to produce the desired level of R&D investment (i.e., there cannot be another firm that could undertake the action more cheaply, and the subsidy provided must be the minimum necessary to induce the client firm to undertake the activity).
- The subsidy must be less than the anticipated net social benefits from generating the action.
- The measure of the subsidy must include the transactions costs, dead weight losses and other "leakages" that occur when the government intervenes in the market.

The third and fourth points amount to the condition that the total costs of the government intervention (discounted appropriately) must be less than, or equal to, the total expected benefits (also discounted appropriately). Both Tarasofsky and Usher provide some quantitative estimates of these costs as a fraction of a dollar of subsidy spent. These range from a factor of 15% up to 100% or more. Hence the total cost of intervention, in their estimation, can be more than double the amount of subsidy given.

It is difficult if not impossible to measure the direct and full spillover effects of technological change that are needed to apply Usher's test of incrementality. These difficulties are well known to practitioners in this field and are implicit in our sections II.B.3 and II.B.4 above. In their Program Evaluation Assessment Report of the Industrial Research Assistance Program (IRAP), which is one of the programs that we study in this monograph, Siddiqi et al. (1983) state:

> Measuring ... benefits presents [a] considerable challenge. The benefits at the level of the individual firm can be specified: for example, new sales, increased qualification and numbers of research and other technical personal. However transforming these into appropriate monetary terms ... can be very difficult. Moreover, there can also be a distinct problem of attribution: how much of the change is really due to the program? The problems of measurement and attribution are even greater when one moves to the more general level of social and economic benefits to Canada, such as increases in employment, overall increases in technological capacity and ability to compete internationally. This is further complicated by the fact that national benefits are seldom simply the aggregate of individual benefits, because some individual benefits are at the expense of other individuals or firms. (p. 10)

According to Usher, framework policies are superior to focussed or blanket policies. His argument is in three steps. First, the uncertainties that surround the process of technological change make it impossible to calculate the social value of externalities. Second, the cost of focussed and blanket policies always exceed the cost of framework policies. Third, a framework policy can accomplish any objective of a focussed or blanket policy.

The details of our structuralist rejection of this view can be found in the conclusion of Chapter 4. Since the definitions introduced in this section are used throughout subsequent chapters, we summarize them here for easy reference:

- **The broad test of incrementality –** The policy or program results in a change in technology, or in the facilitating structure, that policy makers wish to occur and that would not have occurred otherwise.
- **The narrow test of incrementality –** The program or policy results in a change in R&D expenditure, and/or in existing technology, that policy makers wish to occur and that would not have occurred otherwise.
- **The test of ideal incrementality –** The narrow test is met *and* the government's expenditure is no larger than the minimum required, and no other line of expenditure (on another firm or project) would have achieved greater benefits.

Proxies for incrementality – Because programs that fail the broad test of incrementality are clearly undesirable, both internal and external program evaluators place great stress on incrementality. Measuring incrementality between different types of programs is not, however, an easy matter. With framework policies such as R&D tax credits, the issue is whether the national aggregate of R&D has been increased. With a particular project that is part of a focussed or blanket policy, the issue is often whether the project would have proceeded in the absence of the government support. A more difficult issue arises when the project would have proceeded without government support but would have been carried out differently. Perhaps less money would have been spent on it; perhaps the research would have taken a different line of attack; perhaps the client firm would not have engaged in a number of co-operative ventures or established linkages that were facilitated by the program.

When all that matters is the total amount spent on a project, Tarasofsky's measure is the correct one. But it is not an easy matter to tell what the firms would have spent on the project in the absence of government assistance. Accordingly, in practice the evaluators often fall back on a ratio of the amount contributed by the government to the amount spent by the firm and raised from other sources. However, this ratio bears no necessary relation to Tarasofsky's incrementality. For example, consider two firms that planned to spend $1 million and $2 million respectively on their proposed projects in the absence of government assistance. Now assume that they each receive $500,000 of assistance and that, in addition, the first firm spends $1 million of its own money and the second firm $1.5 million. The second firm has a higher ratio of own-to-government funding (3:1) than the first (2:1) but the first has a higher Tarasofsky incrementality measure (0) than the second (-1).

Another proxy (used, for example, by Atkinson and Powers in a paper that we discuss in Chapter 3) requires a minimum amount of government expenditure on the grounds that less would be insufficient to produce significant changes in behaviour. Apart from the problem of deciding how little is too little, it is important to note that there is no necessary relation between the size of the government's contribution and either the project's externalities or the amount of private expenditure that may be induced. For simplicity's sake, assume that there are no externalities on the cost side and that the government pays exactly the difference between the private gain and the private cost of a project. The *cost to the government* is then the difference between private cost and private benefit, while the *net social gain* is the difference between the total social benefit and the total cost, which is the government's contribution plus the firm's costs. The cost to the government and the net social gain need not be related. For example, a small government payment may make possible a project where the social benefits greatly exceed the private ones (i.e., a project with large externalities), whereas a large government payment may be necessary to make possible another project for which the social benefits are only slightly larger than the government's costs (i.e., a project with small externalities only slightly in excess of the government's contribution).

When examining whether projects would have been undertaken at all in the absence of assistance, evaluators of incrementality often fall back on a measure of risk. They argue that low-risk projects would probably have been undertaken anyway and are therefore unlikely to be incremental, while high-risk projects probably would not have been undertaken and so are likely to be incremental. If a program's incrementality is judged to be low, the evaluators often urge the administrators to take on more risky projects in order to increase their incrementality. Aside from the fact that this indicator ignores all the other questions of how different projects would have looked with and without assistance, it is erroneous to think that risk is necessarily related to the type of incrementality that matters. Think, for instance, of the extreme case in which all projects are of a type that no one in his or her right mind would have undertaken in the absence of funding, and they all fail. This is not necessarily irrational on the part of the private-sector clients if, before the project fails, they manage to take out of it, by way of salaries and other emoluments, more than they put in. The assistance has now been 100% incremental in the sense that it finances projects that would not otherwise have happened. But it is not money well spent. Incrementality thus has to be related to making something happen *that is in some defined sense desirable*.[14] The confusion of risk with incrementality seems to have caused evaluators of more than one program to urge the administrators to push risky programs that were beyond the capability of the clients (often small firms) to manage successfully.

IV. POLICY THROUGH STRUCTURALIST LENSES

We accept that neoclassical theory and risk analysis is valuable in situations for which equilibrium theory is relevant. Indeed, for the great majority of questions that are put to economists as policy advisers, it is appropriate for them to reach for the neoclassical tool kit. This is not so, however, for situations in which endogenous technological change plays a key part. Structuralist theories are designed to deal with such non-equilibrium, evolutionary, dynamic situations.

A. The Structuralist Justification for Technology Policies

Whatever theory one uses, the case for an active technology policy requires accepting the proposition that it is socially desirable to accelerate the rate of technological change. We have seen that this proposition can be derived from the neoclassical model, given all of its characteristics, plus the empirical assumption that there exist positive externalities associated with the creation of new, non-rivalrous technological knowledge. In this section, we see why structuralists believe that an optimal R&D policy cannot be derived and why the decision to encourage R&D beyond what the market would provide must depend in the final analysis on a large measure of judgment. The argument has three steps, as follows.

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[14] This may be what Usher is driving at in his ideal measure of incrementality. We agree with the drift of his measure while rejecting the view that projects that do not pass his ideal measure are not worth doing.

1. Knowledge externalities

Structuralists accept that the non-rivalrous nature of technological knowledge creates beneficial externalities. Indeed, because of the complex set of complementarities analysed in the structuralist model and discussed in sections II.B.3 and II.B.4 above, the inventor/innovator of any fundamental new technological idea is unlikely to be able to appropriate more than a tiny fraction of the total social benefits flowing from his or her idea. Therefore, there is a case for encouraging R&D beyond the level that the free market would provide.

However, once we disaggregate below the level of the aggregate neoclassical model, important complexities arise. In particular, we see that the externalities resulting from the creation of new technological knowledge are not evenly spread across all lines of R&D. The private returns to different lines of R&D vary, as does the ability of firms to internalize those payoffs. (The aggregation implicit in the model suppresses these differences by condensing all knowledge into a single measure with one associated externality.) Disaggregation thus reveals a complex second-best problem. If policy makers had full information, and there were no other market failures, an optimal policy in the neoclassical world would provide a different amount of *focussed support* to each different line of R&D activity, and the support would vary with the externalities associated with each line. Since this is impossible, second-best theory shows that there is no general presumption in favour of equal support for all lines of activity over support that varies across activities.

2. No optimal level of R&D

The above type of criticism can be made from within the neoclassical model and requires merely that R&D be disaggregated to reveal different externalities associated with different lines of activity. The second type of criticism goes beyond the neoclassical model to reject the proposition that the market would *necessarily* produce too little R&D even if its externalities were equal in all lines of activity. We observed in earlier sections that there is no well-defined optimum allocation of resources when technology is changing endogenously.[15] Structuralist theories incorporating this fact have the following important implication:

> *Because there is no unique optimum allocation of resources when technology is changing endogenously under conditions of uncertainty, there does not exist a set of scientifically determined, optimum public policies with respect to technological change in general and R&D in particular.*

Even if such an optimum set did exist, we do not know whether agents would produce too much or too little R&D, given that they are making decisions under uncertainty about lumpy investment with lumpy potential payoffs. The market economy encourages innovation by giving rewards to successful innovators, and huge rewards to the really successful, while the unsuccessful suffer losses. There is no existing theory of choice that allows us to predict how agents will react to such uncertain and lumpy possibilities, where there are significant differences in both the ex post and the ex ante payoffs to R&D undertaken by different entrepreneurs.

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[15] Lipsey (1994) analyses in more detail why endogenous technological change at the micro level destroys the concept of an optimum allocation of resources.

3. Policy judgment

Accepting this conclusion has important consequences for how we view economic policy in the area of growth and technological change:

> *If there are no unique optimum rates of R&D, innovation or technological change, policy with respect to these matters must be based on a mixture of theory, measurement and subjective judgment.*

The need for judgment does not arise simply because we have imperfect measurements of the variables that our theory shows to be important, but because of the very nature of the uncertain world in which we live. Although a radical idea with respect to micro-economic policy, the point that policy requires an unavoidable component of subjective judgment is commonly accepted with respect to monetary policy. For two decades from the mid-1950s to the mid-1970s, Milton Friedman tried to remove all judgment from the practice of central banking by making it completely rules-based. When his advice was followed by several of the world's central banks, the monetary rule proved ineffective in determining policy, as many of his critics had predicted it would. Today, the practice of central banking is no different from the practice of most economic policy: it is guided by theoretical concepts; it is enlightened by many types of empirical evidence, studied for the information that each provides; and, in the end, all of these are inputs into the judgment calls that central bankers cannot avoid making.

Because most economists were trained in neoclassical welfare economics, many are unwilling to accept that some micro-economic policy decisions depend on significant amounts of subjective judgment rather than solely on scientific analysis. For obvious reasons, many economists prefer models that provide precise policy recommendations, even in situations in which the models are obviously inapplicable to the world of our experience. Our own view is that, rather than using neoclassical models that give precise answers that do not apply to situations in which technology is evolving endogenously, it is better to face the reality that there is no optimal policy with respect to technological change. In the world described by structuralist models, dynamic efficiency is as inapplicable a concept as is static efficiency.

In rejecting the neoclassical argument, we are not dismissing the possibility that it would be socially desirable to accelerate the rate of technological change. Most economists (the present authors included) believe that innovation and economic growth improve human welfare on average, and that innovation is therefore correctly judged to be socially valuable. Virtually all governments can also be seen to take that view, as is revealed by their own choices on technology policies. But what we cannot do is determine that the current amount of innovation is too much or too little by comparing the actual amount against a criterion of optimality. Where, then, does this leave us?

First, along with most policy makers and neoclassical economists, we make the judgment that policy should seek to increase the amount of resources allocated to creating technologies for future use. Second, unlike neoclassical economists, we hold that there is no unique optimal amount of resources to re-allocate into R&D in general, and into each line of R&D in particular. Nonetheless, we make the judgment that, compared with laissez-faire, the potential payoff from re-allocating resources to increased R&D exceeds any potential losses from statically misallocating those resources.

4. The concept of market failure

The desire to encourage technological advances through public policy can be thought of as a response to a *market failure*. There is no need to ban this concept from structuralist-evolutionary theories. In neoclassical theory, the market fails when it does not achieve the unique optimal equilibrium; in structuralist-evolutionary theory, it fails when it does not lead to a desired and attainable state.

B. Structuralist Roles for Policy

Showing that a policy thought to have a scientific basis must depend on an irreducible element of judgment is not an insignificant accomplishment. If that was all that structuralist theory accomplished, however, it might not be of great practical value. Fortunately, structuralist decompositions also shed much light on ways of accomplishing the ultimate goal of accelerating technological change. These ways sometimes supplement, and sometimes differ from, those suggested by neoclassical theory.

First, as we have earlier noted, technological interrelationships that cause positive externalities are extremely rich, making most externalities context-dependent both sectorally and temporally. A structuralist decomposition reinforces this view and reveals an additional set of externalities associated with the relation between technology and the facilitating structure. Second, there is a major trade-off between innovation and diffusion, complicating technology policy. Third, there are many roles for technology policy beyond internalizing spillovers. Fourth, institutional competence to administer policies and programs becomes a much more complex issue than it is in neoclassical principal-agent analysis. These issues are considered separately in the four sections that follow.[16]

1. Specific externalities

An important way in which structuralist theories assist in motivating and directing innovation policies is by identifying a much more complex set of spillovers than is found in neoclassical theory. The classes of spillovers suggested by structuralist theories cover spillovers (i) between technology, facilitating structure and performance, (ii) within technology, and (iii) within the facilitating structure. (Because economic performance is defined as the final outcome of economic activity, there are no spillovers within performance.) A detailed knowledge of these spillovers suggests policy opportunities that tend to be ignored by the neoclassical model. Many of the specific policy lessons that we use later are related to these spillovers which create opportunities for useful policy interventions and pitfalls for policies that ignore them.

Spillovers within technology – As we observed earlier, developments that improve the efficiency of one technology are often useful in many other technologies. Such was the case, for example, in the 19th century, when improvements in machine tools used in very specific applications turned out to have wider application to the machine tools used in other industries (Rosenberg, 1976). The value of many of these indirect effects cannot be appropriated by their initiators, thus giving rise to inter-technology spillovers. This situation creates a potential role for policy, which we consider in detail below.

Spillovers between technology and structure – A change in any element of technology typically affects the values of many elements of the structure. Spillovers arise because innovators do not usually take account of the structural effects that they induce. A new technology will typically affect the values of most elements of the facilitating structure, such as existing capital goods, whole firms, contracts, locations and items of the infrastructure. Changes to the facilitating structure will in turn affect the values of many other existing technologies and R&D programs. The potential roles for policy are obvious.

Spillovers within facilitating structures – The facilitating structure is composed of a set of interrelated elements. A change in one of them affects the value and efficiency of many others. The externalities arise because agents who change those elements of structure that are under their control typically do not

[16] In the case of Industry Canada, Sulzenko (1997) discusses a major restructuring that is much in line with this structuralist perspective and in which he played a major part as Assistant Deputy Minister for Industry and Science Policy.

take account of induced changes in the values of other elements. For example, before changes in the nature of physical capital can achieve their full potential, they often require changes in human capital, in the physical location and organization of firms, and in the infrastructure. The policy implication is that there is a potential role for government to assist the full adjustment of that structure where private incentives are lacking.

Spillovers from performance to structure and technology – Experience with the use of evolving technologies often alters the values of elements of the existing technology and/or structure. The Schumpeterian model of innovation saw technology developing in a one-way flow moving from pure science to applied work, and then to the shop floor and the salesroom. Modern research shows a two-way flow of information running backward and forward between every stage of the value added chain. For example, von Hipple (1988) shows that some innovations are derived from the initiative of producers, some from downstream users and some from upstream suppliers. New technologies typically have many imperfections that can be identified only through "learning by using," causing users to face significant amounts of uncertainty (Rosenberg, 1982, Ch. 6). Spillovers occur because the experience of the new users often generates non-appropriable new knowledge that benefits producers and, through product improvements, other users. The two obvious places where policy has the potential to assist are in solving the problem of co-ordination between producers and users, and in inducing users to create this knowledge.

2. Innovation and diffusion

Is there an optimum policy to encourage both innovation and diffusion?

Consider, first, a Schumpeterian world in which innovations introduce new stand-alone technologies, which then diffuse through the economy in unchanged form. If R&D is a continuous activity with diminishing marginal returns, there is an optimal policy. It is to pay the innovator a lump sum equal to the social value of the innovation, which includes the opportunity cost of the resources used plus any surplus due to the new idea. The idea should then be made available for diffusion at zero cost.

As we have already observed, however, major radical innovations never bring new technologies into the world in a fully developed form. Instead, these technologies first appear in a crude embryonic state with only a few specific uses. Improvements and diffusion then occur simultaneously as the technology is made more efficient and adapted for use over an increasingly wide range of applications through a series of complementary innovations. The more fundamental is the new technology, including what the literature calls "general-purpose technologies," the more marked is this process of long and slow evolution from crude prototypes with narrow use, to highly efficient products with a vast range of applications.

This whole process is usually called "diffusion" because an original generic idea, such as how to generate electricity, how to drive a cylinder with steam, or the integrated circuit, is diffusing through the economy. However, the process bears no relation to diffusion defined as the use of an unchanged piece of knowledge by more and more agents.

For the simplest illustration of these complex interrelationships, assume a new technology, A, that is combined with a second new technology, B, to produce a single final output. The two technologies are fully complementary with each other. Together, they produce a stream of final value; separately, they produce nothing. Their externalities are reciprocal. There is no way that any joint surplus over the opportunity costs of the resources used to create them can be separated into the surpluses contributed by each. If there were no other considerations, it might be optimal to combine the two activities into one firm to remove the need to separate the surplus. But this is not typically the case. Technologies A and B may be developed by different firms with different expertise and different acquired pools of knowledge. In this case, the division of the joint surplus between the two firms is arbitrary.

Furthermore, there is enormous uncertainty with respect to the range of applications that a new technology may have. The steam engine, electricity, the telephone, the radio, the laser, the computer, the VCR, and fibre optics are examples of technologies that were initially thought to have very limited potential, and that did have very limited actual applications during the first decades. Because of this uncertainty, the social value that any new technology will generate directly and indirectly cannot be determined. Until the lives of a given technology, and of all subsequent technologies that would not have been developed in its absence, have run their full course, the social value of that technology cannot be known, even in principle. What, for example, will be the full social value of electricity, which, among other things, is enabling virtually all of the modern developments in information and communications technologies.

Sophisticated neoclassical treatments can deal formally with all but the last point. However, the versions of the neoclassical models that have been used to study externalities, innovation and diffusion, both in theory and in practice, typically depend on levels of aggregation that eliminate these effects. So we conclude that, in contrast to the aggregate models that are typically used, our structuralist theory produces no optimal policy to allocate resources to innovation and diffusion.

3. Roles beyond internalizing spillovers from technological knowledge

Structuralist models emphasize several features that provide scope for technology-enhancing policies, in addition to internalizing the spillovers that arise from new technological knowledge.

Induced changes in structure – Changes in technology typically require accommodating changes in the facilitating and policy structures. Public policy can respond helpfully in two ways. First, relevant elements of the policy structure can be altered. An example is the regulation of the telecommunications industry after the information and communications technologies (ICT) revolution. In this case, changes are coming all too slowly in Canada and many other countries. Second, public policy can assist adjustments in the facilitating structure that are subject to major externalities; examples are altering the education system to produce skills required by the new technology, or altering elements in the publicly owned infrastructure to make best use of new technologies. (Of course, policy can and often does respond in the unhelpful way of slowing down the necessary adjustments to the structure. This can be done by errors of omission as well as commission.)

Proactive changes in structure – Policies may also indirectly target technological change by altering elements of the facilitating structure. Examples of such policies include attempts to integrate some university, government and private-sector research activities, attempts to create technology information networks, and attempts to change private-sector attitudes toward adopting new or different technologies. Furthermore, a government can attach structural conditions to funds it gives firms to develop technologies that they would have developed anyway. This has been done by more than one government to encourage the development of long-term research facilities. All of these initiatives would fail the narrow incrementality test that measures only direct changes in specified technologies. They would, however, pass the broad incrementality test, which considers structural alterations that would not have happened without the government pressure. A prime example is U.S. military procurement policy; to a great extent, this created the U.S. software industry, and then developed and imposed consistent standards on it (see Lipsey and Carlaw, 1996, p. 311).

Sunk costs – We saw in section II.B.4 that sunk costs and path-dependent technological trajectories play a prominent role in structuralist theories. Sunk costs are important for the development of new products and processes; they are equally important for the acquiring of codifiable knowledge about new technologies, as well as tacit knowledge about how to operate given technologies. One major policy implication is that government bodies can efficiently disseminate technological knowledge by operating on a scale that makes the sunk costs bearable, or even trivial, where they would otherwise be prohibitively high for small firms.

Path dependence – The fact that technologies evolve along path-dependent trajectories suggests that the encouragement of generic technologies in their early stages of development is more likely to produce socially valuable externalities than the encouragement of highly specialized technologies at later stages in their development.[17] However, as Paul David has emphasized, the early stage of many technological trajectories (when government assistance can have most impact) is the point at which exposure to uncertainty is widespread. What looks like a sure winner, such as lighter-than-air craft, hovercraft or atomic energy, may turn out later to have totally unforeseen problems that severely limit commercial success.

One important lesson is that policy opportunities vary over the course of a particular technology's development. Expectations of large spinoffs from a new generic technology must be balanced against the many uncertainties inherent in its early development. Assistance is often best applied after it becomes clear that the technology has major potential but while it is still in a relatively generic state.[18]

A cautionary lesson is suggested by the theory and evidence on competition between firms working on the same technological trajectory. Procurement decisions may lock the economy into one version of the competing technology before the relative merits of the alternatives have been seriously explored (an issue that would not matter if everything was reversible, as in neoclassical theory). Arthur (1988) gives several examples in which this appears to have happened.

4. Institutional competence

The neoclassical model yields optimal policies that do not depend on any specific institutional structure. In reality, as emphasized in structural approaches, various public-sector institutions have different institutional capabilities. The behavioural differences are based partly on constitutional differences, partly on the different power relations between various special interest groups, partly on the differences in the quality of recruits and the subsequent training of civil servants, and partly on differences in accumulated learning by doing in operating each country's specific policy instruments.

The issue here is analogous to that of the difference between technology and capital structure. Technology, which is the blueprint for doing things, is embodied in physical capital, which is part of the facilitating structure. Good technology may be embodied in poor capital if its production is beyond the

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[17] In the interwar years of 1918 to 1939, publicly funded research in the United States played an important part in the development of many basic technologies for the emerging aircraft industry, such as retractable landing gear. Later, in the early development of jet aircraft, there were large externalities as airframes and engines developed for U.S. military aircraft had civilian applications (the airframe for the Boeing 707 and the engines for the 747 jumbo jet). The spinoffs from the stealth bomber were, however, quite small since the highly sophisticated and specialized new technology developed for it had fewer outside applications.

[18] By this criterion, U.S. policy was correct in not offering major support to the aircraft industry before 1914, when its potential was still unclear, and then giving substantial public assistance between the two world wars, when aircraft were evolving rapidly and coming into more general use. (The evidence from other technology support policies suggests that this advice is much easier to give than to follow.)

capabilities of the capital goods producers (as it sometimes is when capital goods designed in the West are produced in less developed nations). Similarly, public policies are blueprints for public-sector actions that are given effect by institutions and their bureaucracies. A policy that looks good in the abstract may work poorly in a particular country because it is beyond the competence of that country's bureaucracy to administer, or because it runs afoul of other incompatible elements in that country's facilitating or policy structures. Many factors may be to blame, including the routines of government agencies, the mind set of the delivery officers, the lending and project approval procedures and all of the principal-agent issues analysed in public choice theory. The obvious but important lesson is that the success of a policy is not determined solely by its blueprint. Instead, success depends partly on the specific context in which the policy is implemented. Good policies are designed to operate within the institutional competencies of the organizations that will administer them.

C. Assessing Specific Types of Policies and Programs

We have accepted the judgment that it is socially desirable to have technology policies, and we have seen some of the insights that structural theories offer about such policies. It remains to investigate how to assess more specific types of policies and programs. In particular, is there anything in structuralist theory to help us, first, to assess the relative advantages of framework, focussed and blanket types of policies and programs, and second, to evaluate the design of particular policies and programs?

1. Framework policies and programs

We have seen that in the aggregate version of the neoclassical model, in which knowledge is a single, homogeneous, continuous variable producing a single positive externality, there is an optimum amount of innovation. Given the externality, this optimum amount can be achieved either by lowering the cost of the inputs to R&D or by increasing the value of the outputs (i.e. technological advances). In contrast, the analysis of the detailed process of technological and structural change provided by structural theory has different policy implications. For one major example, different framework instruments, such as generic R&D subsidies and patent protection, have different effects.

What is specified in the aggregate neoclassical model as a smooth accumulation of knowledge is seen in a structural decomposition to be the net result of many failures and many successes. We have already observed that, in a neoclassical world in which risk analysis fully applies, the expected values of a marginal unit of expenditure on R&D would be the same everywhere, so that lowering the cost of inputs or raising the payoff to outputs would have similar effects. However, in a structuralist model with uncertainty and non-convexities, the calculation that equates expected payoffs to R&D across the economy cannot be performed. Furthermore, knowledge does not always arrive in continuous amounts. It comes instead in discrete packages, the benefits and costs of which also come in discrete lumps. Expected values thus cannot be rationally calculated in advance and are often mis-assessed even after initial breakthroughs have occurred.

One implication is that various instruments of framework policies will have different effects on the amount of R&D performed, depending on both the technological and the structural contexts within which they operate. Here are some illustrations.

R&D versus intellectual property protection – Patents only reward those who succeed, while R&D support is independent of results. Since expected values of a marginal addition to R&D often cannot be rationally calculated, and would not necessarily be equated at every margin, an across-the-board subsidy to costs will have different effects from an across-the-board increase in the security of intellectual property. An additional reason for the different effects is that the ability to extract value from patents varies greatly across types of innovation. The same amount of aggregate R&D will be distributed differently between firms when it is induced by an effective patent system compared to when it is induced by an R&D subsidy.

Context-specific externalities – What looks at the macro level like a single homogeneous externality associated with the accumulation of technological knowledge is seen under a structuralist decomposition to be an aggregate of many different and complex externalities, some of which are negative. From this point of view, equal assistance to all technological advancement is a blunt weapon. Nor is it neutral, as predicted by neoclassical theory.

One undesirable effect of framework policies arises because they benefit all firms, whether or not they are otherwise able to internalize the benefits of their activities. Firms in industries such as pharmaceuticals, where patents are effective, are able to internalize much of the value that they create – enough to provide strong incentives to innovate. Because their profits are already protected by patents, these industries gain double benefits from R&D support.

A second important example concerns the upstream-downstream complementarities of technology. As argued in more detail in Lipsey and Carlaw (1996), the inability to keep the results of pre-commercial research secret may lead to too little of it, while ability to keep it secret may lead to an excess of overly duplicative R&D. An R&D subsidy in sectors where firms are hoarding, and thus duplicating, pre-commercial R&D efforts only aggravates what is often wasteful behaviour. A focussed policy that discriminates between situations where the free market produces too much and where it produces too little pre-competitive research is potentially superior to a framework policy that merely encourages more of whatever is already being done. For example, focussed or blanket policies can create commitments between firms that encourage them to carry out pre-commercial research in which they all share. (The Japanese Ministry of International Trade and Industry or MITI has sometimes helped create commitments that provide firms with incentives to engage in, and share, pre-competitive research.)

Formal and informal R&D – Not only will a framework policy cover some activities that do not need support, but it will miss some that do. For example, because in practice there is no clear distinction between innovation and diffusion, much activity that is related to the development and use of new technologies may not appear to be basic R&D. In several important studies, John Baldwin has shown that many small firms do little recognizable R&D but spend considerable time monitoring what larger firms are doing and adapting the findings to their own uses. Other SMEs engage in much informal R&D that is difficult to separate from routine production activities. Although these activities may be just as important as more formal R&D, much of it will be missed by such framework polices as R&D tax credits.

Conclusion – Out of the feasible set of instruments, structuralist theories do not preclude such framework policies as patents, R&D subsidies and investment tax credits per se. Instead, they provide an explanation for the differential effects of these supposedly neutral framework policies, and a method of going beyond them. From a structuralist point of view, the ideal framework policy would be to give each inventor/innovator a lump sum payment sufficient to provide the appropriate incentives, and then to make the resulting technological knowledge freely and immediately available. This ideal public assistance would vary among agents according to the externalities that their innovations create. In turn, the externalities depend on the total social value that the innovations create and the proportion of that value that inventors/innovators can appropriate through their own unaided efforts. Assistance would thus be tied only indirectly to their R&D and their invention and innovation. Although it is not feasible to focus public assistance precisely on the externalities created by each agent, this discussion casts doubt on the neoclassical theory's assumption that framework policies are superior to focussed and blanket policies because they are non-distorting.[19]

2. Focussed policies and programs

In this section we review the earlier work on focussed policies by Lipsey and Carlaw, and use their results to derive the criteria that we employ in the rest of the study.

Pros and cons – Focussed policies are typically embodied in single programs or even single projects. For this reason, we use "focussed policy" as a generic term for focussed policies, programs and projects, whenever further distinction is unnecessary.

In an ideal world, focussed policies could target exactly where assistance was needed. They would discriminate between private-sector efforts to invent and innovate, in accordance with their (guesstimated) potential to create social benefits that the firm cannot capture. They would not, however, seek to internalize all social benefits, instead aiming only for sufficient incentives. While doing this, they would also seek to strike an appropriate balance between encouraging innovation and encouraging diffusion in each particular context. They would encourage changes in the facilitating structure at rates that were appropriate to the existing pattern of technological change and/or that altered it in such a way as to encourage more technological change. Unfortunately, many of the conditions of an ideal world are not met in the actual world. This causes problems when focussed policies are relied on heavily, and still more when they are relied on exclusively.

- Masses of detailed information are required to calculate the externalities associated with each potential innovation in order to design the appropriate, context-specific focussed assistance.
- The transaction costs required to calculate externalities that can in principle be located, and to design and administer the large set of required focussed policies, would be prohibitive.
- Even if such a set of policies could be designed and instituted at zero transaction cost, their administration would require a highly sophisticated bureaucracy at all levels, from head office to the field.
- The more that a policy or program is focussed on small groups, the more easily clients can capture it.
- The more focussed a policy, the more likely is it to be captured by politicians who have a self-interest in the projects that are accepted and rejected.
- Focussed policies carry the risk of trade sanctions, since subsidies must be generally available to be exempt under World Trade Organization (WTO) rules.

The above points show the undesirability of providing support to technological change *exclusively* with focussed policies.

Where specific needs and major externalities can be identified, while capture and other pitfalls can be avoided, focussed policies can provide effective assistance to specific technologies, industries and even firms. Lipsey and Carlaw (1996) cite several examples. Such focussed assistance can be used to complement blanket and framework policies.

[19] Of course, framework policies may be preferred to focussed and blanket policies on all sorts of other grounds, such as administrative simplicity, the fact that they are more difficult to capture and their relative lack of principal-agent problems. If so, the case needs to be made without assuming that there is a general theoretical argument in favour of framework policies independent of these operational considerations.

Policy lessons – Given the clear advantages in principle but the large set of disadvantages in practice, and given that in the final analysis policy assessment must contain an element of judgment, how can we assess the effectiveness of specific focussed policies? In an attempt to assist in this matter, Lipsey and Carlaw (1996) studied some 30 mainly focussed policies drawn from around the world, for which reasonably reliable indications of success or failure seemed to exist. They then looked at the successes and the failures as two groups, and looked for characteristics that distinguished them. From this comparison, they developed a set of empirically generated policy lessons that refer to the design and operation of policies and programs. Following the lessons that Lipsey and Carlaw derived does not guarantee success, but the lessons suggest some conditions that increase the likelihood of creating successful focussed policies.

Lipsey and Carlaw began by classifying their cases according to the changes in technology (*T*) and structure (*S*) required by the policies, either explicitly or implicitly. First, consider technology. The change in technology refers to the number and extent of the changes in the elements of existing product and process specifications that are needed to effect the targeted overall change in technology. They observed, "Making this measure operational, in a detailed way, and relating the required changes in product and in process specifications is an important part of our ongoing theorizing about technological change" (Lipsey and Carlaw, 1996, pp. 269–70). For purposes of their 1996 paper, however, they relied on an impressionistic metric of "incremental" and "large-leap" technological advances. Within each of the categories of incremental or large-leap, they distinguished whether the policy was to catch up with existing technologies or to push on the leading edge of new technologies. Since studies of technological change show that the vast bulk of private-sector R&D activity is to accomplish incremental changes in technology, the "incremental" category must not be taken to mean unimportant. Although governments frequently attempt big leaps, these are much less common in private-sector innovative activity.

Second, consider structure. A fully operational treatment of the changes required in the facilitating structure calls for consideration of both the type and the magnitude of the structural change entailed by the attempted innovation. In their preliminary treatment, Lipsey and Carlaw used the impressionistic classification of the structural changes required by each policy as being small, medium or large. Large structural changes are usually registered at the industry and firm levels, while medium or small changes may be registered only at the firm level. Table 1 at the end of this chapter duplicates Table 1 on p. 286 of Lipsey and Carlaw (1996). It shows their classification scheme made up of 12 cells combining catch-up, leading-edge, large and small technological advances with small, medium or large structural changes. The items in the body of the table refer to the cases that they studied.

Criteria of success or failure – With focussed policies, it is usually possible to assess what are called outputs in our Figure 1. Since (given the assumptions of structuralist theory) optimality is not a goal that is realizable or even definable, Lipsey and Carlaw took a simpler twofold measure of success of focussed policies and programs:

- Either the policy or program resulted in the development (or diffusion) of a commercially viable new product or process (what they call "type-1 success"); or
- the attempt seemed worth making (e.g., it was not primarily for political purposes with little hope of success even at the outset), and its failure was recognized in a timely fashion and the effort terminated (what they call "type-2 success").

The second criterion of "success" is needed because, given the uncertainties involved with the development of new technologies, any successful policy or program must expect to have projects that fail as well as ones that succeed. All that can be asked of the failures is that they were reasonable gambles in the first instance and that funds were not poured into the projects long after failure was obvious (as happens all too often).

The policy lessons that follow are a refinement of those drawn by Lipsey and Carlaw from their study. Although some of the items may sound trite, each is based on one or more real cases, with the "don't" items all being cautionary tales drawn from failures in real policies, programs or projects. The italicized captions are for easy reference to these lessons in subsequent chapters. They do not, however, stand alone, except as labels; the full statement of each lesson is given in the paragraph attached to the caption.

For our present purposes, we have grouped the lessons into four categories: those that relate primarily to uncertainty, those that are primarily concerned with design pitfalls, those that are primarily concerned with structural relations, and those that are primarily related to market forces and information. We say "primarily" in each case because many of the lessons concern more than one category. While being a convenient way to group our lessons, the four categories are not used for further analytical purposes.

Uncertainty

1. Large leaps are dangerous – Attempts at large technological leaps involve many exposures to uncertainties because they require many changes in a main technology and its various sub-technologies, as well as the accumulation of the tacit knowledge that is required to operate it efficiently. Large leaps in technology for which the facilitating structure already exists are extremely difficult to accomplish; large leaps in technology that also require large leaps in the facilitating structure are nearly impossible to accomplish successfully. The history of focussed policies is replete with failed programs that attempted large technological leaps (of either the catch-up or leading-edge variety) requiring major accommodating changes in the facilitating structure.

2. Successful policies and programs often pursue incremental innovation and (where possible) aid in the acquisition of tacit knowledge – Policy makers can reduce exposure to uncertainty by pursuing incremental innovation, assisting firms to acquire tacit knowledge about established technologies and targeting niche developments. This approach parallels the incremental focus that characterizes much private-sector activity.

3. Pushing the development of a technology off its established trajectory is dangerous – Exploiting the potential of a technology within its established trajectory involves fewer exposures of firms to uncertainties than trying to alter the trajectory or establish a wholly new one.

4. Flexibility is important – In the uncertain world of technological advances, almost the only certainty is that something unexpected will happen. There are many uncertainties related to technological change as well as to the design and operation of new projects, programs and policies. Because coping with this kind of uncertainty requires learning from experience, policy designers and program administrators must be able to change course or cancel any venture as unfavourable experience accumulates. Many programs and projects have failed because their procedures and objectives could not be changed as experience accumulated about what was and was not possible. To allow for change, procedures must be put in place for reviews, amendments and/or cancellation of projects, programs or even entire policies. In the case studies that follow, we refer to the ability to revise the internal structure of policies and programs as program *design* flexibility, and the ability to change course or cut off particular projects as *delivery* flexibility.

5. Diversity is one of the best protections against uncertainty – Because technological advances are uncertain, diverse experiments are often more productive than one big push along what appears to be the most likely path at the outset.

6. Exposure to uncertainty can be reduced by exploiting the interrelation between users and producers – Users of a technology can provide producers with information about desired characteristics and problems with past and present designs; they can also give some indication of market demand for innovations. At the same time, producers can provide users with possibilities of which they were unaware. This lesson was stressed in the discussion of spillovers in section IV.A.1.

Design pitfalls

7. Multiple objectives are dangerous –When policies and programs have multiple objectives, the uncertainties involved in technological advances make it likely that the non-technological objectives will predominate, *and the prediction about the future commercial viability of the technological advance will be whatever is needed to justify the decision to proceed.* If technological objectives become mixed up with political prestige, regional development or any other policy objectives, it is virtually sure that the technology objectives will be made subservient to other ends. The history of technological programs shows many instances where favourable technological judgments continued to be held in the face of accumulating unfavourable evidence because of fear about the employment effect, the regional impact or other non-technological consequences of cancellation. The implication is that, wherever possible, technology advancement policies and programs should not be given additional non-technological objectives.

8. Multiple objectives may be sustainable if there are multiple tools – Lesson 7 relates mainly to focussed policies. More complex policies and programs may successfully employ multiple objectives if they assign separate policy tools to each objective.

9. Multiple objectives may be sustainable if they are clearly prioritized – Since we are considering policies designed to advance technology, whenever there are multiple objectives to be served by the same instrument, priority must be given to the technological objective for the reasons outlined in lesson 7.

10. National prestige should be an outcome, not an objective – Policies and programs should not have national prestige as up-front objectives, whether stated or merely implicit. Such policies and programs are handicapped relative to ones that are chosen for potential commercial viability. They tend to bring the opposite of international prestige and commercially viable innovation. Furthermore, they often hinder technical progress when they introduce inferior technologies that become widely used by many industries.

11. Policies and programs should avoid capture – Capture can come from two directions: the clients in the private sector and politicians who would use policies and programs as political pork barrels. Both types of capture are likely where a policy provides significant funded assistance to a select or limited number of firms. They are made more likely where contributions are allocated on a discretionary basis, and where policy objectives and project selection criteria are ill defined. Political capture also becomes more likely the more that publicity surrounds the creation and operation of the policy or program, and the more that political concerns are allowed to influence the selection process.

Structural relationships

12. Attention needs to be paid to the relationship between technology and structure – Changes in either technology or structure typically induce changes in the other. If policy makers target only one of these, there will be induced consequences in the other, which will affect the overall performance of the policy or program, for example, by imposing unforeseen costs or by retarding the targeted developments. If policy makers target technology and structure in ignorance of the interrelations, they may target an inconsistent set of changes that will inhibit attaining their main goals. However, as pointed out in lesson 1, policies and programs that require large leaps in both technology and structure are prone to failure.

13. Policies and programs can play a useful role in inducing and co-ordinating pre-commercial R&D efforts – Policies and programs can assist in gathering and disseminating non-appropriable technical information. They can also provide mechanisms through which firms can credibly commit to jointly conducting pre-commercial R&D, thus reducing the hoarding of such knowledge and minimizing costly duplication.

14. Policies and programs should seek to maximize positive spillovers – We have seen that different technological advances have different spillovers. These depend, among other things, on the current stage in the development trajectory and the number of complementarities, both within the sub-technologies of a main technology and across technology systems.

Market forces and information

15. Market forces and the market expertise of private-sector agents should be utilized wherever possible – Policy makers can successfully intervene to aid innovators provided commercial and competitive objectives guide the intervention. This implies that market concentration and protection must be balanced against competition in innovation, and policy must respond to commercial signals reflecting viability. Policy makers are ill advised to dictate business decisions (i.e., they should avoid micro-management and the suppressing or ignoring of market signals).

16. Information co-ordination and dissemination are important – Not all firms are aware of the current and evolving best-practice technologies that may be of use to them. Policies and programs that assist in spreading existing technical knowledge can cover the discrete sunk costs of acquisition; these are often too high to be taken on by individual firms, especially small ones.

17. Commercial viability should be sought – Technology for its own sake, commonly called "technology push," has frequently produced technological marvels that are commercial failures.

18. Policies and programs should exploit as much expertise as possible – Although this good advice is obvious, it has been ignored repeatedly in many policies and programs in many countries. Administering any even moderately complex policy or program requires a wide variety of expertise, including technological, commercial, financial and administrative. As much as possible, these skills should be developed in house. Where this is impossible or excessively expensive, mechanisms must be developed to tap outside expertise.

19. Competition-inducing mechanisms increase the chances of success – Policies and programs designed to produce inter-firm competition in innovation increase the likelihood of commercial success. Such competition also induces a variety of diverse experiments by profit-seeking firms, often yielding a cluster of innovations. This approach stands in contrast to policies that suppress competition by choosing and backing a national champion in terms of a firm or a technology.

D. Blanket Policies and Programs

We now come to the third of our three major policy classes: blanket policies, which are intermediate between focussed and framework policies.

1. Pros and cons

Given the general objective of encouraging technological change, and the problems that are associated with framework and focussed policies, blanket policies have much to recommend them. First, blanket policies can be used to push a policy objective without being tied to a particular generic instrument. In contrast, framework policies are typically associated with specific instruments, such as tax credits for R&D and investment, intellectual property protection, and broad-based subsidies. Second, blanket policies can accommodate some context-specific tailoring that does not encourage capture, because they can be made conditional on the general objective. Third, blanket policies can be used to change elements of the facilitating structure that are used by firms in their R&D activities. Examples include technology networks, the establishment of public research labs and business information networks. Fourth, blanket policies can sometimes be used to alter the internal structure of firms by attempting to change attitudes toward the employment of new or leading-edge technologies, and encouraging the employment of university-trained research staff. In Chapters 2 and 4, we see examples of blanket policies that seem to have worked in many of the aspects outlined here.

The most serious problem with blanket policies compared with either of the other two types is their potential to degenerate into unco-ordinated activities that support some firms and activities and not others, with no clear criteria for inclusion and exclusion. We study cases in point in Chapter 3 and see that the policies degenerate into incoherence when the administrators are not given very clear directions on such matters as precise goals, instruments and selection criteria.

2. Assessment

With blanket policies and programs, the direct approach of measuring outputs is seldom easy and often impossible. This forces us to employ an indirect approach that uses the set of policy lessons we have outlined. We now use these lessons as design and operation *criteria* for assessing blanket policies and programs. Our procedure is as follows:

Step 1 – We examine the available assessments made by others. These concentrate mainly, but not exclusively, on outputs. They are often highly suggestive of success or failure but seldom, if ever, conclusive. This will not surprise someone who holds to the structuralist view that policy assessment must contain some element of judgment that cannot be wholly resolved by scientific measurement.

Step 2 – We compare the design of the policy or program in question with Lipsey's and Carlaw's design and operation criteria, using them to judge the potential for success or failure.

Step 3 – Where there is agreement between the judgments reached under both of the above procedures, we conclude that there is a strong case for either success or failure.

Step 4 – If the judgments resulting from the two procedures disagree, we seek to reconcile the differences.

The procedures in Step 2 are as follows: First, in the light of available evidence, we judge how each policy rates with respect to each of our design and operation criteria, assigning a grade of success (*S*), qualified success (*QS*), uncertain (*U*), qualified failure (*QF*) or failure (*F*). Qualified indicates either mixed results (leaning toward *S* or *F*) or only minor applicability of the criterion in question. Where the criterion is not applicable to the program in question, it is graded as not applicable (*NA*). Second, we accumulate these grades mechanically, showing the number of criteria obtaining each grade in the case of the policy or program being studied. Third, we form a final judgment that the overall performance of the policy or program passes or fails our criteria test. In doing this, a further element of judgment is required because all criteria are not equally important — and a few are so important that they constitute necessary conditions for success. Avoiding large leaps in both technology and structure is one of these. Others come close to being necessary. Examples are avoiding such things as capture, extremely rigid design and execution, technology push, and domination by considerations of national prestige.[20]

Where our judgment on the program differs significantly from the other assessors, we seek to reconcile these differences (Step 4). We do this by comparing the theoretical perspectives adopted by ourselves and the other assessors, thereby seeking to identify the source of the differences between the assessments. In the cases we have studied so far, the sources are found in the different assumptions that characterize the theories being employed. In principle, the preferred set of assumptions could be established completely by empirical evidence. In practice, a substantial element of judgment is required in assessing the relevant empirical evidence concerning the applicability of the competing assumptions.

[20] All the criteria that we use in this study come from Lipsey's and Carlaw's case studies of focussed policies. Not surprisingly, therefore, they are not perfectly adapted to assessing blanket policies. In subsequent work, we intend to improve the criteria in three major ways. First, on close examination, many of our existing criteria turn out to be multiple criteria. Further work is needed to specify them more narrowly and tightly. Also, new criteria have been suggested by the further cases that we have studied. Both of these procedures will significantly expand the number of criteria, while making it easier to assign a grade to each policy and program with respect to each criterion. Second, we can use our case studies to evaluate the discriminatory power of each criterion. We shall do this by listing for each criterion the programs and policies that passed or failed. A given criterion is a good discriminator insofar as the programs that received a pass (fail) on that criterion were judged to be general successes (failures). It is a poor discriminator insofar as the programs and policies that received a pass (fail) on that criterion were judged to be general failures (successes). Third, as we study more cases and refine our criteria, we expect to develop more analytically useful groupings than the four we have used in presenting our criteria in this chapter.

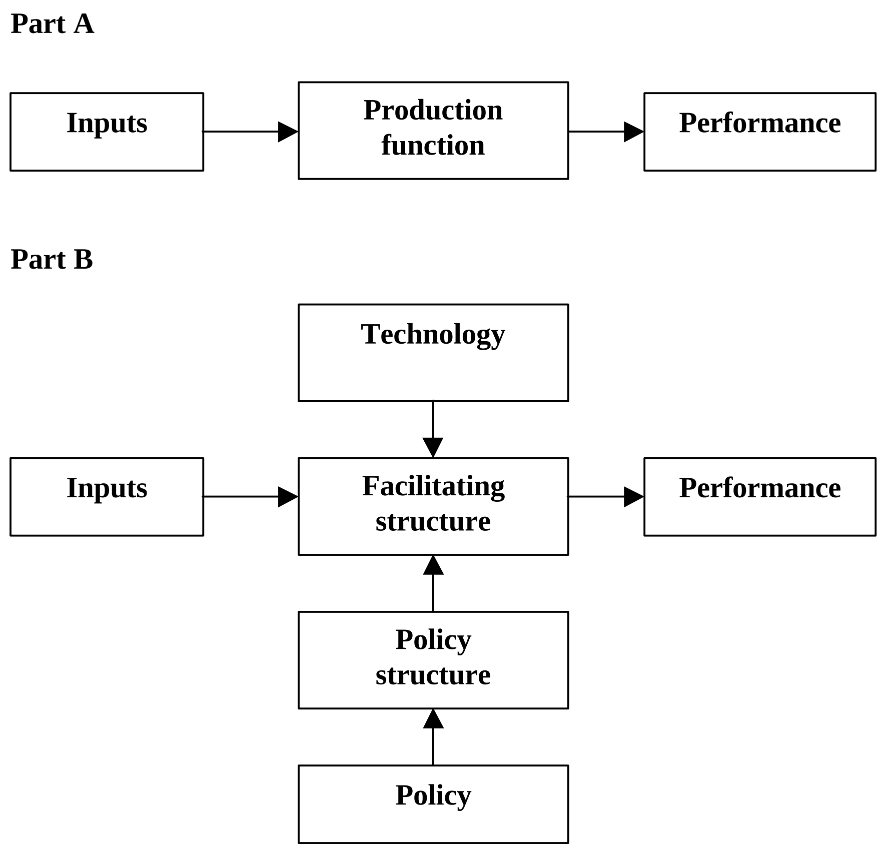
Table 1
Policy Lessons

STRUCTURE	TECHNOLOGY			
	INCREMENTAL		LARGE LEAP	
	Catch-up	Leading Edge	Catch-up	Leading Edge
Large	**Korean electronics** **Taiwanese electronics**	**U.S. military software procurement**	*Japanese commercial aircraft, Phase 1*	**Japanese automobiles** *AGR*
Medium	**Japanese commercial aircraft, Phase 2** **Early Japanese semiconductors**	**SEMATECH (M)** **U.S. military semiconductor procurement** **NACA**	**Airbus (M)** *French micro-electronics* *British computers* **Korean industrial policy** *Indian industrial policy*	*Concorde* *SST*[1] *Alvey*
Small	**Indian trading companies** **Canadian IRAP** **West German SME**	**Stoves in Kenya, boats in India, electricity in Nepal** **MITI: supporting networks, research labs, finance** *Consolidated Computers* *Caravelle*		**VLSI** **U.S. aircraft procurement** *Japanese* 5G[1]

Failure = *Italic* Success = **Bold** Marginal success = **M**

[1] Although these programs were Type 1 failures in that they did not achieve their objectives, they were Type 2 successes in that the programs were halted when they appeared to be failing.

Figure 2
Neoclassical Approach versus the Structuralist Approach



Part A shows the neoclassical approach. Inputs of labour, materials and the services of physical and human capital are transformed by the economy's aggregate production function to produce economic performance, as measured by total national income. The form of the production function depends on the economy's structure and its technology, but these factors are hidden in a black box, whose only manifestation is the amount of output from given amounts of inputs.

Part B shows our structuralist approach. Technology (the blueprints for the products we make and processes by which they can be made) is embodied in the structure, including the internal organization of the firm, the geographical location and concentration of industry, the infrastructure and the financial system. Inputs are acted on by this structure to produce economic performance, measured by such variables as total national income, its distribution and the total amount of employment and unemployment. Public policies are embodied in the policy structure, which can cause changes in the facilitating structure, technology and inputs.

2. THE DEFENSE INDUSTRY PRODUCTIVITY PROGRAM

In this chapter we begin by giving a short history of the Defence Industry Productivity Program (DIPP). We then outline other observers' assessments of the program, which largely reach unfavourable conclusions regarding its ability to meet its economic objectives. Next we apply our design and performance criteria to assess DIPP, reaching a largely favourable conclusion. Finally, we seek to reconcile the differences between our conclusion and those reached by other investigators.

I. HISTORY

The objectives of the Defence Industry Productivity Program differed across two phases. (As we shall later see, the program underwent three organizational phases.) In phase 1, from the program's inception until 1976, DIPP's primary objective was the development and maintenance of Canada's defence industry base. The means used were encouragement of defence exports and, after 1968, civil- or defence-related exports. Because defence was seen as a strategic industry in terms of spinoffs of skills and technologies, the benefits were expected to extend to non-defence industries. In phase 2, from 1976 until the program's termination in 1995, DIPP's ultimate objective became economic growth through exports, while the development of defence-related capability was seen as a means rather than an objective in itself.

DIPP's main predecessors were the Defence Development Sharing program (DDS) and the Canada–United States Defence Production Sharing Agreement (DPSA), both of which were initiated in 1959. DPSA, which was administered by the Department of Defence Production (DDP), was introduced after termination of the Avro Arrow program. DDS and DPSA were designed to meet peacetime needs in a more cost-effective way, while maintaining Canadian defence industries and the skills and capabilities they required.

The programs emerged from the belief that the Arrow's failure demonstrated that there was only one way for Canada to continue participating in the defence market: by targeting niches in co-operation with larger players. A major objective then became to tap the vast reservoir of American defence technology (Lynch, 1980).

A. Precursors

1. The Defence Production Sharing Agreement

DPSA was an attempt to gain Canadian access to U.S. military procurement and to negotiate product areas that would be covered under the production sharing program.[21] It was a non-reciprocal agreement. Until mid-1964, when the agreement was amended by the U.S.-Canada Joint Ministerial Committee,[22] all bids by U.S. firms on Canadian Department of National Defence (DND) requirements remained subject to

[21] Lynch notes that the Defence Research Boards of the Department of National Defence refused to relinquish control of defence research. Thus DDS and, by implication, DPSA were limited to supporting development (i.e., projects for which "the concept had already been proven in the 'Research' phase").

[22] Duty was waived on American bids on Canadian requirements with a value in excess of $250,000.

duties and other restrictions (Lynch, 1980, p. 5). Agreements similar to the DPSA were later sought with North Atlantic Treaty Organization (NATO) countries so that Canada could gain a share of NATO defence purchases.[23]

2. The Defence Development Sharing Program

According to Lynch, significant development programs had to be undertaken that would result in sales to the United States as well as to the Canadian military. Accordingly, the Defence Development Sharing program (DDS) was conceived as a means of initiating and integrating Canadian R&D projects into the U.S. military system without losing control of Canada's advanced technology base. The DDS supported projects in three major categories: (i) "bright idea projects" based on technology that already existed in Canada, (ii) projects that responded directly to U.S. military needs, and (iii) "shopping list projects" involving proposals that had been approved but had not yet reached the funding stage in the U.S. system. The bulk of the initial Canadian projects, 30 by mid-1964, were of the "shopping list" variety. Emphasis was placed on areas of Canadian expertise, including communications equipment, anti-submarine warfare, and short takeoff and landing (STOL) aircraft.

DDS projects were originally to be fully funded through what Lynch terms "back to back" contracts. The Canadian government selected projects off the shopping list and in turn provided 50% of the funding, with the U.S. government providing the other 50%. The contracts were between the Canadian firms selected to undertake projects and the Canadian Commercial Corporation (CCC), which was a Crown corporation acting as an intermediary between foreign governments and the Canadian defence industry. These contracts followed the usual procurement procedure; that is, the government retained rights to technical data arising from the work (Lynch, 1980, p. 10). However, private firms were allowed to repay the government for its contribution in order to obtain the rights to technologies developed.

Implementation required changes within the Department of Defence Production in order to ensure effective delivery. The DDP recruited people with an engineering background and high-level experience in related industrial fields. Field offices were established in Washington, D.C., Philadelphia, Los Angeles and Dayton, Ohio — the major U.S. purchasing centres for aircraft and electronics. The role of the field offices was to ensure that information concerning calls for bids were made known to Canadian firms that might be interested, to deal with procedural problems, and to help Canadian firms liaise with relevant U.S. actors (Lynch, 1980, p. 7). Field offices were later established in London, Bonn and Paris following the decision to broaden the Canadian defence export market through agreements with its European and Scandinavian allies.

DPSA improved market access but did not offer the direct financial assistance that was available to U.S. firms. To level the playing field with the U.S. procurement system, funding mechanisms were introduced. Source establishment assistance was available to support the non-recurring costs related to the establishment of a production source, as well as special tooling costs. Capital assistance was available to support the cost of special test or production equipment needed to meet military specifications (Lynch, 1980, p. 19).

[23] By 1988, bilateral defence, research and production agreements had been established with nine European partners in order to help in the identification of projects for bilateral support.

3. The Defence Development Sharing Agreement

The Defence Development Sharing Agreement (DDSA) was introduced in 1963 as part of the defence development sharing package. This program was also administered by DDP. Its objectives were the same as DPSA.

B. The Defence Industry Productivity Program

In 1966, the funding mechanisms mentioned were renamed the Industry Modernization for Defence Exports Program (IMDE); along with the DDS, this was transferred to the Department of Industry. In 1968/69, IMDE and DDS were merged to create the Defence Industry Productivity Program (DIPP) as part of the merger of the federal departments of Industry and of Trade and Commerce.

At this time, changes were also made to the program's terms of reference. Lynch notes, "The rapid pace of technological development during the era in both space technology and aerospace was such that it was becoming more difficult to distinguish what was defence and what was civil technology." In 1968, after some debate within the bureaucracy, DIPP was authorized to assist in the development of "all manufactured products from industry sectors that contain high technology dual-use civil/defence systems" (Aeronautics Branch, 1993*c*, p. 11). It had a market-driven, export-oriented, cost-shared, repayable focus and involved multi-year contributions (Laycock, 1994, p. 1). It eventually contained the five elements described below.

1. Program elements

Development assistance – Later called R&D assistance, this element supported applied R&D activities for defence and defence-related products on a cost-sharing basis, with a contribution of up to 50% of eligible costs being available. Joint shared-cost programs funded entirely and equally by Canada and co-operating governments were also supported under this element. Industry Canada's Aeronautics Branch provided statistics for a 34-year period, from April 1959 to March 1993. R&D projects represented 65% of total program expenditures ($3.4 billion) but only 25% of all projects. From 1975 onward, the R&D element contained a sub-element focussed on pre-commercial technology development.[24] Projects with a focus on sustaining technologies were eligible for DIPP support and were exempted from repayment, in accordance with the government's contribution repayment policy (Aeronautics Branch, 1993*c*, p. 2; ISTC, 1991, p. 9).

Capital assistance – This element provided loans and contributions supporting the acquisition of advanced capital equipment to upgrade manufacturing capability for defence and defence-related products (Tarasofsky, 1984, p. 48). A direct contribution covered 50% of the cost and an interest-free loan covered the balance; title to the equipment remained with the government until the loan was repaid. The company was also expected to invest an amount equal to the supported costs in a related activity. Over a 34-year period, the capital assistance element accounted for 16% of total program expenditures ($830 million) but 50% of total projects (Aeronautics Branch, 1993*c*, p. 2; ISTC, 1991, p. 9).

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[24] The report of the Aeronautics Branch details DIPP's support for "base sustaining technologies." These are defined as follows: (i) *core*, i.e., "those technologies that maintain a current engineering skill base and [that are] focussed on maintaining or expanding market share"; (ii) *enabling*, i.e., "those R&D activities that raise the technology base required to develop next generation competitive products or processes"; and (iii) *emerging*, i.e., "those R&D activities that represent a departure from current product and process technology and, in general, are in the embryonic commercial stage" (Aeronautics Branch, 1993*c*, p. 2).

Source establishment – This element shared costs associated with the establishment of a Canadian company as a qualified supplier of defence or defence-related products (Tarasofsky, 1984, p. 48). Typical contributions amounted to 50% of eligible costs.

Non-recoverable costs support – This element provided support to Canadian firms that had successfully bid on a foreign project. It provided funding to offset the disadvantages that a Canadian firm experienced as a result of a number of factors, such as adverse conditions unique to Canadian suppliers, the fact that foreign firms had already amortized sunk costs, and a foreign government's support for a competing firm (Tarasofsky, 1984, p. 48). A contribution equal to 50% of eligible costs was payable only if the applicant firm was successful in its bid for the contract (Laycock, 1994, p. 5). Over a 34-year period, source establishment and non-recoverable costs support accounted for 25% of total projects and 19% of total expenditures ($1 billion) (Aeronautics Branch, 1993*a*, p. 2; ISTC, 1991, p. 9).

Market feasibility – In the early 1980s, a fifth element was introduced. It funded studies "to establish specifications and characteristics of defence-related products required to meet market demand, or to determine market sector characteristics" (Laycock, 1980, p. 8). Neither the Peat Marwick study nor the Aeronautics Branch report offer statistics for the market feasibility element. This gap may reflect its relatively recent addition to the program. We do know that between 1986 and 1993, market feasibility projects accounted for 2.25% of total projects and only 0.1% of total expenditures, or $5.2 million (Laycock, 1994, p. 9).

2. Repayment

Until 1986, development, source establishment and non-recoverable cost contributions were subject to discretionary repayment requirements, based on the amount of the profit from sales arising from the supported project and the size of the company's overall contribution. In 1986, in response to criticism from the Auditor General (AG), the standard DIPP repayment clause was modified, setting more specific repayment conditions (AG, 1987, Ch. 16, para. 107).

The February 1990 budget introduced important revisions to the policy of Industry, Science and Technology Canada (ISTC) dealing with repayable contributions; these reflected the government's expressed desire to take a more business-like approach to assistance to firms, and to place emphasis more clearly on investing in economic development rather than subsidizing the private sector. All contributions under ISTC programs, which now included DIPP, were repayable, subject to the exemption of contributions of less than $100,000 (ISTC, 1991, p. 6). For the exemption of contributions greater than $100,000, ministerial approval was required (ISTC, 1991, p. 6).

3. Client base

According to a DIPP document prepared for the 1994 program review of Industry Canada (IC), the program was open to all "Canadian based companies [and partnerships] in the Aeronautics, Space, Defence Electronics, Defence Land and Marine, and other defence related industries" (Laycock, 1994, p. 4). This translated into a client population of over 600 aerospace and defence sector firms. This relatively small group of multinational companies made frequent and extensive use of the program.[25] For example, up to March 31, 1975, a total of 206 firms were responsible for 755 DIPP-supported projects, yielding an average of 3.6 projects per company (Peat Marwick, 1980*e*, p. B-3). In addition, 7 firms accounting for 11 projects "received 59% of all DIPP funds [roughly $750 million] through the fiscal year 1977/78" (Peat Marwick, 1980*a*, p. 8).

[25] By way of contrast to DIPP, note that only about 10% of the Atlantic Canada Opportunity Agency's Action Program clients are repeat users (Aeronautics Branch, 1993*a*, p. 5/2).

Given the special nature of the program's clientele, two unique policy tools were introduced. First, DIPP's terms and conditions permitted retroactive cost sharing if work had commenced or equipment had been ordered prior to the granting of support (Peat Marwick, 1980*e*, p. B-24; AG, 1989, p. 95). Second, memorandums of understanding (MOUs) were used to establish strategic partnerships with major companies that were substantial users of the program. These detailed specific obligations on both parties (Laycock, 1994, p. 4). For example, in return for funding, some companies agreed to assist Canadian suppliers to move up learning curves, thus exploiting the development trajectories established by others.

4. Organization

Three federal departments played key administrative roles for DIPP: Industry Canada and its predecessors, the Department of National Defence and the Department of Supply and Services (DSS). IC had sole ownership of, and accountability for, the program. DND provided military and technological advice. DSS provided contract management on a fee-for-service basis.

We distinguish three different organizational phases over the course of the program's history. The first phase is characterized by the use of Technical Advisory Groups (TAGs) and centralized decision making. During this phase, DIPP's primary objective was strategic support of military technological capabilities. The second phase is characterized by a change in management style. The program moved to a matrix management system, which was more decentralized. The third phase was really simply an extension of the second, with minor revisions.

Phase 1 – Before 1968, program delivery was carried out by project officers in the four branches (Aerospace, Machinery, Electrical and Electronic, and Shipbuilding) of the Department of Defence Production and Industry (formerly the Department of Defence Production). All activities were overseen by the Assistant Deputy Minister (ADM). The Industry Sector Branch (ISB) officer was expected to conduct financial, technical and marketing analyses before a project was approved, and then to monitor and control the project (Peat Marwick, 1980*e*, p. B-25). In 1970, delivery was modified to accommodate a project management approach. Although the ISB officer was still required to possess broad financial, technical and marketing skills, "in-depth analysis became the responsibility of specialist advisers" (Peat Marwick, 1980*e*, p. B-26).[26]

Project proposals were then submitted to one of three Technical Advisory Groups (TAGs), which were "responsible for determining the technical suitability of the proposal" (Senate, 1975, p. 6:66). TAG recommendations were submitted to the Inter-Departmental Committee (IDC), Defence Export Development, which in turn served as an Advisory Committee to the Treasury Board (Senate, 1975, p. 6:67).

Initially, all projects required Treasury Board approval. In 1970, program authority for contributions up to $2 million was delegated to the Deputy Minister (Peat Marwick, 1980*e*, p. C-1). DSS became responsible for negotiating contracts between the Crown and the recipient company, on the basis of the

[26] The financial adviser examined the applicant's financial capability to execute its share of the contract, checked eligibility, and examined the contribution of the project to the company's sales and cash flows (Peat Marwick, 1980*e*, p. B-14). The technical adviser, often the ISB officer, assessed the firm's technical capability to execute the project (Peat Marwick, 1980*e*, p. B-15). The market adviser examined the target market, the extent of the competition and the probability of achieving the forecast sales (Peat Marwick, 1980*e*, p. B-16). The Market Research and Analysis Divisions of the Defence Programs Branch were responsible for this last advisory function. The former provided a quick assessment of the high-volume, low-dollar capital assistance and source establishment projects; the latter provided a more detailed analysis of R&D projects (Peat Marwick, 1980*e*, p. B-16).

specific terms of an approved submission (Peat Marwick, 1980*e*, p. B-54). Program monitoring and control (M&C) became the responsibility of Project Review Groups (PRGs), again with DSS, Department of Industry, Trade and Commerce (ITC) and other government agency (OGA) representation, all of which reported to the Inter-Departmental Committee through the appropriate TAG.

Phase 2 – In 1977, perceived difficulties with ITC management of DIPP led to a restructuring in which a matrix management system replaced centralized administration by the ADM Industry. Program delivery, through five ISBs, became the responsibility of the ADM Industry, Commerce and Development. The ADM Trade Commissioner Service and International Marketing, which was responsible for the Defence Programs Branch of ITC, was also responsible for joint defence projects with the United States and NATO allies. The final position in the matrix was occupied by the ADM Finance, who assumed administrative control of the program in 1977/78.

In this phase, the ISB officer was still the principal actor in the program delivery system (Peat Marwick, 1980*e*, p. B-1). TAGs were eliminated on the grounds that they constituted an additional, unnecessary administrative layer (Peat Marwick, 1980*e*, p. B-27), while the DIPP Committee continued in its advisory role. "The Deputy Minister, the Financial Services Branch, and [as required] the Treasury Board [were] all involved in post-committee approvals to meet the program authorities and requirements of the *Financial Administration Act*" (Peat Marwick, 1980*e*, p. B-40). DSS continued to provide its contract negotiation services.

Eligibility criteria were amended in 1977, placing increased emphasis on the existence of attractive market opportunities (Tarasofsky, 1984, p. 47, citing ITC, 1977, pp. 1–2). Expected sales (Canadian content only) were required to be 10 to 20 times the Crown investment (Tarasofsky, 1984, p. 47).

Phase 3 – From 1980 until the program's termination in 1995, DIPP's delivery system appears to have remained largely unmodified. The delegation of program authority was, however, increased on at least two occasions during this period. First, the ADM level was delegated approval authority up to $500,000, while the Minister's authority was increased from $2 million to $10 million. Above $10 million, Treasury Board approval was required; for contributions of $20 million or more, Cabinet approval was required. Second, program authority up to $100,000 was delegated to the Director General level (Aeronautics Branch, 1993*b*, p. 3).

Under this phase, approval and selection of DIPP projects proceeded according to discretionary criteria. Changes in the criteria appeared to reflect a shift in the program objective toward economic development, as well as overall budgetary pressures.[27] Gone was any reference to production sharing or interest of the Canadian military. New to the list of criteria, however, was the requirement for "generating an acceptable *incremental* return on the investment ... [taking] into account such factors as *incremental* export sales, import replacement, employment, profit [and] capacity utilization" (Tarasofsky, 1984, p. 47, citing ITC, 1977, pp. 1-2) Between 1981 and 1984, incrementality was added to DIPP's *Administrative Directive and Procedures Manual* (AG, 1985, Ch. 12, paras. 84–85).[28]

[27] Since at least 1978, the federal government had been pursuing a policy of spending restraint. This led to the placing of increased emphasis on incrementality and value-for-money considerations in the allocation of contributions through various federal programs.

[28] After 1984, "No project [could] be supported unless assistance was required for the project to proceed in terms of the proposed location, scope or timing" (AG, 1985, Ch. 12, para. 84).

II. Assessments by Others

This section provides an overview of the assessments made of DIPP, organized in terms of the evaluation method they employed. The material is drawn from four main sources: the Auditor General; Peat Marwick and Partners; Tarasofsky; and Industry Canada (Ken Laycock).

DIPP has generally been judged successful when evaluated in terms of its original objective. All the evaluators, including the Auditor General, agree that DIPP maintained and even enhanced Canada’s technological capabilities in defence-related industries. However, when evaluated in terms of its economic objective, DIPP has received mixed and often unfavourable reviews.

A. Design and Operation Characteristics

The general criticisms raised in relation to design and operation of DIPP are: (i) that the program lacked sufficient monitoring and control mechanisms; (ii) that it did not act to sufficiently recover its contributions, which were meant to be repaid by the client firms; (iii) that it had poor financial management control; and (iv) that the processing times for project selection and approval were lengthy.

1. Monitoring and control

Peat Marwick argue that stipulations concerning “company progress reports, PRG meetings, and ISB reporting to ITC ... should [have provided] ITC with adequate control over projects” (Peat Marwick, 1980*a*, p. 87). They note, however, that there were other design flaws that undermined monitoring and control to some extent. First, no guidelines “for the format of company reports and the conduct of PRG meetings” could be identified (Peat Marwick, 1980*e*, p. B-68). Second, because ISB officers were delivering other programs as well as DIPP, monitoring and control became secondary activities (Peat Marwick, 1980*e*, p. B-70). Third, there was a shortage of human resources for this activity, so that data on activities such as post-contract sales were not collected. These, however, are relatively minor criticisms within the general statement that the overall design and delivery of DIPP was sound (Peat Marwick, 1980*a*, p. 86).

The Auditor General’s office was more severe in its criticism: “Monitoring practices were not fully developed or formalized and did not adequately address basic matters, such as scope, methods to be used, frequency, resources to be used, nature and extent of the reports to be provided, progress toward project objectives, and enforcement of terms and conditions” (AG, 1982, p. 293). In response to these criticisms, the Department pleaded lack of available resources (AG, 1985, Ch. 12, p. 91). The shortage of human resources referred to by DIPP may have constituted a design failure. It may also reflect DIPP’s belief that aggressive monitoring and control were unnecessary given the small client base of the program.

2. Repayment of contributions

In 1973, the Auditor General commented on DIPP’s weakness in terms of collecting amounts due to the Crown under the contribution agreements (AG, 1973, p. 63). In particular, the Auditor General found numerous instances in which the agreements omitted, without authority, one of the standard terms and conditions (regarding the sharing of proceeds of sale of prototypes) established by the Treasury Board (AG, 1973, p. 63). Since then, repayment conditions seem to have been tightened considerably. According to DIPP, repayment was a feature of the program in some subsectors (e.g., airframes) for a number of years in response to international (GATT) disciplines. Repayment requirements were not, however, vigorously and generally applied prior to February 1991 (Laycock, 1994, p. 7). The repayment requirements were enforced under various circumstances — for example, when companies showed too much profit or when non-repayment would cause trade problems.

3. Financial management and controls

The Auditor General and Peat Marwick agree that there were problems with DIPP's financial management and controls. At the project selection level, there was little evidence of financial and market analyses (AG, 1982, p. 278). There were no documented industry strategies to guide the Department in allocating funds (Peat Marwick, 1980*a*, p. 92). Furthermore, "Project status and final reports were tabled and approved with little, if any, examination" (Peat Marwick, 1980*e*, p. B-35). Only one R&D project report out of three, and roughly one capital assistance and source establishment project report out of 14, was tabled (Peat Marwick, 1980*e*, p. B-36). Cost-benefit analyses comparing the amount of assistance to be provided, and the expected benefits had largely been omitted (AG, 1982, p. 291). Further, there was no evidence that anything other than the maximum allowable assistance had been considered when approving company proposals (AG, 1982, p. 291).

4. Processing times

According to the information provided by Peat Marwick, DIPP's processing times were quite lengthy. From the time an application was submitted until a final contract was agreed, an average of 377 days passed.[29] According to Peat Marwick, the factors promoting delay included ITC's apparent failure to provide DSS with "specific instructions ... for preparing DIPP contracts" (Peat Marwick, 1980*e*, p. A-23). In addition, DIPP projects did not have priority status at DSS. Finally, they note that the "number of personnel involved in approving projects ... and the plethora of documents which they [had to] approve [made] communication ... difficult, even with good channels" (Peat Marwick, 1980*e*, p. A-24).

Although the delays seem long to the casual observer, they may not have been excessive given the type of firm, the nature of the project, and the amounts of money involved. Peat Marwick recognize the trade-off between screening and delay. The many players in the approval/selection process are said to have provided "extra checks that [ensured] that DIPP objectives [were] kept in mind" and that public funds were protected (Peat Marwick, 1980*e*, p. B-31). They note, "Expediting projects may [have saved] time ... but may [have led] to more errors. Performing more detailed analyses [took] more time ... but may [have led] to better projects" (Peat Marwick, 1980*e*, p. B-32). Indeed, there does not seem to be strong evidence that the firms themselves found the delays excessive.

B. Output

1. Incrementality

Peat Marwick assess DIPP's incrementality, using both the broad structuralist as well as the narrow neoclassical definition of incrementality. Their findings are positive.

[29] An average of 130 days passed from the time an application was submitted until the DIPP Committee provided its recommendation. From the time the Committee provided its recommendation until the Deputy Minister and the Treasury Board's approval was obtained, another 71 days, on average, had passed. From this time to the date of encumbrance of funds, an average of another 53 days had passed. From this date until a request was made to DSS to prepare a contract, another 13 days, on average, had passed. Finally, from the date of the request until a contract was struck, another 110 days, on average had passed (Peat Marwick, 1980*d*, p. 71).

In terms of narrow incrementality, 80% of projects (24 of 30) were deemed to have been incremental.[30] Peat Marwick argue that this is not a high figure given that DIPP projects were quite risky and often would not have been undertaken without outside support.

They also argue (on the basis of a survey of 77 public-, private- and university-sector experts) that DIPP made a significant contribution to the technological capability of the firms it assisted.

Their study also goes beyond the participating firms to assess technological spinoffs. These were defined as the development of (or knowledge to support and develop) one or more related products that would not have been created if the DIPP project had not taken place. They estimate the economic spinoffs at $18 million in 1969 dollars — even though firms found it difficult to cite specific instances or to establish a tight causal relationship between the sales of a product and an earlier R&D product (Peat Marwick, 1980*a*, p. 48).

They also found that DIPP was incremental in terms of its defence objective. That is, the defence sector's continued existence and level of activity were in some ways attributable to DIPP. Additional economic benefits were identified in terms of DIPP's impact on parent-subsidiary relations. A considerable proportion of the DIPP firms were foreign-owned. Peat Marwick found that 33% of these firms undertaking major R&D, and 21% of the other firms with R&D projects, stated that they were in Canada because of DIPP. DIPP was also found to have had some effect on product mandates in about 20% of the R&D firms.

According to the firms surveyed, DIPP played a crucial role in getting them into new markets or new product lines. Most of these same companies said that there was only a small probability that they would have been in existing product lines without DIPP (Peat Marwick, 1980*d*, p. 30). Five companies went so far as to attribute their existence to DIPP.

Taken at its face value, this is impressive evidence for incrementality both of the narrow sort (in terms of the products and technologies developed) and in the wider sense of externalities and structural changes that would not have been realized without DIPP's assistance.

2. Net present value and return on investment

Peat Marwick provide measures of the net present value (NPV) and return on investment (ROI) of a number of DIPP projects. First, aggregate net present value was calculated for 30 projects. A social discount rate of 10% was applied to the cash flows generated by the projects. These cash flows were "adjusted to reflect economic externalities." However, the details of the externalities and the method of adjustment were not provided. According to Peat Marwick, the 10% social discount rate "was derived from the observed economic rates of return produced by the various sectors of the Canadian economy" (Peat Marwick, 1980*b*, p. D-2).[31] Second, the return on investment was calculated for each of the projects and the program as a whole.[32]

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[30] Sometimes several projects were combined into a single case, with the probable result that the number of incremental projects was overestimated (Peat Marwick, 1980*a*, p. 24).

[31] It is not clear whether the social discount rate was fixed by observing the various sectors at the time that Peat Marwick conducted the study, or whether it adjusted the calculation to reflect the rate of return in these sectors across the 20-year period for which it was evaluating DIPP. Also lacking are details about the composition of the sectors and their relevance to the defence industry.

[32] For the program as a whole, an estimation technique was used which treated the program as a single project

The study first calculates DIPP's NPV and ROI for all projects. DIPP yielded a positive NPV of $61.1 million (in 1969 dollars) and an ROI of 10.75%. The net program impact was then calculated by taking the NPV for incremental projects alone, and subtracting program delivery costs (estimated at $29 million) from incremental benefits. This calculation suggested that the economy was less well off than it would have been without DIPP by about $97 million (1969 dollars). It was based on the assumption that, if left in the economy, DIPP funds would have generated the normal return on investment of 10% (Peat Marwick, 1980*a*, p. 18). The ROI for incremental projects was found to be 7.5%.

Peat Marwick also provide this calculation for each of the three program elements. The R&D element was found to have generated ROI of 7.25%. The figure was 10% for the capital assistance element and 10%+ for the source establishment element. The evaluators note, however, that because of the narrower data base available, the latter two results could not be derived with the same precision and are not as robust as for the R&D component (Peat Marwick, 1980*a*, p. 18). The general conclusion from the ROI calculations supports those who hold the view that DIPP funds did not earn their 10% opportunity cost.

3. Attribution of sales and employment

Ken Laycock at Industry Canada documents the effect of DIPP by looking at the performance of the aerospace industry (Laycock, 1994, p. 15). The study notes that Canada has one of the fastest-growing aerospace industries in the western world. The Canadian share of the global market in planes and parts has increased at a rate in excess of Canada's overall manufacturing exports. This industry is one of the few manufacturing sectors to have a positive trade balance. Its engineering and production wages are 13% higher than overall manufacturing wages, and 24% higher than wages for the economy as a whole. Laycock estimates that the economic impact per DIPP dollar was $25 in sales, $18 in exports and $4 in private-sector R&D.

III. A Structural Assessment

In this section we assess DIPP against our design and operation criteria.

A. Uncertainty

1. Large leaps are dangerous. 2. Successful policies and programs often pursue incremental innovation and (where possible) aid the acquisition of tacit knowledge. The aim of the DPSA and DDS programs was to give Canadian firms access to niche activities in foreign procurement markets. These activities were carried on, and in many cases enhanced, under the successor Defence Industry Productivity Program, which was more broadly targeted at developing and exporting niche applications of established technologies. They were incremental, largely market-driven, defence-related research activities. DIPP supported the adoption and adaptation of technology, technology transfer and diffusion. Although Canadian firms had limited resources and operated in a relatively small domestic market, DIPP made a virtue out of this liability by aiding the development of technical expertise suitable to such narrow market niches (Peat Marwick, 1980*a*, pp. 51–52).

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extending over the assumed average life of the individual projects (seven years), and in which the Crown and private investments were spread over the first three years and the returns over the last three. Using the summed NPVs as the NPV for the program as a whole, the evaluators calculated corresponding returns with the program expenditures kept fixed (Peat Marwick, 1980*b*, p. D-3).

DIPP encouraged the diffusion of existing technologies and the development of research capabilities within Canadian firms. The majority of DIPP projects involved the firm in a technology fairly new to it (Peat Marwick, 1980*d*, p. 59). Furthermore, the MOUs were an explicit attempt by DIPP to ensure that parent firms would transfer tacit knowledge to Canadian subsidiaries in exchange for support from DIPP. Under many of the MOUs, the prime contractor agreed to bring Canadian firms up the learning curve as a condition of DIPP funding.

Most of the projects were in the middle ground, involving more than mere duplication but less than large leaps; they were thus halfway between embryonic and mature technologies (Peat Marwick, 1980*d*, p. 59). Some 60% of projects were described as imaginative applications of existing technology, just over 21% were described as significant advances in the state of the art and 17% were described as routine applications of existing technology; none of the projects were described as significant breakthroughs in the state of the art (Peat Marwick, 1980*d,* pp. 59–60). This is evidence that most of DIPP's projects avoided the pitfalls identified by Lipsey and Carlaw (1996) of attempting technological advances that were either too large or beyond the structural capabilities of the firms being assisted.

Furthermore, most of DIPP's projects (70%) were market-driven, giving rein to the tendency of private firms to pursue incremental innovations rather than large leaps. The other 30% of projects were initiated by government; in other words, the Department became aware of a specific contract (and/or market) and notified the company that it judged to have the most experience in the field (Peat Marwick, 1980*d*, p. 31). In these cases, the program was providing market information rather than making specific technological decisions for the firms. For the most part, large leaps were avoided because firms were operating in a competitive export market, instead of being subsidized in a protected domestic market.

All these successes occurred despite the design problems mentioned in section II, such as the program's built-in mechanisms for political and prestige interference, which could have led to large leaps. In fact, some such leaps were attempted. For example, the Tactical Airlines Guidance System (TAGS) project seems to have failed largely because the DIPP-inspired attempted leap in technology was beyond the capacity of the firm involved[33] (Peat Marwick, 1980*c*, pp. A1–A36).

The fact that TAGS was an exception to the general practice is attributable to a number of DIPP's key characteristics, in particular the targeting of export-oriented activities that developed proven technologies for specific niches, and the institutional competence of the program officers. In addition, the lessons from the Avro Arrow project stayed with the program, causing politicians and bureaucrats by and large to scale their technological ambitions to fit the existing facilitating structure of the industry, while attempting incremental improvements in that structure. Although DIPP rates as a success in all of this, its occasional failures at attempted large leaps cause us to rate it as only a qualified success. On criterion 1 as well as criterion 2, it receives a grade of *QS*.

3. Pushing the development of a technology off its established trajectory is dangerous. DIPP sought to exploit existing trajectories rather than to establish wholly new ones as a means of maintaining and further developing a core of technological competence in defence-related sectors. Firms were able to

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[33] The Tactical Airlines Guidance System (TAGS) involved the design, development and flight testing of an integrated, triple-redundant, digital, fly-by-wire control system for helicopters. CAE Electronics Limited of Montreal was supported for this project. The firm was pushed well beyond its expertise in design and building of flight simulators for commercial aircraft, which proved to be inadequate for the project. Furthermore, the DIPP never clarified the project's commercial prospects. The technical objectives were not met and there was no significant commercial fall-out. About $12 million of DIPP funding was written off when the project ended.

acquire knowledge about technologies that had already been proven (at least militarily) and to develop niche sub-technologies that were complementary, thus greatly reducing their exposure to uncertainty.

Support was provided to encourage the development of upstream technologies from which technological trajectories could be both generated and sustained. DIPP used the R&D assistance element to support the development of a number of different classes of technologies: "sustaining technologies" maintained the research program and technological capabilities within the firm; "engineering base technologies" improved competitive performance and/or positioned the industry to compete in future market opportunities; "core technologies" maintained current engineering skills and focussed on maintaining and/or expanding market shares; "enabling technologies" enhanced the technological competence required to develop the next generation of products or processes; and "emerging technologies" were new pre-commercial technologies.

By exploiting the trajectories of established technologies and by seeking to incrementally improve and develop upstream technologies to generate new trajectories, DIPP operated in accordance with this criterion and therefore we rate its performance a success. On criterion 3 it receives a grade of *S*.

4. Flexibility is important. This criterion is divided into flexibility within the design of the program and flexibility in terms of how it was delivered.

Design flexibility – DIPP did not go through anything like the numerous design revisions experienced by either the Industrial Research Assistance Program (IRAP) or the Industrial and Regional Development Program (IRDP). It did, however, make some changes in response to critical assessments of its performance. Important examples are the 1977 revision to incorporate a matrix management system, and the addition of the marketing element in the early 1980s. In other cases, DIPP did not act to meet some external criticisms, specifically those concerning its monitoring and control, repayment policy, financial management and processing times. The lack of response can be interpreted in two ways. First, given the experience of the client firms, the nature of the market and the modest technological advances being sought, the initial design of DIPP was more or less right. Second, the administrators of DIPP were wrong in not accepting the outside criticisms mentioned above. Even if this is so, there is no underlying design fault but rather a difference in judgment between administrators and external assessors about satisfactory design requirements.

We find no compelling evidence that DIPP's administrators should have responded to the outside criticisms that they ignored. For example, the long processing time may have been a reflection of a satisfactory balance between scrutiny and speed. In addition, the informal nature of the monitoring and control may have been appropriate to the small number and relative sophistication of the client firms. Accordingly, we tentatively judge DIPP's internal flexibility to be a success in making only those amendments that experience and judgment suggested to be necessary. On the criterion of design flexibility, it receives a grade of *S*.

Delivery flexibility – As we observed in Chapter 1, the uncertainties of technological development imply that many attempted advances will fail. This fact in turn requires that successful programs have effective mechanisms for identifying projects that are in danger of failing or have already failed, and then for reacting appropriately in terms of amendment or cancellation. In DIPP's case this was not a serious problem, for two reasons. First, failure was clearly identified when a firm did not obtain a contract on which it bid. Second, specific international agreements and general trading rules precluded the indefinite support of a failed initiative — as has often occurred in other situations.

Although DIPP did not require its own internal mechanism to recognize and cut off failures, it did require a mechanism to reward and encourage success after the fact. In this light, the retroactive granting of support to firms that had successfully bid for contracts assisted them in making further technological advances and successful contract bids. This policy of retroactive support would have been dangerous in programs where success or failure could not be judged independently of the support granted to clients. Thus, the AG's criticism of DIPP, while cogent in general, misses the special circumstance making this an appropriate mechanism for the program.

If the program as a whole had been a failure, the targeted firms would have been consistently unsuccessful in their contract bids. If that had been the case, we would have had to judge success in terms of this criterion by whether the entire program was cut off when enough information had been generated. Since DIPP's client firms were obviously successful in bidding for contracts and in developing increasingly valuable defence-related activities, particularly in aerospace, there is no need to make this assessment of flexibility in the face of failure.

Because the program did not require a complex mechanism to recognize and react to unexpected project failures and because it had a mechanism to reward unexpected (i.e., not previously supported) success, we rate DIPP as a success in this regard. On the criterion of delivery flexibility, it receives a grade of *S*.

5. Diversity is one of the best protections against uncertainty. Blanket programs have the capacity to encourage many diverse experiments. Nonetheless, other governments have not been able to resist the temptation of selecting and pushing a particular firm or technology as a national champion to the exclusion of all others (see Lipsey and Carlaw, 1996, for examples). DIPP avoided this temptation.[34] Instead, it backed as many firms and technologies as the market would support; indeed, it probably expanded on what would have been the results of the unaided market. It did so by pushing for broadened product mandates, broadened technological capacities and the provision of defence industry intelligence. The cost- and risk-sharing focus of DIPP also enabled firms to undertake more experiments than they would otherwise have done, or different ones. (This was especially true for subsidiaries located in Canada.) In addition, participation in DIPP also provided access to foreign technologies and know-how, thus permitting more and/or different experimentation. The early use of MOUs and the continuation of jointly funded Canada–U.S. projects are cases in point.

For avoiding the national champion temptation and encouraging diversity instead, DIPP rates a mark of success. On criterion 5, it receives a grade of *S*.

6. Exposure to uncertainty can be reduced by exploiting the interrelation between users and producers. Given the targeted military procurement market and the sophistication of most of the firms operating in it, interchange of information between producers and users was usually effective without third-party assistance. Where this was not the case, DIPP had several assistance mechanisms. Its roles included the identification of market and technological opportunities. For example, projects were selected from foreign governments' shopping lists, and suitable Canadian companies were informed of them.[35] On the basis of its information gathering in foreign countries, DIPP anticipated future needs and encouraged firms to develop the technologies to satisfy them (i.e., the engineering, emerging and enabling technologies referred to in a previous section). The market feasibility element, added in the early 1980s, had the aim of linking users and producers. Although valuable, it was not used as much as it might have

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[34] The Avro Arrow project, which predated DIPP, was a clear Canadian example of the national champion approach. The 1997 CBC documentary on the Arrow shows the strong popular appeal of the national champion approach.

[35] Another mechanism was the Canadian Commercial Corporation (a Crown corporation), which acted as a go-between for Canadian firms and foreign governments in negotiating procurement contracts.

been in other markets, probably because of the close relationship between the relatively small number of producers and users in this market.

Because there was not a great deal of scope for policy mechanisms related to this criterion, the limited number of mechanisms employed by DIPP seem to have been appropriate. We therefore rate DIPP as a qualified success in this respect. On criterion 6, it receives a grade of *QS*.

B. Design Pitfalls

7. Multiple objectives are dangerous. 8. Multiple objectives may be sustainable if there are multiple tools. 9. Multiple objectives may be sustainable if they are clearly prioritized. DIPP did not always have the same main objective. It made a switch from maintaining a strategic presence in defence technologies and in the defence market to encouraging economic growth. This probably reflected changes in the economic climate and in the focus of government policy. Two points are important in assessing this change. First, at any one time there was only one main objective. The program was therefore able to focus its activities on that overriding objective — something that we shall see was important in both the IRAP, which had prioritized objectives, and the IRDP, which did not. Second, the change in main objective was little more than a change in emphasis, since technology enhancement was seen as a major instrument for supporting the defence sector in the first instance and for economic growth in the second.

DIPP also had other stated objectives. Domestically, these were to: (1) “maintain an industrial base for the supply of defence equipment in times of emergency”; (2) “maintain in Canada a defence industrial capacity to service and maintain advanced DND equipment”; and (3) “minimize the cost of acquisition to DND of equipment and supplies” (Peat Marwick, 1980*b*, p. E-1). On the international front, DIPP sought to “serve the objectives of international defence development and production sharing arrangements,” and to “demonstrate Canada’s advanced technology capability to foreign nations,” thereby contributing to Canada’s strategic relations with its allies (Tarasofsky, 1984, p. 47, citing ITC, 1977, pp. 1–2; Lynch, 1980, p. 23). However, on closer examination, all of these appear to be more detailed specifications of the main objective. Hence, they did not require prioritization.

DIPP also had what appeared to be multiple sub-objectives, such as supporting R&D, capital assistance and source establishment. For each of these it had specific instruments and, on close examination, these all supported the main objective of technology enhancement.

While DIPP did not, therefore, have conflicting multiple objectives, its administrators had potential conflicts arising from the multiple programs they delivered. DIPP was only one of the many ITC programs delivered by ISB officers (Peat Marwick, 1980*e*, p. B-8). This fact may have been responsible for the lack of focus in program delivery, noted by Peat Marwick. DIPP contracts were also only a few among the many contracting activities conducted by the DSS, and did not have priority (Peat Marwick, 1980*a*, p. 3). Peat Marwick argue that the lack of priority contributed to the long processing times, noted earlier. The lesson is that, since it is difficult to prioritize programs delivered by a single group of bureaucrats, such a complex administrative structure should be avoided where possible.

Because DIPP avoided the many serious conflicts associated with multiple objectives and because no serious delivery flaws were associated with the multiple-program administration,[36] we rate it a success in this respect. On criteria 7, 8 and 9, it receives a grade of *S*.

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[36] If further evidence was forthcoming that the multiple administration caused significant delivery problems, we might have to alter the grade to a qualified success. However, any problems associated with multiple administration

10. National prestige should be an outcome, not an objective. One of DIPP's initial objectives was to maintain a high Canadian profile in the international procurement market. The means chosen for achieving this end were appropriate. Firms and technologies were not selected for their direct prestige content, nor were they chosen for their Canadian ownership.[37] Instead they were chosen for their chances of market success, with prestige being the outcome of many successful projects.

We have already noted many of the structural features of the program that were in line with this criterion. Among the most important were the many layers and federal departments involved in the screening process, the targeting of niche applications and incremental research, and DIPP's active recruiting of technical and market expertise. These gave it the institutional competence to counterbalance any tendencies to aim for prestige. On this last point it should be noted that politicians are not the only group that can be seduced by overt prestige considerations. At other times and in other places, the technocrats themselves have gone for unrealistic technology pushes and high-profile projects.

On criterion 10, DIPP clearly was a success. It receives a grade of *S*.

11. Policies and programs should avoid capture. Capture can come either from private-sector clients or from the political arm.

Private capture – The objectives of the program were so close to those of its client firms (i.e., success in the international procurement market) that there was little motive for private capture to redirect the program's efforts. Nevertheless, there is evidence suggesting some private capture of the contributions and monitoring. Specifically, as pointed out by the AG, Peat Marwick, Tarasofsky and Usher, most contributions were at the maximum allowable rate; repayments were sporadic and increased only after criticism from the AG; there was a general lack of monitoring and control; and there was intensive and repeated use by a few client firms. The reasons for this form of behaviour may have been capture or possibly the attitudes of the program's administrators. In any case, these features did not seriously interfere with the program's performance but, insofar as they were avoidable, they reduced the effectiveness per dollar spent.

Political capture – DIPP did have features with built-in susceptibility to political pressure. These included the practice of informing local politicians of projects in their jurisdictions at the time of project applications, the levels of discretionary bureaucratic power in the selection and approval process, and the wide authority given to the Minister to approve or reject specific projects (AG, 1985, Ch. 12, para.11; Peat Marwick, 1980*e*, p. B-80). Although it is unrealistic to expect politicians not to be notified of major expenditures in their constituencies, a selection system could have provided for public announcements only after the selections had been made. This procedure, which would have greatly reduced the opportunity for political influence in the selection process, was not adopted. The Auditor General (1989) provides some case studies and key examples where political interference appears to have played a role. Another illustration is the TAGS project, discussed earlier.

DIPP thus experienced problems with both private and public capture. Design characteristics encouraged the problems and the evidence is consistent with some degree of both types of capture. Nevertheless, none of this seems to have diverted the program from its overall objectives, nor to have prevented it from

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would have been minor compared with those associated with multiple objectives. Furthermore, they could have been eliminated by a relatively simple redesign of the program's delivery mechanism.

[37] Seeking prestige by backing a home-owned champion has led to some dramatic failures at other times and in other places. In the defence industry market, multinationals were bound to play a dominant role.

achieving them. For these reasons, we rate the program as a qualified failure in this respect. On criterion 11, it receives a grade of *QF*.

C. Structural Relations

12. Attention needs to be paid to the relationship between technology and structure. This criterion can be divided into two parts: the appropriateness of the program's internal structure and the appropriateness of the program's technological objectives to the economy's facilitating structure.

Internal structure – We have already observed that the program was well structured to meet its technological objectives. Although several perceived design problems were dealt with through revisions to the program, in other cases DIPP did not meet outside criticism by redesigning. It appears that DIPP administrators did not accept the criticisms of their monitoring and control, contribution repayment, and financial controls. This is a matter of judgment and we have already stated that, given the lack of evidence to the contrary, we side with DIPP.

Facilitating structure – The design of DIPP's eligibility criteria exploited existing relationships between technology and facilitating structure, and sought to induce changes in the structure where these were needed. By supporting the purchase of new capital, the capital assistance element focussed on firms undertaking R&D projects and technological upgrading. The R&D element supported the development of a variety of different technologies, all targeted at eliciting different structural responses from the firm. For example, some were designed to prompt the firm to create upstream technologies that could spin off a number of commercial development trajectories, while others were intended to improve the firm's technological capacity. Furthermore, DIPP sought to match niche technology development projects with Canadian capabilities, as is evidenced by the selection of projects from foreign shopping lists.

DIPP worked within the structure of national and international defence markets. It had direct support from, and an interface with, the Department of National Defence. It also was designed to address the disadvantages created by subsidies and other support practices in the aerospace and defence sectors undertaken by foreign governments.

DIPP seems to have been a clear success in dealing with the relationship between technology and the facilitating structure, avoiding the major pitfalls presented by failure to align technological objectives with existing structures. Although the view is debatable, DIPP seems to us to have been largely successful in adapting its own structure to its evolving experience. In our judgment, therefore, it rates as a success in this respect. On criterion 12, it receives a grade of *S*.

13. Policies and programs can play a useful role in inducing and co-ordinating pre-commercial R&D efforts. As noted above, DIPP targeted several classes of upstream technologies. It focussed on sustaining, engineering base, core, enabling and emerging technologies, with the aim of inducing clients to engage in pre-commercial R&D. By demonstrating the capacity of these firms to generate pre-commercial innovations and use them to create new products, DIPP helped Canadian firms obtain product mandates from their parent companies (Peat Marwick, 1980*a*, p. 17).

Although DIPP encouraged pre-commercial R&D, it had no obvious mechanisms to prevent firms from hoarding their upstream technologies, or conversely to encourage the joint production and sharing of these technologies. It is not clear, however, whether such commitment mechanisms were needed, given the structure of the relations between firms and customers and the nature of the niche technologies being targeted.[38]

It is clear that DIPP encouraged pre-commercial research but it is not clear whether it missed an opportunity to create needed commitment mechanisms for the joint production of such research. We therefore rate DIPP as only a qualified success in this respect. On criterion 13, it receives a grade of *QS*.

14. Policies and programs should seek to maximize positive spillovers. DIPP's support of strategic technologies in defence and defence-related civilian firms was an overt attempt to create and exploit technological spillovers. By creating and maintaining defence technologies, DIPP sought not only to support defence directly but also to create a pool of technological expertise for industrial purposes. Laycock observes:

> DIPP plays a strong role in technology spinoff, and diffusion ... aimed at promoting change to the new economy. In this process, a relatively narrow range of extremely high technology defence technologies are routinely spun off and diffused through a wide range of civil high technology sectors spanning commercial markets in aeronautics, advanced materials, power supplies, environmental systems, medical devices, simulation, and marine equipment. (Laycock, 1994, p. 12)

The intent and the structure to create and exploit spillovers were certainly present. Although it is extremely difficult to measure these with any precision, Canada has a thriving defence industry and civilian firms, particularly in aerospace. There is thus a strong prima facie case to rate DIPP a success in this respect, and no strong evidence to the contrary. On criterion 14, it receives a grade of *S*.

D. Market Forces and Information

15. Market forces and the market expertise of private-sector agents should be utilized wherever possible. DIPP's main thrust was to succeed in the international procurement market. Hence it was substantially market-driven — although political and other influences seem to have led to some departures from this excellent market focus (departures that, to no surprise, resulted in failure). Furthermore, with some important exceptions, DIPP did not attempt to foist technology pushes onto unwilling firms but instead backed niche developments that firms were themselves willing to back.

Most of the details relevant to these judgments have already been noted. For example, DIPP sought to develop niche technologies for export; firms were allowed to obtain support retroactively, a practice that gave some market signal as to the viability of the project; and DPSA and DDS were set up so that Canadian producers could learn about foreign government demands and then bid on them.

The evidence is that most of DIPP's efforts were successful with respect to this criterion. However, in view of the departures noted by Peat Marwick and the AG, where political pressures and internally generated technology push seem to have influenced projects, we can only rate the program's overall performance as a qualified success. (Indeed, with some modest redesign, these mistakes could have been avoided in the future had the program continued.) On criterion 15, DIPP receives a grade of *QS*.

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[38] Commitment mechanisms are most needed in cases where a large number of firms are all seeking the same fundamental breakthrough. They are least needed when one or two firms are seeking a new variation on an established technology.

16. Information co-ordination and dissemination are important. This criterion concerns assisting where necessary in the gathering, co-ordinating and disseminating of information. As we shall see in Chapter 4, IRAP's client base is large and diffuse and includes many small firms that cannot bear the fixed (and sunk) costs of becoming fully informed. They require information not only on market demand but about existing technologies and the potential for adapting these to their own use. By contrast, DIPP's client base was smaller, being composed of larger firms that were technologically sophisticated and with simpler required information linkages. Reflecting this composition, most of DIPP's important information and co-ordination activities were of the user-producer variety discussed earlier under several criteria, in particular no. 6.

One of the main objectives of DIPP's predecessors was to gain access to, and information about, the U.S. defence market. DIPP itself maintained the role of information and intelligence provider to its client firms. It helped identify technology and market opportunities. It assisted Canadian firms in gaining access to the U.S. procurement market. It also helped firms participate on projects with U.S. firms through its use of MOUs. In addition, DIPP set up field offices in major aircraft manufacturing centres in the United States as well as in Bonn, London and Paris to ensure that Canadian firms knew about and were able to bid on all eligible projects.

In this respect, therefore, DIPP rates a clear success. On criterion 16, it receives a grade of *S*.

17. Commercial viability should be sought. This criterion is intended to avoid the many disasters that have occurred at different times and in different countries when major technological advances proved to be commercial failures. DIPP largely avoided this error because it targeted niche developments for sale in foreign markets, where trade disciplines prevented the endless subsidization of commercial failures.

Furthermore, the intelligence role played by DIPP helped identify emerging technologies with market potential for its client firms. It did not force firms to undertake research programs. Instead, DIPP provided information on potential projects to its client firms, and projects were selected in response to the firms' applications.

In summary, DIPP was basically a market-driven program with cost sharing, where repayment clauses and other safeguards sought to avoid the distorting effects of "free money." If there were faults, they lay not in the selection of projects but in the application of the checks to ensure efficiency in the use of support funds. For producing commercially viable projects, DIPP rates a clear success, but because of worries about the efficient use of its contributions, we rate it only a qualified success in this respect. On criterion 17, it receives a grade of *QS*.

18. Policies and programs should exploit as much expertise as possible. In contrast to IRDP, DIPP clearly operated in conformity with this criterion. First, it recruited experts in technology and engineering to be its delivery officers. Second, it tapped into the specialized knowledge of the Department of National Defence for information about the domestic and international defence markets, and it turned to the Department of Supply and Services for contracting expertise. Third, DIPP made use of the expertise available in the client firms by, in most instances, supporting rather than supplanting their decision-making mechanisms.

DIPP's performance in these respects contrasts with failures in other programs in Canada and elsewhere. Accordingly, we rate it a clear success in this regard. On criterion 18, it receives a grade of *S*.

19. Competition-inducing mechanisms increase the chances of success. The U.S. procurement, at which most DIPP-supported activities were directed, already contained a competition-inducing mechanism. Although Canadian firms competing for contracts had their development expenses covered, the significant rewards went to those who succeeded in this competitive market. The Canadian contribution could have been channeled through a single local champion in the mistaken belief that this approach would be efficient. Instead, DIPP avoided this pitfall by encouraging as many Canadian participants as local conditions could support.

Once again, although this may seem a modest success, failures at other times and in other countries show that a major pitfall was avoided. We therefore rate DIPP a success in this respect. On criterion 19, it receives a grade of *S*.

IV. CONCLUSIONS

Step 1 – On our 19 criteria DIPP rates 12 clear successes, 6 qualified successes and 1 qualified failure.

Step 2 – Our qualitative judgment about the successes and failures is that, on major points of design and achievement, DIPP operated in conformity with our criteria. The one clear case of failure involved political capture and a possible internally generated technology push. However, this seems to have affected a relatively small number of DIPP's projects. Cases of qualified success involved considerations of efficiency, such as monitoring, control and repayment requirements, and multiple program administration. Here there is an element of judgment at play: DIPP's own officers felt that less was required than called for by outside observers. Even if the criticisms in this area were all justified, they did not prevent the program from achieving its main objectives of supporting a commercially viable industry in defence and defence-related civilian products.

The sources of the failure and of the qualifications on success in meeting our criteria were all remediable without changing either the basic structure or focus of the program, had it not been terminated (without any major review and assessment of its performance). For example, political interference could have been reduced by withholding public announcement of DIPP projects until internal selections had been made. In addition, to the extent that more effort was required to ensure efficiency, the human resources could have been made available and some of the high degree of discretion replaced with rules ensuring a minimum level of monitoring and control.

Step 3 – There is a significant difference between our structuralist assessment and other critics' economic performance assessments. These criticisms fall into two main classes. The first set concerns economic payoff. The argument is that, although DIPP supported many successful projects, the return did not cover the opportunity cost of the public funds and accordingly the program had to be justified on non-economic grounds. The second set concerns DIPP's design and administrative efficiency. The argument is that DIPP's results could have been achieved more efficiently.

Step 4 – Can we reconcile our assessments with those of the critics? We turn first to the performance assessments. If we take their estimation procedures at face value, there are still questions about the details of their application. For example, the seven-year time horizon for the valuable life of any project seems odd, for two reasons. On the one hand, it seems short when one is dealing with major externalities that can spread through much of the economic system. On the other hand, it seems long when dealing with the direct payoff to a particular contract, even when it involved some new niche technological development.

As a second example, the appropriateness of the 10% opportunity cost applied to DIPP investments can be questioned. Peat Marwick calculated this as the rate of return achieved in sectors closely related to the defence sector. The rationale for DIPP's assistance was that this was a "strategic sector." We take this commonly used, but often vague term, to mean a sector that has a net outflow of positive externalities to other sectors. In other words, while it received some benefit from externalities created in other sectors, it conferred a greater value of externalities on them. When the government addresses this problem by encouraging investment in the strategic sector, it will raise the overall net social return while lowering the private return in the strategic sector (and raising it in the other related sectors). It is therefore inappropriate to require that the return on government investment in the strategic sector should equal the return in the other related sectors.

Putting these technical points aside still leaves us with the issue of measuring externalities. Peat Marwick claim to have made an accounting of the spillovers generated by DIPP projects. We would argue, however, that a full accounting of the technological externalities detailed in Chapter 1 was not even attempted (and is unlikely to be possible). Accordingly, the estimated returns to the projects were almost certainly below the true social value.

Finally, if we take the Peat Marwick calculations at their face value, DIPP needed to generate less than 3% in external social value on its investments to meet the 10% criterion. While it may not be possible to verify that the program did achieve these modest externalities, we note that the industries supported under the program were successful and in some cases have become world leaders.

We turn now to the efficiency-related criticisms. The most serious one seems to us to be DIPP's slackness in requiring repayment of support funds on successful projects. In supporting something as uncertain as technological change, even of the niche variety, it was appropriate for DIPP to take a position similar to an equity rather than a debt holder. DIPP did so by not requiring repayment on unsuccessful projects, but it should still have routinely required repayment on projects that were successful in returning a sufficiently high private rate of return. That it did not do so is consistent with capture (or failure to appreciate the importance of this point on the part of the administrators). General laxity in monitoring and control has been cited by many critics. Although this may have reflected a slackness on the part of the administrators, it may also have reflected their correct judgment that (given the nature of the firms, markets and technologies involved) much less formal monitoring was needed than for more diverse programs, such as IRAP. This is a matter of judgment and we suspect the truth lies somewhere between the two positions. The long processing time was also criticized, as was the use of ISB officers and DSS contract administrators to deliver multiple programs. In assessing this criticism, we note at least two trade-offs involving speed of decision. First, speed must be balanced against scrutiny to reduce unwarranted support. Second, it must be balanced against the need to tap outside expertise where it is not available internally. In view of the lack of client complaints about excessively long delays and obvious failures in the selection process, we find no reason to conclude that inappropriate decisions were taken with respect to either of these trade-offs. A further criticism was made, particularly by the AG, of the retroactive support granted to some projects that had already won contracts. We have already discussed this under the flexibility criterion (no. 4 above), where we argued that this was a wholly appropriate procedure given the nature of the problems with which DIPP was dealing.

Conclusion

Having reviewed these criticisms in the light of our structuralist assessment, we find nothing in them to cause us to alter our judgment that DIPP was an overall success in its design and performance. Whatever flaws it had could all have been remedied with relatively minor alterations in design and delivery if the program had not been arbitrarily eliminated as part of an overall cost-cutting exercise.

3. FOUR PROGRAMS TO ENCOURAGE TECHNOLOGICAL CHANGE: IRDIA, PAIT, EDP AND IRDP[39]

This chapter covers four programs that were important parts of Canada's overall technology policy and that were administered by a succession of departments, starting with the Department of Industry, then by Industry and Trade, then by Industry, Trade and Commerce (ITC), and then by the Department of Regional and Industrial Expansion (DRIE). The latest successor of this department is Industry Canada.

The *Industrial Research and Development Incentives Act* (IRDIA) and the Program for the Advancement of Industrial Technology (PAIT) came into existence at roughly the same time (1966 and 1965, respectively). IRDIA replaced section 72(a) of the *Income Tax Act*, which provided R&D tax credits. In 1976, PAIT and IRDIA, along with a number of ITC programs, were rolled into the Enterprise Development Program (EDP). In 1983, two federal departments, ITC and Regional Economic Expansion (DREE), were merged in the new Department of Regional Industrial Expansion; as a result, EDP was subsumed by the Industrial and Regional Development Program (IRDP). The process ended in 1989 with the termination of IRDP and admitted failure.

The final program in this sequence, IRDP, well illustrates the operation of our general evaluative criteria. Study of the whole succession of programs, however, allows us to trace four evolutionary themes. The first is the evolutionary path of program design, starting with a fairly clear design and progressing toward increasing vagueness. The second is the evolutionary path of objectives and acceptance criteria, moving toward ever-increasing generality and vagueness. The third is the evolutionary path of the institutional apparatus, shifting toward ever-increasing complexity. The fourth is the evolutionary path of the institutional competence of the administrators of each program. As a result of the increasing vagueness in all aspects of the programs, more and more discretion was required from delivery officers — discretion that they were not always well equipped to exercise. Furthermore, attitudes and experience of personnel transferred from one program to its successor were not always compatible with the needs of the new, more integrated program.

In section I, we give a brief outline of the history of the four programs. We look first at the *Industrial Research and Development Incentives Act* and the Program for the Advancement of Industrial Technology (which preceded the Enterprise Development Program). We then review the EDP, which eventually was itself subsumed by the Industrial and Regional Development Program. In section II, we discuss others' assessments of these programs. In section III, we apply our own design and operations criteria to assess the programs individually, paying special attention to IRDP. In section IV, we use our criteria to assess the evolution of the whole set of programs. Finally, in section V, we contrast the different factors leading us and other investigators to the same conclusion: that the entire evolution led to failure.

I. HISTORY

A. The *Industrial Research and Development Incentives Act*

The purpose of the *Industrial Research and Development Incentives Act* (IRDIA), which was passed in 1966, was "to assist Canadian industry to undertake new and expand existing scientific research and

[39] Research for this chapter was done by Michele Platje.

development programs and to provide well equipped facilities for such work" (Department of Industry, 1968, p. 7).

IRDIA enabled any taxable Canadian corporation to apply for R&D grants in two categories. First, a firm could claim a 25% grant on any increase in eligible R&D expenditures over the average expenditure of the previous five years. Second, capital expenditures undertaken for the purpose of conducting scientific research and development were eligible for a 25% grant (Tarasofsky, 1984, p. 28). Grants issued under IRDIA were exempt from federal income tax.

Applications were retroactive, although a firm was able to "obtain a prior opinion from the Department of Industry as to whether a grant [might] be payable" (Department of Industry, 1972*a*, p. 7). The application had to be submitted within six months after the end of the applicant's fiscal year in which the R&D took place. A mass of detailed information was required, including a commercial and technical description of the applicant's business, markets and sales, a minute description of its R&D facilities, and a description of R&D projects and programs briefly explaining the goals, methodology and results (Department of Industry, 1972*a*, p. 6). The applications also required a mass of financial and administrative detail about the projects to be supported (Department of Industry, 1972*a*, p. 7). Any support for R&D coming from other sources had to be reported. All assets acquired for R&D through capital expenditures during the grant's year date had to be listed and any subsequent disposal reported.

In 1970, a system of partial grant payments was introduced. Under special circumstances, this allowed companies to benefit from funding before the fiscal year was over. A further amendment in 1971 stated that firms "with an 'acceptable record of performance under the program' could receive partial payment of their ultimate subsidy in advance" (Tarasofsky, 1984, p. 28). In 1976, the program was amended to exclude the payment of subsidies for R&D expenditures made after December 1975. This was done as part of a round of federal cutbacks meant to help fight inflation and recession.

According to the ITC annual reports, there were 638 IRDIA grants in 1969/70 amounting to $23.1 million. In 1970/71 there were 900 IRDIA grants, amounting to $30 million. In 1971/72 there were 874 grants issued, amounting to $31.3 million. IRDIA was terminated in the 1977/78 fiscal year and subsumed by the EDP.

B. The Program for the Advancement of Industrial Technology

The Program for the Advancement of Industrial Technology (PAIT) was established by the Department of Industry (subsequently the Department of Industry, Trade and Commerce) in 1965. As a civil counterpart to the Defence Industry Productivity Program (see Chapter 2) and an extension of the Industrial Research Assistance Program (see Chapter 4), PAIT was meant to "stimulate product and process development" (Smith, 1974, p. 191).

Objectives – PAIT's basic objective was to promote the growth of efficient, competitive manufacturing and processing industries in Canada by providing financial support for product and process development, the results of which were to be marketed "at home and/or abroad" (Tarasofsky, 1984, p. 28). It was intended to provide direct assistance for product and process innovation in all sectors of Canadian industry, promoting product specialization and rationalization based on technical innovation, as well as access to international markets (Senate, 1975, p. 6:49).

The program's detailed objectives were export promotion and import substitution, increasing productivity in manufacturing, helping companies in product specialization and rationalization, reducing the dependence of Canadian manufacturing on foreign technology, encouraging small and large companies to adopt innovative programs and "well thought out product lines with strong future market potential," encouraging innovation in order to "promote and exploit unique Canadian capabilities," and providing "new employment opportunities in industry" (Tarasofsky, 1984, p. 28). This was a rather tall order and, liberally interpreted, it would have covered a very large number of firms and activities.

All Canadian incorporated companies, and consortiums of companies, that met the requirements of the program were eligible for support (Senate, 1975, p. 6:49). Qualifying activities could be in-house or subcontracted, and included "engineering development phase of product and process innovation projects," some non-recurring pre-production, and marketing in the definition of product specifications (Senate, 1975, p. 6:49).

Selection criteria – Projects were "assessed as to their technical and financial resources and facilities" (Senate, 1975, p. 6:49). Firms undertaking projects were to have engineering, production and marketing capabilities with previous work experience in the field, qualified staff or subcontractors. Proposed or existing facilities and equipment had to meet the requirements for project development and manufacturing. The applicant's financial status had to be in good order as assessed by an audit of its financial statements for the past three years, its available resources and the adequacy of its accounting system. Each project had to be based on scientific principles, with a plan showing the technical and staffing ability to carry it out. A marketing plan was also required, addressing such matters as commercial viability and export possibilities.

Forms of assistance – Allowable costs included current costs (e.g., labour, materials and rent) and some capital costs, which were for general-purpose equipment with uses beyond the project but not including buildings (Senate, 1975, p. 6:52). Applicants were obliged to exploit the results of their PAIT-funded products in Canada. Applicants that did not do so were subject to remedial action by the Minister subject to clauses 5(4) and 10 of the PAIT Assistance Agreement (Senate, 1975, p. 6:53). The title to results and property remained with the company.

Before 1970, assistance was in the form of a grant that was repayable if successful results were achieved. PAIT's payback provisions, combined with the provisions of the federal *Income Tax Act*, made commercial loans more favourable than PAIT support (ITC, 1975, Ch. 6, p. 53; Tarasofsky, 1984, p. 29). This situation was inconsistent with the view that externalities justified support over and above what the market would provide.

In 1969, public hearings were held at which strong criticisms were expressed of PAIT's payback provisions; as a result, the program was revised in 1970. Non-repayable grants of up to 50% of a project's estimated costs were now made available under the program. Beyond the 50% grant, interest-free loans were offered in special circumstances; these were repayable if the project generated positive results. The year 1970 marked a watershed for PAIT. All projects approved before the 1970 revision were deemed part of PAIT I. All subsequent projects were part of PAIT II. The distinction was made in order to keep track of PAIT I's projects that had repayment liabilities stretching into the life of PAIT II. After the 1970 program revision, the number of PAIT projects more than doubled and commitments of funds rose from nearly $13 million to $51 million in one year (Tarasofsky, 1984, p. 29).

Administrative procedures – An applicant's eligibility was assessed by a branch officer and a scientific consultant at the PAIT program office. Financial assessments were carried out by the ITC Financial Services Branch. Certification was provided by a project officer or (on that person's recommendation) by the Director/Chief of the Industry Sector Branch (ISB). Before approving a project, the Financial Analysis Directorate evaluated the cost-accounting records and systems of the company involved. The PAIT program office then decided whether or not to encumber funds, after which the Deputy Minister had to give the final approval (Senate, 1975, pp. 6:55-56). A PAIT advisory committee, which was interdepartmental at the Assistant Deputy Minister level, oversaw the assessment of projects (AG, 1978, Ch. 13, para. 20). Monitoring of all projects then fell under the monitoring mechanisms of the line branch.

Evaluation of projects – ITC listed a number of criteria for evaluation. The Department stated:

The results of a project are related to the economically regenerative aspect of Industrial R&D:

- sales of price and performance competitive products of unique Canadian design in large domestic and export markets;
- value added as a measure of economic output and growth resulting from the project;
- benefits such as increased employment, establishment of new capital facilities and equipment for manufacturing of the developed product, upgraded employment skills, and advanced management and marketing techniques related to product innovation as a factor in modern business enterprise. (Senate, 1975, p. 6:58)

Results – The number and dollar amounts of all PAIT projects are given in Table 2. In the 1975 report to the Senate, the Department detailed the results and achievements for the period 1965–75, which included 879 approved projects with over $477 million in expenditures. PAIT's share equalled $238.5 million and 447 completed or terminated projects as of December 31, 1974. Out of 447 completed or terminated projects, 194 were expected to achieve sales and the remaining 253 failed in either a "technical, marketing or financial sense" (Senate, 1975, p. 6:58). Most of the failures were attributed to commercial rather than technical reasons (Tarasofsky, 1984, p. 81). This is a high failure rate; the reasons cited suggest insufficient scrutiny of the commercial possibilities of projects that may have proved successful technically.

Table 2
Number of PAIT Projects and Dollar Amounts

PAIT I			PAIT II		
Year	**No. of projects**	**Dollar amount**	**Year**	**No. of projects**	**Dollar amount**
1965/66	16	400,000	1970/71	137	13,095,000
1966/67	58	4,600,000	1971/72	152	27,428,000
1967/68	59	6,364,000	1972/73	138	26,537,000
1968/69	43	4,304,000	1973/74	122	25,558,000
1969/70	56	5,290,000	1974/75	91	29,499,000
			1975/76	104	26,900,000
			1976/77	112	25,455,000

Source: Touche Ross, 1981, p. A-24.

C. The Enterprise Development Program

In 1972, Volume 2 of the Report of the Senate Special Committee on Science Policy suggested that "the time [had] come to integrate all specific R&D incentive programs into a single multi-purpose program" (Senate, 1970-77, p. 572). In 1976, the Sharwood Report, *An Evaluation of Industrial Support Programs,* undertaken for ITC, provided the rationale for establishing the Enterprise Development Program (EDP) in that year. This program consolidated seven of ITC's existing programs[40] but not the Industrial Research Assistance Program, which remained in the hands of the National Research Council (NRC).

Objectives – Some of the programs integrated into EDP were adjustment programs, while others were designed to support innovations. As a result, EDP had two distinct objectives: to assist companies in adjusting to changing competitive circumstances and to foster innovation (Touche Ross, 1981, p. 1; Usher, 1983, p. 6).

Instruments – EDP provided assistance to businesses in four ways:

1. Loan insurance of up to 90% (100% in certain situations) was provided to support term loans made by private lenders to manufacturers and processors for restructuring their operations or to supplement working capital when normal financing was not available on reasonable terms (AG, 1982, Ch. 10, para.18). The government's liability under this insurance could not exceed $250 million (AG, 1978, Ch. 16, para. 116).
2. Direct contributions, normally up to 75% of eligible costs, were made for research, development and design projects, where the project represented a significant burden on the company's resources (AG, 1982, Ch. 10, para. 18).
3. Direct contributions, normally up to 75%, were made toward fees for consultants to conduct a variety of studies, including feasibility studies, productivity enhancement studies, and product development and design studies (AG, 1982, Ch. 10, para. 18).
4. Direct loans were made available in limited circumstances (AG, 1982, Ch. 10, para. 18).

Eligibility – Projects were eligible for EDP support in two broad categories: Innovation Assistance and Adjustment Assistance. We shall consider Innovation Assistance first. Eligible projects were classified as follows (Johnson, 1982, pp. 32–35):

1. For proposal development, consulting fees could be claimed as a shared cost of up to $100,000 for potential EDP projects. A maximum amount of $200,000 could be claimed to conduct feasibility studies.
2. EDP could share costs for studies designed to "study user requirements or conduct market testing of new or improved products," up to a maximum of $100,000.
3. For product development and design of projects incorporating new technology in the development of new processes or products, shared costs could be claimed without limitation.
4. Project exploitation to develop a market strategy for consultation could be funded as shared costs of up to a maximum of $100,000.
5. Productivity enhancement to perform feasibility studies could be funded as shared costs up to $100,000.
6. Under Special Assistance for Specific Industries, industries such as tanning or footwear, or those injured in certain tariff agreements, could seek a contribution of 80% of costs with a ceiling of $125,000 for consultation fees.
7. Protective contributions were allowed if consulting fees were incurred to protect Crown interests in EDP projects.

[40] These were the General Adjustment Assistance Program (GAAP), the Automotive Manufacturing Assistance Program (AMAP), the Pharmaceutical Industry Development Assistance Program (PIDA), the Program for the Advancement of Industrial Technology (PAIT), the Program to Enhance Productivity (PEP), the Industrial Design Assistance Program (IDAP), and the Footwear and Tanning Industry Adjustment Program (FTIAP).

8. Pollution abatement technologies were eligible for support provided that "the work represented a significant contribution toward pollution abatement in Canada and the firm [agreed] to disseminate the technology to other Canadian firms."
9. Micro-electronics utilization incentives were provided up to a maximum shared cost of $100,000 for fees incurred conducting feasibility studies in technologies related to micro-electronics.

The eligibility criteria for firms applying for support of projects within any of the above classes were quite broad:

> (1) The firm on a restructured basis must appear viable, i.e., a going concern with 'lasting power,' and
> (2) The cost of the project including exploitation must be a significant financial burden on the firm, i.e., in terms of present net worth, annual cash generation and other resources. (CCH, 1982, p. 43)

Companies with less than $10 million in annual sales could receive up to 75% of the cost of an approved project, and larger companies could receive up to 50% (Palda, 1984, pp. 95–96). Grants were issued to firms that convinced the EDP board that their particular project would be a "significant burden" to undertake without grant support. Administrators looked at the project's profitability, expected sales, employment per dollar of grants, and the "viability of the firm itself." Further, "Awards [were] frequently accompanied by advice or binding requirements upon the commercial policy of the firm." The program emphasized investment in computers and electronics (Usher, 1983, p. 6).

We next consider Adjustment Assistance. This was made available to firms in industries injured by the Tokyo Round of tariff agreements under the General Agreement on Tariffs and Trade, or GATT (Johnson, 1982, p. 33). The assistance came in the form of loans or loan insurance to help restructure manufacturing or processing operations.

The eligibility criteria for Adjustment Assistance were simple:

> (1) The firm on a restructured basis must appear viable, i.e., a going concern with 'lasting power,' and
> (2) The loan with loan insurance must be on a last resort basis, i.e., after refusal from conventional lenders (including the FBDB [Federal Business Development Bank] normally). (CCH, 1982, p. 43)

Administration – Administration of EDP was carried out by the Central Enterprise Development Board in Ottawa[41] and regional branches in each province (Johnson, 1982, p. 29). The board used the staff and facilities of the Department of Industry, Trade and Commerce, allowing both private- and public-sector involvement in decisions to provide assistance to manufacturing and processing industries. The regional branches were restricted to administering applications for corporations with sales of $5 million or less, and where the Crown funding did not exceed $200,000.

Expenditures – Contributions totalled $26 million for the 1977/78 fiscal year, $6 million of which was in the form of direct loans to the footwear and tanning industry (AG, 1978, Ch. 16, para. 116; MOSST,

[41] "The Board consisted of 18 members, of whom 9, including the chairman, were appointed by Governor in Council from the private sector" (AG, 1982, Ch. 10, para. 19). The other 9 were ex officio members from the public service. Three members constituted a quorum.

1981*b*, p. 21). In 1978/79, authorized grants under the program were $37.3 million and loans were $135.6 million (Usher, 1983, pp. 6–7). In that year, the cost-sharing ratio was changed to 75% of direct costs (ITC, 1978, p. 15) and EDP's loan guarantee ceiling was increased from $350 million to $1 billion.

Program amendments – In 1980, EDP was expanded to provide special assistance to the electronics industry (Palda, 1984, pp. 95–96). In 1981, the Micro-electronics Support Program (MSP) was formed as a part of EDP to encourage competition by Canadian manufacturers in international markets (Johnson, 1982, p. 34). The program was administered by the Micro-electronics and Instrumentation Division of the Electrical and Electronics Branch of ITC, with a 1981/82 budget of $7.5 million.

The Industry and Labour Adjustment Program (ILAP) was also established in 1981 as an extension of EDP. The program was designed to assist the community-based adjustment component of industry and its workers, operating with a $350-million budget for the first three years. The program consisted of two sub-programs: the Community-Based Industrial Assistance Program and the Community-Based Labour Adjustment Program.

EDP's outstanding loan insurance totalled $101 million in March 1982, with approved contributions totalling $135 million (AG, 1981, Ch. 2, para. 95). An ITC internal audit for EDP showed that expenditures under the program were authorized at $66 million (AG, 1982, Ch. 10, para. 21).

In 1982, the Support for Technologically Enhanced Productivity Program (STEP) was introduced as an "extension of the Special Electronics Fund (SEF) of the EDP," with a 1982/83 budget of $20 million. EDP was amended to provide for the financing of "market potential studies" that supported the program's purpose (Palda, 1984, p. 95).

D. The Industrial and Regional Development Program

In January 1982, the government announced a major reorganization of the economic portfolio, involving the amalgamation of the departments of Industry, Trade and Commerce (ITC) and of Regional Economic Expansion (DREE) into the new Department of Regional Industrial Expansion, or DRIE (AG, 1982, Ch. 10, para. 16). In 1983, EDP was absorbed into the newly established Industrial and Regional Development Program (IRDP), which combined EDP, STEP, the Co-operative Overseas Markets Development Program (COMDP), the Institutional Assistance Program (IAP), the *Regional Development Incentives Act* (RDIA), the Montreal Special Area Program, and the Magdalen Island Special Area Program (Johnson, 1984, p. 137).

IRDP was born in 1983 out of this departmental merger. It was specified as a five-year program designed to bring together ITC's industrial development and DREE's regional development expertise. The intention was to allow a greater role for regional objectives within Canadian industrial policy. It was also meant to address the Auditor General's concerns about consistency of program delivery and project selection under EDP.

IRDP's objective was to promote regional industrial development, sustained growth and international competitiveness. This was to be done through the support of private-sector initiatives, emphasizing projects, industries and technologies having "the greatest potential for economic return." It represented an attempt to create an ideal program built around the following five principles developed by the Ministerial Program Review Task Force (Nielsen, 1986):

1. Regional and sectoral strategies should be harmonized.
2. Prerequisites to receiving assistance should be assessments of (i) projected success, (ii) firms' development plans and (iii) the need for assistance.
3. Cost and risk should be shared between grantor and grantee (except where the interest was exclusively the government's).
4. There should be "enriched" support where a firm was disadvantaged by size or location.
5. Comprehensive assistance should be provided at the local level, with headquarters acting as a resource for all local service centres.

Although IRDP lasted only five years, it went through a series of revisions and reorganizations. The number and extent of these seem to reflect a struggle between the regional and industrial aspects of the program. Never resolved to anyone's satisfaction was the tension arising from the absence of an overall direction for the program as a whole. Here we briefly present some of the more important aspects of this evolution.

Design – IRDP developed a "four-tier" index based on Statistics Canada's 260 census divisions. The index measured the degree of regional economic development, with tier I containing the most developed census divisions and tier IV contained the least developed.

Originally, IRDP consisted of six program elements, a number of sub-elements and three financial instruments. The Industrial Development Climate Funding element supported projects that provided for: *(a)* studies, scholarships or course development in areas where there was a deficiency of knowledge or managerial/technological capability; *(b)* the establishment of non-profit centres or institutes; *(c)* the provision of specialized services or dissemination of scientific/technical information to commercial operations; *(d)* economic development studies; and *(e)* infrastructure development (DRIE, 1984, p. 2). Assistance under this element was limited to "essentially non-profit organizations carrying on activities in support of commercial operations" (DRIE, 1984, p. 2). Tier I was ineligible for support, while all others were eligible for up to 100% funding.

The Innovation element supported projects aimed at the development or design of new or improved products and processes. It also supported projects that promised "an improvement or expansion of technological capability" or that showed "promise of economic success or strategic importance to a region" (DRIE, 1984, p. 11; DRIE, 1985, p. 5). It contained five sub-elements: *(a)* consultant studies, *(b)* development of new products and processes entailing significant technical risk, *(c)* development of technological capability, *(d)* development and demonstration of products and processes entailing limited technical risk, and *(e)* design. Manufacturing and processing operations were eligible for support, as were tourism operations before November 1984. Under all five sub-elements, the government contribution for tier IV applicants could be up to 75% of eligible costs. Tier I applicants could receive up to 50% of eligible costs (Young and Wiltshire, 1990, p. 11).

The Establishment element supported projects establishing new production facilities in less developed regions. Consequently, tier I projects were ineligible. Eligible tier IV projects could receive up to 60% of plant establishment costs and up to 75% of the cost of related studies.

The Modernization and Expansion (M&E) element was aimed at increasing industrial productivity. It targeted the improvement, modernization and expansion of existing manufacturing and processing operations through either the acquisition of new, advanced machinery and equipment or the adaptation of micro-electronics technology. It had four sub-elements: *(a)* consultants' studies, *(b)* modernization, *(c)* expansion and installation, and *(d)* incorporation and application of micro-electronic devices. Under the four sub-elements, tier IV projects were eligible for cost sharing up to maximums of 75%, 50%, 50%

and 75% respectively. The cost-sharing maximums for tier I projects were 50%, 50%, 25% and 25% respectively.

The Marketing element was designed to help identify, develop and exploit new domestic and international market opportunities, as well as to enhance competitiveness within existing markets (DRIE, 1985, p. 8). It consisted of two sub-elements: marketing and consultant studies. All tiers were eligible for assistance, with tier I having a cost-sharing maximum of 50%, while the tier IV maximum was 75%.
The Restructuring element was aimed at changing the products, methods of production, services, markets or management procedures of a commercial operation in need of renewal (DRIE, 1984, p. 11). Projects in all four tiers were eligible for restructuring assistance, with cost-sharing maximums of 50%, 60%, 75% and 75% respectively.

Funding – IRDP possessed three different funding instruments: (1) grants for a study, a scholarship or the development of courses relating to industrial development; (2) R&D support, which might be fully, partially, conditionally or specifically repayable or unconditionally non-repayable; and (3) participation loans (DRIE, 1984, p. 9). Regulations defined the appropriate form of assistance for each project. For example, under the Industrial Development Climate element, assistance for studies was to be provided in the form of a grant, while all other eligible project-types were to receive contributions. Repayability was determined according to IRDP policy and was conditional on the level of technical risk of a project. By and large, the lower the technical risk, the more likely it was that repayment would be required (DRIE, 1984, p. 9).

Design revisions – In March 1984, nine months after its inception, major revisions were made to IRDP. The Industrial Development Climate Funding element was eliminated on the grounds that efforts to encourage the development of industrial infrastructure were best undertaken through federal-provincial agreements. The Restructuring element was also eliminated, this time on grounds of lack of use. Tourism operations were made ineligible for support. Tier I areas were excluded from support under the Modernization and Expansion element. Finally, the cost-sharing maximums were reduced under all elements and in all tiers (DRIE, 1985, p. 8). Too little time had passed for these extensive revisions to have been a reaction to evidence accumulated on the program's performance. Since the revisions coincided with a party change in the federal government, and with changes within the Department, a policy change seems the most likely explanation.

In April 1986, the responsibility for innovation projects with eligible costs of less than $100,000 was transferred from IRDP to the NRC's Industrial Research Assistance Program (IRAP).

In June 1987, all projects with eligible costs of $20 million or less in New Brunswick, Nova Scotia, Prince Edward Island and Newfoundland became the responsibility of the newly formed Atlantic Canada Opportunities Agency (ACOA). In Manitoba, Saskatchewan, Alberta and British Columbia, a second new body, the Western Diversification Office, was delegated authority similar to ACOA's, beginning in August 1987.

Delivery – Project approval was determined according to statutory criteria, rules and regulations. DRIE established a large set of administrative directives to serve as a complete checklist. Although the directives were intended to provide flexibility in information requirements on each application, they turned out to be an administrative straitjacket. While project officers were required to assess all projects, regardless of size, against these criteria, they were also encouraged to "let common sense prevail" when evaluating project proposals. Common sense was to be tempered by the use of established discretionary criteria, such as incrementality, commercial and economic viability, significant economic benefits to Canada, and value for money. Project officers were instructed to "tailor the depth of analysis to the size and complexity of the case" (DRIE, 1984, p. 2-1). Further, IRDP policy directives required that the assessment of a project always take into consideration "certain factors." For example, incrementality was to be assessed in relation to the applicant's "technological capability ... ability to raise necessary financing on reasonable terms [and] need for support, such as retaining its competitive position" (Young and Wiltshire, 1990, p. B-4).

Many of the external program evaluations that we consider below found that, far from exhibiting the intended flexibility when selecting and approving applications under the innovation element, IRDP delivery officers applied a narrow and rigid definition of innovation that focussed on high-technology projects. This approach had adverse effects on the less developed tiers. For example, tier IV accounted for only 4% of IRDP innovation funding, and the Innovation element for only 1%.

The Ottawa-based Program Development and Operations Branch (PDO) was responsible for ensuring national co-ordination and consistency in program delivery by providing policy advice and interpretation. PDO developed the IRDP Policy and Administrative Directive, and operated a "hot line" service for regional staff. The Program Policy Management Committee (PPMC) played a similar role in co-ordinating IRDP's delivery. The committee brought together representatives from the regional offices on a bi-monthly basis to exchange views and discuss issues in program delivery (AG, 1985, Ch. 12, para. 29). However, it did not possess any decision-making authority. These centralized co-ordinating activities were eliminated in a later restructuring of the program.

IRDP was delivered by DRIE's 10 regional offices, with authority for project selection and approval delegated to the local staff. In almost all instances, regardless of the level of eligible projects costs, program reports were to be developed in the regional offices. Before September 1984, approval authority for projects with eligible costs up to $100,000 was delegated to the Regional Executive Directors (RXDs) of each office. They were also given discretionary authority to reject applications of up to $500,000. Projects above $100,000 required Ministerial approval. Projects involving support of $1 million or more were reviewed by the DRIE Internal Board, which was chaired by the Deputy Minister and composed of Assistant Deputy Ministers from DRIE headquarters.

The complex design of IRDP's delivery system appears not to have been followed in practice. According to Young and Wiltshire, no projects were actually approved under the established delegation of authority until July 1988, less than one year before the program expired. The Ministerial power to override selections and approvals was used extensively and, in practice, the delegation of authority for project approvals seems to have been undermined.[42]

After September 1984, approval authority for projects of between $100,000 and $1 million was transferred to the Minister of State for Small Business. In 1987, RXDs were delegated approval authority for projects entailing contributions of up to $250,000, while their discretionary rejection authority was increased to $1 million. Directors General were delegated approval authority up to $100,000, while Directors could approve up to $50,000. This delegation of authority was conditional on any project's not being subject to Ministerial approval. The Minister of State for Small Business was left to approve projects of between $250,001 and $1 million. Contributions of over $1 million, as well as all participation loans and loan guarantees, were to be approved or rejected by the DRIE Minister, with the Internal Board conducting reviews of discretionary rejections. Over $10 million, Treasury Board approval was required, while projects over $20 million required approval of the Cabinet Committee on Economic and Regional Development, or CCERD (DRIE, 1984, pp. B47–49). In August 1987, all

[42] The Minister's power of inquiry had the effect of making any project subject to Ministerial approval as soon as the Minister's office made an inquiry. DRIE also issued letters of notification to the MPs from whose constituencies applications were received.

commitment authority below the Deputy Minister level was withdrawn from the staff. As of June 1988, however, RXDs possessed the reduced authority to approve or reject projects up to $100,000. In 1989, as required by the original sunset clause, the program expired.

II. ASSESSMENTS BY OTHERS

A. Evaluations of IRDIA and PAIT

1. Auditor General

In 1978, the Auditor General examined all ITC programs and activities that involved grants and contributions, with the exceptions of activities of the Standards Council of Canada and Statistics Canada. The AG found many instances of surprisingly sloppy administration. The process of submitting, evaluating and approving contributions under many programs involved several divisions and branches of ITC, which led to confusion (AG, 1978, Ch. 13, para. 18). In some cases, files lacked essential documents; in others, no files could be produced. Consequently, it was difficult to establish definitely what procedures had been followed and what controls had been exercised (AG, 1978, Ch. 13, para. 18). The minutes kept of board meetings were inadequate (AG, 1978, Ch. 13, para. 18). There were no formal audits of the projects under PAIT (AG, 1978, Ch. 13, para. 18). In some instances, the required letter of agreement did not appear to exist. Furthermore, it was often difficult to identify signing officers of existing letters of agreement, because either the letters were not signed or signatures were not distinct and titles (which would have allowed the eligible signature to be identified) did not appear below the signatures (AG, 1978, Ch. 13, para. 1). The length of time it took to process an application was questioned, and time lags of a few weeks to several months were noted between the approval of contributions and the preparation of commitment requisitions (AG, 1978, Ch. 13, para. 1). There were cases of inadequate control over revisions to approved applications.[43] In addition, the AG found that the administration and control of the PAIT I program had not been effective in ensuring the maximum repayment of contributions to the Department on a timely basis[44] (AG, 1982, Ch. 16, para. 124). He also found that a problem in collecting refundable contributions under PAIT I had carried over into the operation of EDP (AG, 1982, Ch. 16, para. 118). The firms that had been supported under PAIT I were still required to repay their loans even after it had been accepted under PAIT II that the earlier repayment requirements were too stringent.

[43] In one instance, the Advisory Committee for the Program for the Advancement of Industrial Technology approved a submission of $97,500. Subsequently, the company involved presented a new request for $351,400. A submission requesting $353,600 received Committee approval although it erroneously included part of the funds already committed on the basis of the original submission. This brought the total approved to $451,100, although the financial analysis of the estimate recommended only $379,800. Despite what had been approved, an amended commitment requisition was prepared for a different amount, $383,000, without further reference to the Advisory Committee (AG, 1978, Ch. 13, para. 18).

[44] Under the PAIT I program, 200 projects were funded for $23.4 million. The Department's analysis of these projects indicated that 68 were terminated ($4.4 million), 21 had been fully repaid ($1.0 million), 10 recipients became bankrupt ($1.3 million), 43 had signed repayment agreements but had not fully repaid ($10.2 million), and the repayment status of 58 projects was not resolved ($6.5 million). Approximately $5 million of unpaid interest was outstanding on projects with repayment agreements (AG, 1982, Ch. 16, para. 123).

2. Sharwood

The Sharwood report made a number of criticisms of the PAIT program. The report argued that $125 million spent by the program from 1970 to 1975 on R&D and technical improvement would have been spent regardless of government funding (Sharwood, 1976, pp. 6–7). A study of loans issued through PAIT I could not find significant evidence that companies invested more of their own funds than they would have done without the government support. However, an Intra-departmental Committee Report judged that PAIT II was "moderately successful" since firms generally increased R&D investment by $0.96 for every grant of $1 (Sharwood, 1976, p. 32). The report noted that a considerable proportion of PAIT funding went to large firms, most of which would have proceeded without government support (Sharwood, 1976, p. 9). These grants failed the narrow test of incrementality. There was also no evidence that PAIT projects were consistent with the broad test of incrementality. This finding contrasts with that for a number of IRAP projects, discussed in the next chapter, where even if projects would have proceeded without the grant, deliberate changes in the facilitating structure were induced by the government support. The report also argued that not enough money was given to the many small firms that might have benefited more from it than the larger firms to which the support was going.

Sharwood made the common identification of incrementality with high risk and argued that there was evidence of this in the grants to small firms, since many of them were for very risky projects with a high failure rate. Of the 196 projects supported, 56% involving $18 million in government expenditures were not successful. The report recommended that the program give increased grants to small companies with limited financial resources so that they might engage in projects involving high technical risks.

While admitting that PAIT had some moderate success in achieving its objectives, the Sharwood report recommended that tax stimulation would be a better method of encouraging R&D as it would serve more firms than did selective grant programs. Furthermore, the report stipulated that grant money should be designated only for high-risk ventures, while support for R&D in larger firms with low risk should be sought through other means.

The report criticized the overly long processing time for applications, as well as stacking of grants. It noted several instances in which firms were able to acquire funding support for a single project from a number of different ITC and other government programs.

Another important criticism concerned poor monitoring procedures, particularly when projects deviated from original plans. Despite provisions allowing it to redeem funds, the Department rarely took action to do so even when the money was used in a way that was felt to be inappropriate. The Department offered two reasons for this behaviour. First, since the firms involved had committed considerable funds of their own to projects, the decision to deviate from project plans must have been in the firms' best commercial interests. Second, officials felt that they did not have the expertise necessary to judge the plan changes (Sharwood, 1976, p. 31).

B. Evaluations of EDP

1. Auditor General

The Auditor General studied EDP in 1982. His report states that the examination "disclosed a number of serious problems and weaknesses in the Program's system of financial management and control. Although certain problems may be attributable to the newness of the Program, *many weaknesses were inherited from the predecessor programs*" (AG, 1982, Ch. 16, para. 118; italics added). The review of

EDP suggested that objectives and strategies were not clearly documented or defined, and that this situation undermined the credibility of the program, which could effectively commit the government "to more than $11 billion in loan insurance" (AG, 1982, Ch. 10, para. 4).

Many more specific criticisms were made. The regional offices' evaluations of the financial viability of applicant firms were unsatisfactory because project officers were not required to have training and competence in financial analysis (AG, 1982, Ch. 16, para. 119). Parliament was not being given sufficient information to judge the magnitude and impact of the program since information was not supplied on items such as authorized ceilings, premium revenues and losses for the year in the Estimates and Public Accounts (AG, 1982, Ch. 16, para. 131). The program procedures did not ensure that all projects would be subject to audit. Indeed, program officers routinely failed to follow up on the cases that were most suspect, i.e., those that did not apply for the last tranche of their grants (AG, 1982, Ch. 16, para. 131). In order to acquire the last 10% of an EDP grant, the project was subject to an audit. Any project awaiting the last 10% of its grant was placed in a holdback liability account. In January 1978, the holdback account amounted to approximately $3.5 million, representing at least $35 million of disbursements. Many holdbacks dated back several years.[45] Since this money was available to projects already approved and completed if they passed reasonable tests, the failure to claim the closing grants suggests undisclosed problems with many of the projects — problems that the lack of departmental follow-up left undiscovered.

In discussing the general benefit to Canada, the Auditor General concluded:

> In our 1982 audit of the Enterprise Development Program of the former ITC Department, we noted the absence of guidelines defining what was meant by "economic benefit to Canada." At that time, the Department indicated that this issue would be addressed in the new program under development following the merger of ITC and DREE. However, the new Industrial and Regional Development Program was launched without these guidelines being finalized and implemented. As a result, we observed that projects were funded where evidence of the need for DRIE assistance was questionable, and that the statements of expected benefits were often increased beyond a level that was supported in file documentation. (AG, 1985, Ch. 12, para. 14)

2. Tarasofsky

In assessing EDP, Tarasofsky reviewed six case studies (Tarasofsky, 1984, pp. 27–40). One company was awarded a subsidy on a project with a predicted rate of return higher than the average return to the company's other operations. A second project was funded in order to allow the firm to pay off debts before the project could proceed. A third firm received some funding for a fairly routine project that was expected to yield high profits. A fourth firm that suffered liquidity constraints received funding for a project that was expected to produce a low return. These four seemed to represent either marginally acceptable or wholly inappropriate use of the program's funds.

A fifth project's aim was to improve an existing product sold in the export market. It was a relatively risky project with an expectation of low returns, and was therefore unlikely to have proceeded without funding. A sixth project's aim was to support a leader in the high-technology field, with a proven track record of using government support programs. The project expected high rates of return. It appears that liquidity considerations were the determining factor in granting the subsidy to this project (Tarasofsky, 1984, p. 37).

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[45] For example, one project, amounting to $58,180, had been in the account for five years; another, amounting to $100,000, had been there for four years (AG, 1982, Ch. 16, para. 128).

Tarasofsky suggests that non-subsidy-type government funding would have been more efficient than funding by EDP. He suggests that the nationality of firms and the level of technology of the industries concerned were, but should not have been, factors in the decision to grant subsidies. As he sees it, one of the major problems with EDP's granting of subsidies was that the process was firm-oriented rather than project-oriented.

3. Touche Ross

The 1981 report by Touche Ross and Partners contained an evaluation of EDP's rationale and design, as well as a comprehensive audit of the efficiency of program delivery, organization structure, human resources, management information, regionalization and risk management, as well as a compliance audit. On program delivery, many of the board members expressed the feeling that the program did not have enough innovation projects, although there was no consensus on whether these should be targeted at high technology. There was also a strong opinion that the program should avoid increasing the number of projects "for companies in serious financial trouble" (Touche Ross, 1981, p. vi). Difficulties included "a lack of clear policy guidance on the appropriate types of projects in various industry sectors," confusion as to what was an innovation and what was an adjustment program, "unclear and contradictory criteria for selection of projects," and the lack of a basis for comparing projects (Touche Ross, 1981, p. vii). The organizational structure led to ambiguity in responsibilities and inconsistencies in the application of the program's criteria. These in turn led to an inefficient and lengthy negotiation process (Touche Ross, 1981, p. xxvii). "The criteria regarding project eligibility [were] interpreted in varying ways by the different Boards" (Touche Ross, 1981, p. xxix). It was impossible to assign program accountability to any one responsibility centre (Touche Ross, 1981, p. xxxi). Program officers had no incentive to monitor projects once they had been approved, partly because they were no longer directly accountable and partly because emphasis was placed on the preparation of submissions (Touche Ross, 1981, p. xxxi).

The report found several problems with the efficiency of EDP delivery. The delay time in the processing of applications was excessive,[46] although the caseload per officer was low.[47] The documentation required for an EDP application was excessive for small projects and insufficiently focussed. There was also no hierarchy of approval limits within the program, a fact that caused significant delays, especially in smaller projects (Touche Ross, 1981, p. xxvi). Each project required up to six different approvals. Program officers lacked proper training and expertise (Touche Ross, 1981, p. xxxiv).

No one organizational unit of the program had the complete skill set required to deliver the program, and training was not available when required. "The management information used to produce operational and program plans [was] inadequate. Monitoring information was found to be lacking and there were inadequate controls over outstanding loans, loan guarantees and contributions" (Touche Ross, 1981, p. xxxvi). Assessments were focussed mainly on financial viability of the firm. Only innovation projects were assessed for technical viability, and this was done with inadequately trained personnel. Marketing considerations and the financial viability of the specific project were largely ignored (Touche Ross, 1981, p. xlii).

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[46] The delivery time from the initial inquiry to the signing of the contract was 35.6 weeks. Standards in other institutions varied from a low of 4 to 6 weeks, to a high of 13 to 26 weeks (Touche Ross, 1981, p. xxiii). Delivery times for EDP were generally longer than for its predecessor programs. The users of the program were dissatisfied with the time required for processing.

[47] The caseload per officer was low: 2.7 new projects per person-year for central projects, and 12.2 new cases per person-year for regional cases. The caseload for monitoring projects was 7.7 per person-year for central cases, and 22 per person-year for regional cases (Touche Ross, 1981, p. xxiv).

Touche Ross attempted to estimate the degree of incrementality under EDP by enumerating the number of programs considered to be high-risk (they thus used a proxy measure of incrementality derived from the narrow sense of the term). They found that only 18% of central projects and 8% of regional projects were rated as high-risk, and concluded that there was little incrementality (Touche Ross, 1981, p. xliii). Since a majority of applicants denied assistance under EDP went ahead with their projects, Touche Ross concluded that the program was not incremental (Touche Ross, 1981, p. xlv).

Finally, the report noted that external factors (including political/regional issues and other ITC personnel) seemed to have influenced board approvals on some projects: "There was a common concern by Board members and officers about the issues of political interference and conflict of interest by Board members" (Touche Ross, 1981, p. xlvii).

C. Evaluations of IRDP

IRDP was reviewed by a number of experts including the Auditor General, the consulting firm of Young and Wiltshire (1990), DRIE's Operations, Audit and Evaluation branches (1986, 1987, 1988), the Task Force on Federal Policies and Programs for Technology Development (1984), the Ministerial Task Force on Program Review (1986), and Atkinson and Powers in their paper "Inside the Industrial Policy Garbage Can: Selective Subsidies to Business in Canada" (1987).

1. General criticisms

The following points cover what seem to us the main criticisms raised by these evaluations:

1. The speed of introducing the program that became IRDP, and the merger that became DRIE, caused a lack of direction for the program in terms of strategy, client groups, objectives, administration, accounting and revision.

2. The potentially conflicting objectives of regional development and of industrial and innovation policy were not reconciled.

3. The decision-making authority within the program (specifically, the Minister's ability to override all decisions) was a source of inconsistency and carried the potential for political capture.

4. Personnel drawn from the two merged departments appear to have maintained mind sets more appropriate to their original departments than to the new organization.

5. The lack of technical expertise on the part of the delivery officers was a serious defect when judgments were needed about the technical feasibility and commercial viability of projects involving technology transfers or technological change.

6. IRDP's excessive flexibility in terms of self-revision, and its lack of flexibility in other dimensions, caused severe problems of administration. (These evaluations of IRDP tend to focus on the propensity of the program to revise itself. We argue in the next section, however, that there were several dimensions to IRDP's flexibility problems.)

In addition to the above points found in all evaluations, the individual evaluations make several specific criticisms.

Auditor General – The Auditor General focussed on the program's lack of centralized control and strategic direction, and the program's lack of consistent information and records. He found errors in the data for nearly all the approved projects, half of which were considered serious. He also noted discrepancies between project file information and information given to senior management, the Minister, and the Treasury Board for their use in making project approval decisions.

Young and Wiltshire, and DRIE – In addition to noting the major problems listed above, the studies conducted by Young and Wiltshire and by DRIE's internal branches raised further issues. One of the most important was incrementality. Young and Wiltshire defined four levels of incrementality: absolute, high, lower and no incrementality.[48] They found that 28% of projects were absolutely incremental, 28% highly incremental, 33% low incremental and 11% not incremental (Young and Wiltshire, 1990, p. 59). Among the rejected applicants, Young and Wiltshire found that IRDP assistance would have been absolutely incremental in 31% of the rejected projects, highly incremental in 33%, low incremental in 23% and not incremental in 13%. They conclude, from this, plus analogous data from EDP and IRAP, that IRDP had a perceived impact on clients similar to its predecessor, EDP, and its contemporary, IRAP. However, DRIE's internal evaluation found IRDP's incrementality to be lower than IRAP's.

On the basis of views expressed by project officers surveyed, Young and Wiltshire conclude that IRDP's application process, which stressed business plans and required detailed knowledge of why companies needed assistance, was biassed against truly innovative projects and incrementality. They argue that the uncertainty associated with significant technological advances precluded providing the detail required by IRDP.

Young and Wiltshire found a limited impact of IRDP on regional economic development. The innovation projects were not generally appropriate for disadvantaged regions, and capital assistance projects were not apparently successful in diversifying economies significantly away from core regional industries. Furthermore, "Projects in disadvantaged regions which strayed from the regions' industrial strengths had very high failure rates" (Young and Wiltshire, 1990, p. 66). Two other studies have also supported these views: the 1987 usage analysis of the IRDP Innovation element by the Evaluations Directorate, and the 1988 study performed by the Operations and Audit Branch of DRIE.

Additional evaluations – The studies conducted by the two government task forces and the analysis by Atkinson and Powers provide some additional views of IRDP. The studies argue that IRDP officers acted in a risk-averse way by selecting safe, low-risk projects. This view is consistent with the observation that IRDP required applicants to lay out in significant detail the project, its needs and its expected returns, both external and direct — something that we have argued is difficult, if not impossible, for many innovation projects. Atkinson and Powers also describe the IRDP decision-making process as representing the "garbage can model" of decision making because of the wide scope of acceptance criteria, the multiple elements and objectives, and the discretion allowed to program delivery officers. They note that this decision-making model, plus a tendency of bureaucrats to be risk-averse, led to the

[48] The definitions are as follows: In the case of *full incrementality*, IRDP assistance was essential to the undertaking of the project. Respondents indicated that not receiving IRDP funding would have had a major negative impact, that they could not then have gone ahead with the project and that there were no alternative sources of assistance available to them. In the case of *high incrementality*, IRDP assistance was highly necessary and important to the undertaking of the project. Respondents indicated that not receiving IRDP funding would have had a major negative impact but that they either could have gone ahead with the project anyway (on a different scope and with different timing), or could potentially have found an alternative source of assistance. In the case of *lower incrementality*, IRDP assistance was less necessary to the undertaking of the project. Respondents indicated that not receiving IRDP funding would have had a *minor* negative impact. In the case of *no incrementality*, IRDP assistance was not needed for the undertaking of the project. Respondents indicated that not receiving IRDP funding would have had no impact at all on the project (Young and Wiltshire, 1990, pp. 58–59).

selection of projects that were safe in terms of commercial viability, but the result was that the narrow test of incrementality was not met. The failure of IRDP both in terms of narrow incrementality and in general leads Atkinson and Powers to advocate framework policies in place of IRDP. They go further than this by passing judgment on all blanket policies, saying that in every case they are inferior to framework policies.

III. A STRUCTURAL ASSESSMENT

We now use our design and operation criteria to assess the experience of the four programs under consideration. In this section we apply our criteria to the individual programs, while in section IV we study the evolution of the whole set. Recall that the tags for the various criteria are only labels; the full description of each is given in Chapter 1.

A. Uncertainty

1. Large leaps are dangerous. 2. Successful policies and programs often pursue incremental innovation and (where possible) aid in the acquisition of tacit knowledge. The designs of all of these programs appeared to avoid the dangers associated with large leaps because they were reactive and demand-driven, with projects being initiated, designed and carried out by the client firms. Especially at early stages, the program administrators were criticized for being risk-averse. Nevertheless, several key characteristics of the programs seem to have encouraged a shift toward big leaps, as shown by the evidence from the various performance evaluations.

First, the narrow definition of innovation used by many of the project officers precluded the provision of the full range of innovation assistance, especially to projects one would associate with the exploitation of established trajectories and the acquisition of tacit knowledge. The small but important technological advances often fostered by IRAP were not encouraged by these programs (Tarasofsky, 1984; Sharwood, 1976; Young and Wiltshire, 1990, pp. 40, 67).

Second, the confusion of incrementality with high risk pushed project officers to seek high-risk projects when trying to meet the narrow incrementality criterion. The result was to focus innovation funding on relatively high-technology and/or high-risk projects when it was not being excessively risk-averse (DRIE, 1988; Sharwood, 1976; Touche Ross, 1981). As a result, small firms were sometimes encouraged to attempt technological advances well beyond their capacity, with a resulting high failure rate. The tendency to seek high-risk projects under IRDP may have been partly a response to the many criticisms levelled at PAIT and EDP for not seeking out enough high-risk projects under their respective innovation support elements. That approach in turn emerged from a mistaken identification of incrementality with high risk (as discussed in Chapter 1). The end result was a combination of safe projects appealing to risk-averse administrators, along with high-risk projects meant to allay the criticism of not having sufficient incrementality. Left to fall between these two extremes were the countless small innovations (incremental innovations as distinguished from radical innovations) that, as we shall see in Chapter 4, have often been successfully encouraged by IRAP.

Third, delivery officers typically lacked sufficient technical expertise for preliminary assessment of projects, and this was rarely undertaken in the early programs. EDP had some of this activity and IRDP more, but the issue of expertise was never successfully addressed. Furthermore, the project officers could not routinely aid the applicant firms in acquiring tacit knowledge, nor were they able to assess what would be an appropriate incremental change in a firm's technology. They thus lacked the ability to push projects in incremental and commercially viable directions. This lack of institutional competence explains several failures as well as the tendency to seek either big technological leaps or safe, zero-risk projects that rarely advanced technology (DRIE, 1988, p. 20).

Fourth, the programs did not provide support for preliminary market research. The lack of support reduced the chances of making incremental developments on established trajectories, and encouraged leaps that were not commercially sound. Surveys suggested that failures were due more often to lack of commercial rather than technical viability.

Fifth, there is some evidence that political influence was a concern in these programs as early as PAIT. Under IRDP, the Minister had override power and exercised his approval authority with respect to 90% of the program's authorized assistance over its five-year life (Young and Wiltshire, 1990, p. 16). All of this could have been expected to encourage the selection of prestigious high-technology projects.[49] Although mistakes were not made in every case, there is evidence of attempts at many large leaps, along with a corresponding failure to pursue incremental innovation or to assist in the acquisition of tacit knowledge. We therefore rate IRDP and its predecessors a qualified failure in these respects. On criteria 1 and 2, they receive a grade of *QF*.

3. Pushing the development of a technology off its established trajectory is dangerous. This criterion is of particular importance to focussed programs, where established trajectories have sometimes been neglected in favour of more spectacular pushes in new directions. Grants under PAIT, IRDIA and EDP often imposed restrictions on the day-to-day operations of firms as well as their financial behaviour (Sharwood, 1976; Touche Ross, 1981). For example, when EDP provided funding support for Consolidated Computers, it insisted on unprofitable behaviour to alter the firm's balance sheet. Later it took over much of the company's day-to-day operation, to the detriment of the entire project (Lipsey and Carlaw, 1996, pp. 317–18).

The extent to which IRDP operated in conformity with this policy criterion is not altogether clear. Its Modernization and Expansion element provided assistance for the acquisition of new, advanced machinery and equipment, which significantly enhanced the productivity of existing activities in manufacturing and processing. Assistance was also available to manufacturers and processors in support of the first installation of proven micro-electronic devices, and for the incorporation of electronic products into products and processes. IRDP-Innovation could support studies to identify opportunities for technology transfer and for the development of technological capability, through projects that did not directly lead to identifiable sales but were of strategic importance to the firm. All these arrangements are consistent with this criterion. However, several of the more general design faults noted in our discussion of criteria 1 and 2 tended to undermine the effective operation of these potentially valuable elements.

As a result of these design problems, IRDP sometimes dictated to the market by pushing technology beyond the capabilities of the firms it assisted. In doing so, IRDP missed chances to help firms exploit established trajectories according to their acquired expertise. Instead, it may have diverted the R&D efforts of some client firms into uncharted territory that they were poorly equipped to explore.

In the cases of PAIT, IRDIA and EDP, there is clear evidence that firms were sometimes forced off their development trajectories. In the case of IRDP, the missed opportunities also argue for failure, although

[49] We were also told that, in a number of cases, the Minister's override power allowed prestigious, high-technology policies to proceed where more mundane incremental projects would probably have been more appropriate. This assertion is consistent with what we know about IRDP's general design and performance, but we have yet to find documentation of specific cases.

the evidence is less conclusive. Overall, we rate the entire series of programs a failure in this respect. On criterion 3, they receive a grade of *F*.

4. Flexibility is important. In Chapter 1, we distinguished flexibility in the design of policies and programs from flexibility in the delivery of specific projects.

Design flexibility – Each of the programs examined in this chapter sought to incorporate flexibility into its design. What actually happened, however, was that vagueness was introduced into the terms of reference of each program. PAIT and IRDIA were early criticized for this vagueness. With the amalgamation of seven ITC programs under EDP, the problem was compounded. By further combining many of ITC's and DREE's programs, the designers of IRDP sought to offer broader, more comprehensive and more flexible assistance than any one of the superseded programs could have done on its own. Furthermore, with the introduction of IRDP, the Department was meant to create terms of reference for its policy in general and for support programs in particular. Unfortunately, the terms of reference were never developed. The haste with which the program was designed and implemented led to a number of design failures that are outlined in our discussions of other criteria. As a result, the hoped-for improvements in performance did not materialize, the terms of reference became vaguer and the administration became more ambiguous than that of predecessor programs.

As we observed in section I, the programs underwent several rounds of fundamental revisions. In the case of PAIT, the changes appear to have successfully addressed design flaws in the way the program interacted with tax regulations and IRDIA. In the case of IRDP, however, the revisions occurred too soon to reflect a reasoned response to lessons learned in operation. IRDP continued to redesign itself, although only in some cases were the revisions related to specific criticisms concerning performance. To some extent, IRDP may have been too flexible in rewriting its rules. This finding reinforces the general lesson that flexibility is not an end in itself but is meant to deal with lessons learned from accumulated experience that could not be predicted in advance because of uncertainties. In spite of all its revisions, IRDP never found a way to deal with its several fundamental design flaws. We therefore rate it and its predecessors failures in this regard. On the criterion of design flexibility, they receive a grade of *F*.

Delivery flexibility – The designers of these programs intended to create flexible delivery systems, especially in the case of EDP and IRDP. As noted in section I, the ideal program was to be delivered locally through a decentralized structure, with headquarters acting as a resource for all concerned. In all cases, project approval was to be determined according to statutory criteria. At the same time, project officers were encouraged to use discretionary criteria and to let common sense prevail. Each program also had a built-in mechanism which should have ensured learning from experience. Assistance agreements included conditions such as the submission of progress reports, review of the project at regular intervals, submission of financial statements at specified dates, and maintenance of working capital or equity minimums. The objective was to provide mechanisms through which technical, financial or marketing problems would come to the attention of the Department. Agreements also included the right of the Department to cease funding a project.

As the evaluators of each of these programs noted, however, while a less elaborate form of monitoring would have sufficed, the technical monitoring of innovation projects was inadequate. Furthermore, the Auditor General and the Operations Audit Branch Study (May 1988) found that, in all regions reviewed, the technical monitoring of projects was either ignored or only inadequately performed. The failure was attributed to the shortage of technical expertise and a mind set that simply did not encourage monitoring. This view is supported by the low rate of terminations of bad innovation projects, and by the failure to follow up on firms that did not claim the last 10% of the funds allocated to their projects under EDP.

With respect to project selection, evaluation and other design aspects, the programs seem to have produced inconsistency rather than flexibility. First, under PAIT and IRDIA, delivery was inconsistent as a result of the vague terms of reference, the discretion allowed to delivery officers and the fact that initially personnel were not allocated to a single program. The subsequent melding of several programs under EDP, and the melding of two departments under IRDP, brought together program officers with conflicting mind sets. Second, the flexibility of the discretionary criteria allowed different program officers to interpret the rules and statutes differently, providing another cause of inconsistencies in eligibility and delivery. Third, the hurried creation of IRDP, during the period of departmental merger, probably caused a lack of strategic direction in terms of intended clients as well as in terms of the nature and extent of clients' use of the program.

With the intent of creating flexibility, the program created a mass of rules and regulations regarding such things as who was eligible under which element and for which type of funding, who was to approve or reject projects for what amount of money, and whose constituency was responsible for the project. Also, the programs placed a significant informational burden on applicants. In many cases, firms were required to provide much more detailed and voluminous information than was required by private financial institutions (Touche Ross, 1981). These procedures, combined with the additional requirement to use discretionary criteria and tailor the analysis to the particular project, actually confounded the application and delivery of the program, and ended up reducing rather than increasing its flexibility.

On account of the many failures outlined above, we rate the programs as failures in this respect. On the criterion of delivery flexibility, they receive a grade of *F*.

5. Diversity is one of the best protections against uncertainty. This lesson does not usually apply to blanket programs because they push broad objectives and support many projects. Indeed, given the full range of cost- and risk-sharing assistance offered by EDP and IRDP and their predecessors, the programs should, in principle, have encouraged many diverse experiments.

There is some evidence, however, that the design and delivery of the programs may have limited experimentation. The inconsistent delivery processes created uncertainty about such factors as eligibility and sharing amounts, which served to reduce the number of projects. Young and Wiltshire as well as the Audit Branch cite the view of a number of project officers that uncertainty and low benefits made many clients give up even though they were truly in need of assistance. According to Touche Ross, the terms of reference were so vague and the discretion of delivery officers was so broad that projects could have been turned down or accepted on the basis of the same application information. Thus, the inconsistent manner in which the programs were delivered probably reduced the diversity and number of experiments receiving support.

Sharwood notes that the majority of PAIT funding went to a small number of large multinationals. The Evaluations Directorate also noted (as did the May 1988 DRIE study) a decrease in the portion of project costs from that under EDP, especially for projects undertaken in tier I. It was suggested that lower cost-sharing ratios were in part responsible for the apparent decrease in the use of IRDP's innovation program. In addition, the narrow definition of innovation, which focussed on high-technology projects instead of incremental ones, restrained IRDP from diversifying experiments, particularly in less developed regions.

Although meeting this particular criterion is not usually a problem for blanket-style policies, the evidence of missed major opportunities to create diversity, combined with some actual pressure to reduce it, leads us to rate all the programs as qualified failures in this respect. On criterion 5, they receive a grade of *QF*.

6. Exposure to uncertainty can be reduced by exploiting the interrelation between users and producers. EDP had a consulting element, which could have helped firms exploit the user-producer linkage, the element was used exclusively to help firms work through the maze of the program's application criteria. Under IRDP, both the Innovation Studies and the Marketing elements could have helped firms to assess markets for their new technologies and to obtain feedback from users. In practice, however, these elements seem not to have been delivered by the program officers and so were not used by their clients.[50]

Although not of major importance, the clear failure of the program officers to deliver results with respect to this criterion under EDP, and the same presumed failure under IRDP, lead us to conclude that the programs failed in this respect. On criterion 6, they receive a grade of *F*.

B. Design Pitfalls

7. Multiple objectives are dangerous. 8. Multiple objectives may be sustainable if there are multiple tools. 9. Multiple objectives may be sustainable if they are clearly prioritized. PAIT's objective of promoting the competitiveness of manufacturing and processing firms was to be achieved by improving technology and encouraging import substitution. These objectives could have been complementary if the import substitution objectives were carried out in activities supporting technological innovation where the Canadian firms had a comparative advantage. They would have been in conflict, however, if the Program's efforts protected Canadian industries that had lost their competitive edge.

EDP was an amalgamation of several programs that had two distinct objectives: to support product and process development, and to support adjustment where firms were hurt by GATT agreements. These objectives made explicit the potential conflict under PAIT. Should support be provided to industries that had lost competitiveness or only to industries seeking competitive advantage through R&D and technological innovation? According to Touche Ross, EDP board members felt that too much support was being given to adjustment projects and not enough to innovation projects.

IRDP was supposed to harmonize industrial/technological development with regional development, and to accelerate the processes of innovation and adaptation in all parts of the country. The fundamental flaw, however, was the belief that economically viable industrial/technological objectives could be pursued by the same body that was concerned with regional development. By its very nature, regional development goes against market signals, at least in the first instance. In contrast, industrial/technological programs seek to work in harmony with market forces. Intervention may be required to overcome short-run inhibitors to technological advancement, such as externalities and fixed costs, but the ultimate objective is to assist technologies and industries that are or will become commercially viable — and hence are sited in the most profitable locations. This fundamental conflict was accentuated by combining two types of personnel, each with a mind set shaped by experience with only one or the other of these goals.

Could this conflict of objectives have been resolved by prioritization? Different approaches are required for regional and for industrial/technological development, even when the final object is to establish a region that can stand on its own feet. For this reason, it is virtually impossible to prioritize these two developmental objectives. Either a program/element pursues maximum economic gains by encouraging industries and technologies to become established where they promise to be most profitable, or it takes a longer-term perspective and encourages developments that may pay off in terms of putting a region on its feet over a much longer time horizon. It is difficult, if not impossible, to do both under the same umbrella.

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[50] This statement is based on interviews although we find no documentary evidence one way or the other.

Could the problem of multiple and conflicting objectives have been resolved by using multiple tools? At first sight, the multiple instruments of the programs (six program elements, each containing sub-elements in the case of IRDP) might have seemed designed to achieve the two main objectives. On closer examination, however, not one of the instruments was assigned to a single objective. Instead, each was assigned to various functional categories, any of which could have served all of the program's objectives.

Because there was no prioritization of IRDP's two chief objectives, there was no prescribed hierarchy determining which should dominate in the case of conflict. Add to this fact the delivery officers' different mind sets, the discretion that these officers were required to exercise, and the substantial amount of political interference in the program, and it becomes obvious that the multiple objectives were crippling. For these reasons, the programs rate as clear failures in these respects. On criteria 7, 8 and 9, they receive a grade of *F*.

10. National prestige should be an outcome, not an objective. None of the programs under consideration attempted to foster national champions in terms of technologies or firms. They never became vehicles for explicit pursuit of national prestige, as have many policies, programs and projects throughout the world. They therefore rate as successful in this respect. On criterion 10, they receive a grade of *S*.

11. Policies should avoid capture. We have two pieces of evidence indicating that the early programs may have suffered from the problem of capture. First, there was concern among EDP board members that the program had too much potential for political capture, and some concern that political capture had occurred (Touche Ross, 1981). Concern was also expressed that PAIT and, to some extent, EDP were influenced by factions within ITC itself. This second finding is consistent with the fact that there were several different programs amalgamated under EDP and that the personnel administering the programs were not dedicated to a particular program. Touche Ross concluded that there was evidence for such interference within EDP. Lack of transparency encourages capture, and the AG found that PAIT and EDP had inadequate disclosure procedures so that politicians were not well informed about projects. As a case in point, Touche Ross reported, "Four projects' files could not be found and no explanation was provided as to their whereabouts" (Touche Ross, 1981, p. xlix).

IRDP had a built-in susceptibility to political interference and control. At its inception, public announcements stressed its flexibility and generous funding levels; a flood of applications resulted from this overselling of the program. The resulting high public expectations encouraged political capture, and further complicated the already difficult task of melding two distinct departmental structures and efficiently operating the program (Spence, 1989).[51] The Minister's power to review and veto projects meant that program authority rested in Ottawa, despite claims about delegation and decentralization. The practice of sending letters of notification to local MPs at the time that applications were received also invited political interference.

These programs allowed discretion at all levels, from program officer up to the Minister. IRDP lacked strategic direction because program criteria were not put into operation, and pressures for political capture were further exacerbated by the failure to harmonize the dual objectives. As a result, capture by politicians was nearly inevitable in the case of IRDP, and it certainly did occur on a major scale[52] (see AG's Report, 1987; Young and Wiltshire, 1990; and Atkinson and Powers, 1987).

[51] In contrast, IRAP has avoided the political limelight, in part because it has never been associated with pronouncements about major economic transformations or "new industrial ages."

[52] The AG's Report provides the following example: "In December 1983, DRIE received an application for a $20 million contribution in support of a project in Alberta involving the expansion of an amusement park, and construction of a marine attraction and water theme park to create a major tourist attraction at a shopping mall.

Because of overwhelming evidence of significant political capture, we must rate IRDP and, to some extent, its predecessors as failures in this respect. On criterion 11, they receive a grade of *F*.

C. Structural Relations

12. Attention needs to be paid to the relationship between technology and structure. There are two dimensions to structural problems: (1) internal structure, which is the structure of the program itself, and (2) external structure, which is the facilitating structure being affected by the program, either intentionally or unintentionally.

Internal structure **–** The internal structures of the programs were not well harmonized, nor were the early ITC programs harmonized with each other. PAIT I was designed without consideration of how it would interact with IRDIA and the existing tax regulations; the result was that it had to be redesigned. Once the process of integrating programs began, the conflicts that we have already reviewed arose in the objectives, in the selection and approval authority, in the administrative procedures, and in the mind sets of delivery officers. Furthermore, the programs lacked systematic direction from the top. Although the programs were altered over time, the changes never came close to addressing the serious structural conflicts.

Facilitating structure **–** Structuralist theory reveals several possibilities for encouraging technological change. First, a new technology that is consistent with the existing facilitating structure may be encouraged. Second, changes in the structure may be encouraged in the expectation that they will induce further technological change. Third, a new technology that is at least partly inconsistent with the existing structure may be encouraged while, at the same time, the necessary structural alterations are assisted.

To cope with each of these three situations, substantial institutional competence is required. In the first, administrators must be able to identify technological changes that can be supported by the existing structure. As we shall see in the next chapter, IRAP has operated effectively in the second situation, although this was never among the objectives of the programs reviewed here. In the third situation, even more expertise is required to identify and bring about the needed structural adjustment.

The designs of the early programs (PAIT, IRDIA and EDP) reveal that the structural linkages of a particular project were not emphasized. Instead, the emphasis was placed on the applicant's financial viability. EDP had written criteria allowing for a review of the technical viability of innovation projects, but it lacked the appropriate expertise to carry out such reviews effectively, causing serious difficulties in at least one case of which we are aware. Consolidated Computers was in effect ruined by the program administrators' lack of understanding of the technological and structural relationship surrounding the Key Edit technology (Lipsey and Carlaw, 1996, pp. 317–18).

Two points stand out about IRDP. First, the program appears to have been designed with an awareness of structure. For example, the IRDP Innovation element could have assisted firms to develop needed

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Review by DRIE staff indicated that the project would proceed without federal funding. This made the project ineligible for funding. DRIE also noted that construction had begun in summer 1984. The project was reviewed again in March 1985 by the DRIE Internal Board which recommended against support under IRDP. In July 1985 Cabinet reviewed the project and directed that federal funds be restricted to the Canada–Alberta Tourism Agreement. This agreement had been signed in May 1985 and provided for a maximum of $5 million per project. Funding under this agreement would have required provincial support which was not given in this case. In August 1985, notwithstanding the Cabinet decision, the DRIE Minister offered the company a $5 million contribution under IRDP" (AG, 1987, Ch. 16, paras. 94-96).

technological capabilities or to acquire tacit knowledge through technology transfer and consultant studies. The Industrial Development Climate element could also have contributed to efforts to develop both firm-level and regional structures. However, these elements do not seem to have been used to encourage any adjustments in the facilitating structure. Second, the operation of the Innovation element in less developed regions was inconsistent with the required structural adjustments, since they often encouraged technologies that could not be supported by the existing facilitating structure. Young and Wiltshire note a number of unsuccessful cases in less developed regions, where the attempt at introducing or developing a technology went well beyond the capacity of the region's facilitating structure, and particularly the capabilities of the client firms.

The various internal structural conflicts outlined earlier contributed to these external failures. In particular, the difficult tasks of fitting technologies to the existing facilitating structure, and of changing them where needed, demanded more expertise on the part of administrators, more prioritization of goals, and more co-ordination between instruments and delivery elements than were available.

The programs failed to remove the difficulties in their own internal structures, and they supported projects that were sometimes inconsistent with the existing facilitating structure while making no attempts to encourage required changes in that structure. For these reasons, we rate them as failures in this respect. On criterion 12, they receive a grade of *F*.

13. Policies and programs can play a useful role in inducing and co-ordinating pre-commercial R&D efforts. As noted in Chapter 1, such a role is not a part of programs' design characteristics; instead, it is an opportunity created by them. Since programs cannot do everything, it is not necessarily a criticism that they ignore some opportunities. Nevertheless, grasping this opportunity could have contributed to fulfilling many of the programs' specific objectives. In the case of the early programs, there is no indication that they supported projects designed explicitly to exploit this opportunity. In the case of IRDP, its Industrial Development Climate element did support, among other things, "the provision of specialized services and the dissemination of scientific or technical information to commercial operations." This service might have been used to support the generation of pre-competitive R&D and to disseminate its results. There is no evidence, however, that IRDP sought to design the procedures and commitment mechanisms that would have been needed to focus on this type of research, as has been done successfully elsewhere — particularly by the Japanese Ministry of International Trade and Industry and some U.S. procurement programs.

Since we can find only one relevant program, which has had only a limited relationship to this criterion, we assign the programs the rating of not applicable (*NA*).

14. Policies and programs should seek to maximize positive spillovers. We strongly suspect that, because of the several design flaws already noted, little attempt was made to select technologies by their externalities and their complementarities to those that were already in existence. However, we cannot establish from currently available evidence that this was the case. On this criterion, therefore, we assign to the programs a grade of uncertain (*U*).

D. Market Forces and Information

15. Market forces and the market expertise of private-sector agents should be utilized wherever possible. Market forces were often ignored. For example, under the programs there were some unsuccessful technology pushes, as well as relatively large technological leaps that would not have been attempted under pure market incentives. They failed for several reasons. First, although the programs had the capacity to assist marketability studies, few were undertaken. Indeed, in reviewing IRDP, Young and Wiltshire found that projects emphasized technology push rather than commercial viability partly because there was often no support for preliminary market research. Second, the staff often lacked the expertise to assess the potential commercialization of the projects that they were assisting. Third, IRDP's Regional Development and EDP's adjustment assistance objectives were not consistent with this criterion, since each sought to suppress market signals. Fourth, sharing ratios were often set too high to exert strong market disciplines on clients.[53] Furthermore, as the AG notes, in some cases assistance "was greater than the amount the analysis indicated was needed" (AG, 1985, Ch. 12, para. 50). Fifth, "Awards [were] frequently accompanied by advice or binding requirements upon the commercial policy of the firm" (Usher, 1983, p. 6). Sixth, the confusion of incrementality with high risk led to the pushing of some projects that were too risky to meet any reasonable market test. The programs thus failed in this respect on several separate counts. On criterion 15, they receive a grade of *F*.

16. Information co-ordination and dissemination are important*.* EDP supported consulting, but this was mainly used to learn how to apply to the program. IRDP's Marketing element was designed to support projects for collecting and disseminating information (literature, advertising, trade shows, seminars, market research and analysis), as well as promoting Canadian standards and product specifications to increase the marketing of the products or services of commercial operations. As we have already noted, however, the market research component of the Innovation element, the Marketing element and the Industrial Development Climate element were rarely delivered. A number of factors may be responsible, including the mind set of the delivery officers who came from ITC and DREE, as well as a lack of strategic objectives within the program.

Another problem was that those charged with delivery often lacked sufficient technical expertise, and this deficiency was not compensated for by an IRAP-like technology network (see next chapter). As a result, the project officers could aid applicants neither in acquiring tacit knowledge nor in learning about relevant technologies in use elsewhere. In addition, they were unable to assess what an appropriate technological increment would have been for the firm.

For their failure to collect and disseminate information in ways that other programs (notably IRAP) have shown to be feasible, we rate the programs as failures in this regard. On criterion 16, they receive a grade of *F*.

17. Commercial viability should be sought*.* PAIT, IRDIA and EDP all sought to determine the financial viability of the firm rather than the commercial viability of a given project. Only rarely was the technical viability of a project considered. For this reason, the early programs clearly failed in this respect. On criterion 17, they receive a grade of *F*.

IRDP was oriented toward the support of commercially viable projects. In fact, Young and Wiltshire found that 93% of projects supported were rated as successful by client firms; this was roughly the same success rate that the same authors found in non-IRDP projects. There is some indication, however, that IRDP was less successful than it could have been in supporting commercially viable projects. First, in less developed regions, the Innovation element met with limited success resulting, as we have already noted, from the narrow definition of innovation applied as well as the technology pushes that occurred in some instances. Furthermore, as most assessors have noted, the application and processing procedure was lengthy and confusing for clients, possibly creating uncertainty about the program. Finally, the lack of technical expertise on the part of the delivery officers hindered the selection of technologically and commercially viable projects.

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[53] For example, with a 75% government share, a $100,000 project that yields only a 2.5% return yields 10% to the private-sector funds, while a total failure may still be acceptable to the clients if they can take out more than $25,000 in salary and other emoluments.

These somewhat conflicting signals lead us to rate IRDP as uncertain in this respect to (although we think a strong case could be made for rating it a qualified failure). On criterion 17, it receives a grade of *U*.

18. Policies should exploit as much expertise as possible. We noted that the programs lacked both in-house and outside technical expertise from either governmental or private-sector sources. They had neither an IRAP-like network nor a set of linkages and partnerships with other key science and technology actors. They engaged in some technology push, dictated some technological and business practices to firms, and may also have ignored or overridden the technical expertise of client firms. Accordingly, the programs clearly failed in this respect.[54] On criterion 18, they all receive a grade of *F*.

19. Competition-inducing mechanisms increase the chances of success. We find some weak evidence that EDP discouraged competition. Interviews with EDP board members suggest that the program was supporting too many adjustment projects and not enough innovation projects. To the extent that this support was for firms already lacking competitiveness in international markets, EDP was guilty of suspending competition-inducing mechanisms. Furthermore, Touche Ross found that, up to 1981, roughly 40% of the applications to EDP were for what was described as "staying in business," rather than for new R&D or innovation (Touche Ross, 1981, p. vii).

By and large, we find no strong evidence that IRDP either encouraged competition, as some national policies have done (particularly in Japan), or discouraged it by selecting national champions among firms or technologies. IRDP's twin objectives of industrial/technological and regional development seem to be the only design facets that are related to this criterion, and they create a clear conflict. Regional development tends to suppress competition, while industrial/technological development with a focus on international competitiveness tends to support competition.

We have no way of knowing which of these conflicting objectives dominated on balance in each of the programs. There is, however, some evidence (at least in the case of EDP) that competition was not a paramount consideration in a number of the applications. Thus we rate the programs as qualified failures in this regard. On criterion 19, they receive a grade of *QF*.

IV. A STRUCTURAL ASSESSMENT OF PROGRAM EVOLUTION

A parallel section to the present one is not found in our other chapters because this is the only sequence of programs we have studied where one evolved into the next. This circumstance gives us a chance to see what lessons can be learned specifically from the process of program evolution as opposed to the design of individual programs. Most of the points that we use here have been noted in other contexts. Here, however, we bring them to bear on the issue of multi-program evolution.

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[54] These views have been supported by several studies. For example, the May 1988 Audit Branch study of IRDP's Innovation element found that DRIE's delivery officers, on the whole, did not possess sufficient technical expertise. The IRDP delivery process did not allow for sufficient technical review, and technical expertise was not available for project analysis (DRIE, 1988, p. 20). Other studies have argued that the manner in which IRDP actually operated gave politicians and senior bureaucrats a great deal of selection and approval authority, which led to the choice of high-technology and high-prestige projects over some more mundane, incremental ones. Another result was that international competitiveness and regional development objectives were brought into conflict (see Young and Wiltshire, 1990, p. 40; Evaluations Directorate, 1987, p. 2) and sometimes allowed regional considerations to take precedence over economic ones.

The evolution of policy from relatively specific programs, such as PAIT and IRDIA, through to the broader program, IRDP, with its integrated objectives and administrative personnel, highlights a common theme: the characteristics of any current program emerging from an evolution of past programs are path-dependent. The specific objectives and the administrative apparatus of a technology program will be dependent on the characteristics of the programs out of which it has evolved. As with any path-dependent evolution, the end result is likely to be quite different from, and often inferior to, what would have been designed if the final program had not been constructed piecemeal from predecessors. Of course, the designers of a new program can learn lessons from past programs. But if a new program is to be devised, its structure of goals, criteria and institutions needs to be designed to fit the program. Where policy makers have tried to build a new program by amending existing structures, the results have usually been unsatisfactory.

A. The Evolution of Design and Objectives

We have already observed that the generality and vagueness of the objectives and evaluation criteria gave the program administrators an undesirable degree of latitude in deciding what to support and what not to support. Of necessity, administrators made on-the-fly policy that should have been clearly enunciated at the policy-making level. Probably the same vagueness was also responsible for the lack of clear organizational structure that Touche Ross noted in their evaluation. In this respect the programs provide a contrast to IRAP, where the organizational structure seems to have grown up in a uniform way to conform to the specific objective of increasing technological competence in Canadian firms. The point is that a well-articulated program objective can help condition the evolution of an efficient organizational structure for the program, even if the details of the program change.

The programs started out as straightforward granting agencies, with financial officers assessing the viability of companies that applied for grants under IRDIA and PAIT. In the move to EDP, several design problems surfaced. First, the new program lacked a clearly defined, well-articulated objective. Second, its acceptance criteria for projects were so vague that support would go to few if any projects if all the criteria were taken as necessary, or else it would go to almost any project if any one criterion was taken as sufficient. This vagueness forced delivery officers to exercise a large measure of discretion, whether they wanted to or not; not surprisingly, it led to many inconsistencies in project acceptances and evaluations.

The problem was compounded in the IRDP integration because another major objective, that of regional development, was added to the industrial development objectives of EDP. The vagueness observed in EDP was meant to be addressed by the drafting of departmental guidelines, which were supposed to clarify the policy objectives of each program within DRIE. As we observed earlier, the guidelines were never drafted. As well as the problems caused by multiple and unprioritized objectives and vague acceptance criteria, there were many problems caused by the conflicting mind sets of delivery officers who were drawn from two previously distinct federal government departments, DREE and ITC. We say more about this below.

Lacking precise objectives, the administrators evolved them case by case. The end result showed a number of important deficiencies. First, the support went mainly to large firms, with less going to middle-sized firms and relatively little to small ones. Second, the reasons for support for large firms were sometimes questionable. One example was the significant expenditure of funds to induce two multinational corporations to perform R&D in Canada. If the aim is high-value production and high-wage jobs, it is not always best to subsidize the location of R&D; what matters far more is the location of the production facilities. Concentration on R&D was in line with some of the conventional wisdom of the time but not a well-thought-out or well-articulated means of obtaining the desired industrial and technological objectives. Third, too much money was spent bailing out firms that were in trouble, or (in the jargon of the program) helping them “stay in business.” This is seldom a good policy and should be carefully and publicly considered whenever it is to be used. It should never be done at the discretion of administrators whose commission is to encourage technological advances. In short, the evolution of program design and selection criteria in this branch of ITC programs went from well-articulated and specific to poorly articulated and vague. The result was that a number of questionable projects were supported.

B. The Evolution of Institutional Competence and Institutional Apparatus

The programs started out as lenders of last resort to companies conducting R&D, and so they were set up institutionally to act like banks. The application criteria and process were based on the practices of private lenders but required more detail in terms of financial information. What the programs appear to have lacked was personnel with technical expertise related to the R&D that they were supporting.

Although EDP did some technical assessments of its innovation projects, the lack of technical expertise on the part of its program officers was cited by a number of critics. In both the PAIT-IRDIA and EDP eras, the main concern of project officers was for the financial viability of the firm rather than the commercial viability of the project.

To compound the already existing lack of expertise, an additional problem arose with the formation of IRDP: the introduction of project officers from the completely different background of regional development policy. Combined with the dual objectives of regional and industrial development within the program, this caused problems in terms of what officers thought was an acceptable application or an acceptable analysis of the project. There was thus a great deal of variation in the technical viability of the projects undertaken under IRDP. Many of them failed.

Under PAIT and IRDIA, the program personnel were never dedicated solely to the running of any one program. Instead, project files were only a part of the daily duties of personnel involved. This approach seems to have been a viable institutional design in the case of the small PAIT and IRDIA programs. However, with the integration of seven programs into IRDP, the lack of focus of the program officers became a serious issue.

Policy makers’ lack of concern over the persistence of institutions and entrenched practices is also seen in the combination of regional development with industrial development under a single institutional apparatus. There were no adjustment mechanisms set up for integrating the two quite different sets of personnel and objectives.

C. Lessons

The discussion of the problems associated with this particular set of evolving programs suggests some more general lessons. These may be applicable to all such processes where programs follow each other in a succession that may or may not include mergers of separate programs.

- A path-dependent evolution of a series of programs will almost invariably end up with a program that is different from, and inferior to, the program that would have existed if the end program had been designed from scratch. The design of a new program should include provisions to design an organizational structure consistent with the new program, rather than constructing it piecemeal from other administrative structures.

- While continual revising and refining of a single-objective program or series of programs may improve it, the continual revising/refining and combination of programs with multiple objectives is likely to produce vagueness and confusion in objectives and selection criteria.
- The entire implementing structure can become less effective when structures devised for different programs are combined.
- Evolution in response to criticisms can often make matters worse. In this case, there were at least two problems. First, the program was criticized at an early stage for being a focussed rather than a framework policy. The attempt to evolve a focussed or blanket policy into a framework policy is almost bound to fail because the policies are different in all respects. A framework policy needs to be designed along with a compatible organizational structure. Such a policy might evolve out of a related framework policy, but not out of a blanket or focussed policy. Second, the Department was often criticized for having too many partially overlapping programs. The solution was to start over with a new organizational design, not to combine the existing programs into a single large, heterogeneous structure.

V. Conclusion

Step 1 – On our criteria, the programs achieved a score of 13 failures, 3 qualified failures, 2 uncertains, 1 success and 1 not applicable. (These do not add up to 19 because on one criterion the early programs of PAIT, IRDIA and EDP rated a clear failure, while IRDP was uncertain.)

Step 2 – The programs seem never to have established clear, precise criteria for what they were trying to do and why. The confusion about incrementality clearly illustrates this lack of clarity. Useful incrementality does not necessarily mean high risk. Of course, if one selects sufficiently risky projects, they will clearly be incremental because no one would ever risk private-sector money on them. To be productively incremental, however, one needs to accomplish something that would not otherwise have been done *and that is worth doing*. If one accepts the case for a general underproduction of research, a framework policy will function more effectively than a policy that is selective. The value of selective policies is that they fill in gaps where private incentives fall particularly short of public benefit. Doing so involves clearly identifying places where externalities are notably large or market imperfections (often caused by what economists call non-convexities) strongly inhibit the generation of socially useful knowledge. These reasons for support do not seem to have been well thought out, and the administrators were left to establish their own criteria for whom to support and why. Since this kind of justification of selective policies is subtle and since its successful application requires much information, it is unlikely that civil servants without a highly specialized training in market failure would happen upon useful criteria. They obviously did not, and so they were left to stumble about in the dark supporting one firm and activity here and denying support to another firm and activity there, without a clear understanding of the underlying reasons for selectivity.

This fundamental flaw in theoretical understanding was combined with many design flaws. The final program in the succession, IRDP, sought to combine into one program two objectives in such fundamental conflict that they would best have been administered quite separately. The inability to reconcile the conflicts within a single program was probably sufficient to produce failure. This problem aside, IRDP had other major design faults that were probably sufficient to make the program unworkable, barring a major redesign and restaffing.

For these and many other reasons outlined earlier, our qualitative judgment is that the entire sequence of programs went from poor to worse in performance, and that the end product, IRDP, was a clear and major failure.

Step 3 – As we noted in section II, the other assessors of the programs have all been highly critical. Their assessments rated the programs as performing poorly at the outset and clearly failing at the end of the period. Since our assessment of the structural design and operation of the programs agrees with the other assessments, and since our criteria showed almost no clear successes, we reach the confident judgment that on balance the programs were failures. This, of course, does not mean that everything they did failed. On the whole, however, we find that, given the ever-worsening design, they could not have succeeded to a major degree.

Step 4 – Since our own structuralist assessment agrees with the assessments made by others, there is no need to reconcile any conflicting results. It is worth noting, however, that our reasons for finding the programs to be failures differ significantly from those given by Atkinson and Powers, the report of the Task Force on Federal Policies and Programs for Technology Development (the Wright Report) and the Nielsen Report. Their findings provide only limited insights into the fundamental reasons why IRDP failed. By and large, they do not substantiate their views with factual analysis of each case. Instead, their analyses are based on an abstract neoclassical theoretical perspective that prefers framework-style policies in almost all circumstances.

The Wright Report clearly advocates framework policies over blanket policies. It argues for the replacement of discretionary subsidies with tax-based measures because subsidies "sometimes [encourage] firms to undertake dubious R&D projects which would be uneconomic without government assistance ... to collect federal money for research they might well have undertaken anyway, [and] ... the growth of bureaucracies whose risk avoiding propensities militate against successful R&D" (Wright, 1984, p. 10). The findings of the Nielsen Report are almost identical.

Atkinson and Powers argue that industrial assistance programs suffer from a lack of economic and political rationale. They favour "familiarity," meaning that there is no incentive to reconsider criteria that have been politically acceptable in the past. Furthermore, in their view, bureaucrats are risk-averse. They argue that the combination of the lack of rationale for giving grants and the risk aversion of bureaucrats implies that "industrial assistance programs will often succumb to the logic of the 'garbage can model of decision making'," where "an opportunity for choice is viewed as a garbage can into which problems, solutions and decision makers are dumped as they become available" (Atkinson and Powers, 1987, p. 209). The result is an unexpected set of outcomes. Atkinson and Powers seek to establish their view that IRDP fitted the "garbage can model of decision making" through empirical measurements of the extent to which IRDP operated in conformity with its regional development objective. They argue that if IRDP had been delivered in a manner consistent with the regional development objective, the amount of funding should have been inversely related to the tier designation for the regions in which projects took place (i.e., more money should have been spent in less developed regions). They find that more funding went to the more developed regions and that larger projects received relatively less funding. They thus conclude that the program conformed to their model of decision making.

These views are given theoretical support in Usher's 1983 review of five federal subsidy programs. Usher argues that focussed policies such as IRDP can be replaced with less costly framework policies, which deliver the same output at lower cost. The argument is based on a neoclassical definition of incrementality that ignores structure and tends to advocate high-risk technology projects because they are easily identifiable as incremental under this narrow view (not because there is a perception of a commercially viable payoff). A fuller critique of Usher appears in the assessment of IRAP in the following chapter.

In our view, Atkinson and Powers are not far off the mark in suggesting a “garbage can model” for IRDP’s decisions. The results could hardly have been otherwise, given the pervasive vagueness in all dimensions that we have documented throughout this chapter. But we disagree with Atkinson and Powers when they argue that such a model of behaviour is endemic in all government programs of the types that we call blanket or focussed. All three assessments that we have mentioned in this section appear to derive their conclusions from a belief (based on a neoclassical theoretical perspective) that framework policies are superior to all other types and that, as far as possible, support for technology should be taken out of the hands of bureaucratic decision makers. This unqualified condemnation of two classes of policies (blanket and focussed) does not distinguish between IRDP, which we also assess to be a failure, and IRAP and DIPP, which we assess as successes. Nor does it suggest how to distinguish between the successes and failures of the focussed policies studied by Lipsey and Carlaw (1996).

4. THE INDUSTRIAL RESEARCH ASSISTANCE PROGRAM

In this chapter we give some highlights of the history of the Industrial Research Assistance Program (IRAP). We then consider the assessments of IRAP performed by others. Next we apply our own method, which leads to the conclusion that IRAP is a clear success. Finally we seek to reconcile the differences between our favourable assessment and the less favourable assessments of several other observers.

I. HISTORY

IRAP was set up within the National Research Council (NRC) in 1961. It was part of the government's response to the belief that Canadian industrial research and Canadian government support for such research were both insufficient to maintain this country as a major industrial nation. IRAP was one program in a larger government effort. It was to be the counterpart to the Defence Industrial Research Program (DIR) of the Defence Research Board, and to Industry, Trade and Commerce's Defence Industry Productivity Program, both of which supported defence-oriented commercial research efforts. IRAP was also meant to complement ITC's Program for the Advancement of Industrial Technology (Tarasofsky, 1984, p. 28).

IRAP's original objective was to "stimulate a build-up of competent research teams in industry," through the funding of "relatively long-term applied research projects in science and engineering which [offered] reasonable potential for achieving major advances" (NRC, 1969, Appendix 2, p. 1; Appendix 1, p. 1). A further objective was to use the personal contacts and co-operation fostered by the program in order to promote greater understanding of industrial research needs and interests on the part of government scientists. The program also sought to promote greater awareness of available government resources among university scientists (NRC, 1964, p. 12).

Throughout its long history, IRAP has gone through a complex evolution of expanding functions and coverage, as well as numerous reorganizations of its administering bodies. Most of this evolution appears to reflect a continuing desire to learn from experience and to respond to altered economic conditions.

Until 1978, IRAP consisted of a single program element of technical and financial assistance, later known as IRAP-P (Contributions to Large Projects). Originally, IRAP assistance was provided to long-term research projects, normally lasting up to five years. Matching grants (with costs shared on a roughly fifty-fifty basis)[55] "covered the payment of the salaries of scientists, engineers and technicians engaged [on a continuous basis] in approved positions on an approved project" (IPO, 1973, p. 9). Soon, client firms' allowable costs were extended to cover contracts with university professors. The object was to develop a mutually beneficial exchange between industry and academia. Industrial projects were to provide university researchers with practical applied science or engineering experience. University researchers were to offer guidance to "young, inexperienced teams" and provide their industrial colleagues with the benefit of a broader (basic) research perspective, either through employment as a member of project research staff or through "seminars, discussion groups, and similar activities" (NRC, 1969, Ch. F, p.10; IPO, 1973, p. 9). During this initial phase, the program's general objective was to

[55] In 1965/66 the average annual grant to projects was $31,000, while the average project length was 4.5 years (NRC, 1966, p. 16).

promote increased R&D capability within Canadian industry through the creation and/or expansion of private-sector R&D teams.

Phase II began in the mid-1970s, with the same overriding objective but broadened means. During the recessions at the beginning and middle of the 1970s, the objective became somewhat more defensive: to maintain as well as to expand “research teams during [this] difficult economic period” (Simmonds, 1983, p. 4). Initially, costs occasioned by subcontracting of research or the use of consultants were ineligible, except under special circumstances (NRC, 1969, pp. 2–3, 6). In the 1970s, “project selection was based on multiple considerations, of which the creation or expansion of research teams [was] but one” (IPO, 1977, pp. 3–4). At that time, consultant’s fees and the subcontracting of research became allowable costs.

In 1978, three new program elements were added. IRAP-M (Contributions to Small Projects) increased assistance to small- and medium-sized enterprises (SMEs) having little or no in-house R&D capability and experience. Financial assistance was aimed at enabling eligible SMEs to solve specific technical problems, either in house or by subcontracting. Furthermore, by demonstrating to SMEs the benefits of using technology, it was hoped that IRAP-M would serve to increase their awareness of, and motivation to use, technology and the available technical resources and support, whether within government or the private sector. The desired long-term effect was to motivate SMEs to build up their own (in-house) R&D capabilities. Eligibility as well as the amount and terms of support were altered as experience accumulated. In short, IRAP sought to create a specific policy tool to address the particular problems of SMEs within the general objective of increasing the technological competence of Canadian firms. This is precisely the useful role that blanket policies and programs can play by tailoring support to a facet of the economy with unique structural characteristics, while at the same time conditioning that support on the general, overall policy objective.

The Science and Engineering Student Program (SESP), the Scientific and Technical Employment Program (STEP) and its replacement, the New Technology Employment Program (NTEP), were wage subsidy programs of a type common in Europe. STEP was aimed at stimulating “the creation of permanent jobs in the private sector for unemployed graduates with scientific or engineering background” (NRC, 1979, p. 40). SESP subsidized the hiring of university students to work on industrial R&D projects. A possible future benefit was permanent employment.

The Manufacturing Assistance Program (MAP), which became IRAP-L (Contributions to Laboratory Investigations), made contributions to laboratory investigations. As with IRAP-M and IRAP-H (Contributions to Firms Employing Undergraduates), this new element appears to have been introduced as a means of creating a permanent policy tool for the delivery of specialized assistance. It covered up to 75% of total costs (not to exceed $6000 in 1983) of small firms subcontracting to “laboratories, institutes, or consulting firms” the task of solving specific technical problems or fulfilling “testing requirements with their products or processes” (Siddiqi et al., 1984, p. 7). IRAP-L, together with IRAP-M, were responses to the growing understanding that small firms were “particularly important contributors to economic growth and employment” (NRC, 1990*a*, p. 3).

IRAP continued its evolution in the 1980s through its third phase. The new emphasis on “technology fusions” and decentralization probably represented as much a new appreciation of, and a response to, changed economic circumstances as it did a need to improve on past performance. The stated objective was to “increase the calibre and scope of industrial research and technological development in Canada ... through facilitating the connection of industry to appropriate technological resources by providing technological advice, information and financial assistance” (quoted in Siddiqi et al., 1984, p. 21). In *A Practical Perspective*, the NRC detailed its plans to remake itself in order to facilitate technological fusion, through the building and maintenance of industry-government-university linkages, technology transfer from consolidated and rationalized NRC laboratories, the co-ordination of multi-sector programs, the undertaking of more joint projects with external partners, and a range of other efforts to "make itself as much a part of the industrial community as ... of the scientific community" (NRC, 1985, pp. 5, 37). As part of this effort, IRAP was streamlined and decentralized.

The newly created LabNet was a vehicle for promoting technology diffusion, joint projects and multi-sector co-operation. IRAP-R (Contributions to Major Projects Involving Technology Transfer) was a new element created by merging IRAP-P with a separate NRC technology transfer program, the Program for Industry/Laboratory Projects (PILP). IRAP-R focussed on "complex, collaborative R&D projects" that were aimed at identifying and exploiting technology transfer and development opportunities in three broad technology areas: biotechnology, agriculture and food; informatics and electronics; and materials and advanced manufacturing (NRC, 1990*a*, pp. 9, 18). To be eligible, projects had to involve some element of collaboration with federal, provincial, university or international partners; there had to be "a group of organizations either informally co-operating or in a formed consortium" (NRC, 1990*a*, p. 9). IRAP-R provided up to 80% of direct salary costs and 50% of supporting subcontract costs, with IRAP's share of total project cost to be roughly 50% (NRC, 1990*a*, p. 9). "A 'typical project' ran for 2–3 years at $100–200 000 per year" (NRC, 1990*a*, p. A-8). IRAP-R also sponsored sector-specific seminars and workshops to assist in forming networks of researchers in specific subject areas from various Canadian and international research institutions (NRC, 1990*a*, p. 9).

IRAP-S, the International Technology Service, was introduced in 1988 to facilitate the transfer of international technology to Canadian firms and "to promote co-operative research and development between foreign owners of the technology and Canadian companies" through private-sector technology brokers (NRC, 1995, p. 2). It was part of the LabNet until 1989, when it became part of the National Elements. In 1990 it was terminated because of the difficulty of administering the element and because it was felt that the form of technological brokering it provided was best left to the private sector (NRC, 1990*a*, p. 4).

IRAP's next reorganization, announced in its 1991 Strategic Plan (SP), appears to have marked the acceleration and intensification of phase III. The objectives are increased decentralization, increased selectivity with an emphasis on "more complex projects with greater technical impact," the consolidation of the program's five elements into "two, more flexible elements," and continued efforts to build and strengthen the IRAP network through "increased linkages with complementary programs and services" (Strategic Plan, 1991).

The Technology Enhancement (TE) element is "designed to allow for support of a variety of small-scale initiatives enabling [SMEs] to demonstrate and improve their technical competence as a basis for more substantive endeavours" (NRC, 1993, p. 7). Up to 75% of eligible project costs, which include the salary costs of subcontractors and consultants as well as some travel-related expenses up to a maximum of $15,000, can be covered under TE. "Feasibility studies, technical analysis and problem solving by consultants" are eligible for funding (NRC, 1994, p. 7). The Research, Development and Adaptation (RDA) element, perhaps more so than the TE element, reflects IRAP's emphasis on decentralization and its desire to attune program delivery and project selection to regional needs and conditions. RDA focusses on more complex projects "involving applied research and development of an unproven nature, as well as the adaptation of technologies of proven merit" (NRC, 1993, p. 3). RDA also seeks to meet the needs of firms lacking "a specific technical expertise or a crucial technology to undertake a project" (NRC, 1994, p. 7). Normally, $15,000 to $350,000 is available over a period of up to 36 months, covering up to 50% of total project costs.

Under the Strategic Plan, IRAP's operational management has been redesigned. Seven Regional Directors (RDs) are now "fully responsible for the management, administration and delivery of all projects within their geographical area" (Goss Gilroy, 1993, p. 11). The discretion of RDs, in tailoring program delivery and project selection to regional needs and conditions, is subject only to the overview of the Technical Assessment and National Co-ordination (TANC) group and the Planning and Administration (P&A) group. Considerable discretion is given to senior Industrial Technology Advisers (ITAs) who deliver IRAP services, and a de facto veto goes to the RD. While approval for large projects is subject to oversight from Ottawa, the RD may refuse to approve contributions of any value. The RD can simply turn down an application for up to $100 000. For contributions in excess of $100,000, the RD "can refuse to forward a recommendation to headquarters," thereby blocking approval of contributions "that must come out of his/her regional budget" (Goss Gilroy, 1993, p. 11).

IRAP's complicated history includes the prior history and eventual merger with IRAP in 1981 of the NRC's Technical Information Service (TIS), which provides technical advice for firms that have no funding. Space limitations allow us only to give highlights of IRAP's history; a chronology appears in Table 4.1 at the end of this chapter.

II. Assessments by Others

The assessments made of IRAP by others range from glowing praise to scathing criticism. For the most part, those giving negative assessments adopt the narrow definition of incrementality discussed in Chapter 1. Many of the positive assessments rely on soft data.

Two common themes clearly emerge from the assessments. First, project officers have the institutional competence and technical expertise to make the program successful. Second, IRAP has had clearly positive effects in terms of its general goal of increasing technological competence in Canadian firms.

A. Circumstantial Evidence

Those providing general surveys of IRAP's performance without detailed measurements have given assessments that are almost universally favourable. The Task Force on Federal Policies and Programs for Technology Development (1984), the Ministerial Task Force on Program Review (1985), the Report of the Standing Committee on Industry, Science and Technology, Regional and Northern Development (1991), and various reports by the National Advisory Board on Science and Technology (1992 and 1994) all praise IRAP. These evaluations are based mainly on circumstantial evidence, with the exception of the Study Team Report for the Ministerial Task Force on Program Review. This document cites a few statistics on improvements in the R&D capability of firms assisted by IRAP, and makes some reference to "direct sales benefits."

The report of the Task Force on Federal Policies and Programs for Technology Development, better known as the Wright Report, referred to the "commonly heard criticism" that "most programs are over-administered" to such an extent as to deter firms from applying and that, contrary "to the spirit of successful industrial research," project approval and monitoring controls are excessive because of the risk aversion of administrators. The report singles out IRAP as a notable exception to this general rule, identifying the expertise and experience of those who administer and deliver it as key to the program's administrative simplicity and its "business-like manner." According to the Task Force, IRAP received unanimously high praise as a program success and thus passed "the overriding test of all technology-related programs in support of industry ... do their intended clients endorse them?" (Wright, 1984, pp. 9-10).

The report of the Ministerial Task Force on Program Review (also known as the Neilsen Report), the Report of the Standing Committee on Industry, Science and Technology, Regional and Northern Development, and relevant National Advisory Board on Science and Technology (NABST) reports all repeat the general conclusions of the Wright Report without, however, providing substantial further evidence. The Standing Committee's report, *An Inquiry into the Industrial Research Assistance Program*, mainly summarizes the conclusions of other studies. Two NABST reports — *Measuring Up to the Benchmark and Moving Ahead* and *Spending Smarter* — praise IRAP. The NABST recommends that consolidation of the government's "*patchwork of ad hoc programs*" (italics in original) should be continued with a view to building an "integrated support system" based on IRAP, in light of "its excellent record of collaboration with federal, provincial and private sector agencies." It attributes IRAP's success, in part, to "the Industrial Technology Advisers who deliver the IRAP services [and who possess] considerable direct experience in the management of technology in the SME" (NABST, 1992, pp. 18–19). *Spending Smarter* examines a number of federal departments/agencies and their programs, with reference to four principles established by NABST as the basis for a federal science and technology (S&T) policy framework. The Advisory Board also argues that IRAP plays an important role in the knowledge and information dissemination necessary to the development of a knowledge-based society.

B. Effectiveness Statistics

The NRC has evaluated IRAP on a five-year cycle, often using independent consulting firms. IRAP's Field Advisory Service (IRAP-C) has received praise from users. For example, in one survey, 86% of firms stated that they had improved their technical capability over a four-year period. Of these respondents, 90% attributed "an important or very important role [to IRAP] in that change" (NRC, 1990*b*, p. 27). The program for hiring undergraduates has also been judged effective. A further report (McGuire, 1983) provides favourable statistics about direct sales benefits, indirect benefits, the ratio of incremental sales to IRAP's contributions, and incremental job creation. Another study undertaken for IRAP (BCM, March 1984) seeks, on the basis of interviews, to determine the degree to which IRAP's information and advice inputs are incremental in the broad sense. Of 67 projects, 33% or 22 were found to be incremental. Commenting on the views of respondents about the importance of IRAP advice, the 1990 *Evaluation* states, "The precise percentages of these results should be treated with some scepticism, since they were given by the program recipients" (BMC, March 1984, p. 25). The McGuire study considers the possibility that respondents were merely giving polite answers. They find this unlikely for three reasons. Interviewer's impression to the contrary, the detailed nature of the information volunteered in the responses and the common response that "simply going through the process of applying for IRAP support made them clarify and improve their research work" (BMC, March 1984, p. 24).

IRAP/NRC itself explicitly recognizes the limited extent to which sales, savings, jobs and so on can be attributed to IRAP. A former president of the NRC, Dr. Pierre Perron, reporting to the Standing Committee on Industry, Science and Technology, Regional and Northern Development, stated, "Strictly speaking, IRAP and similar programs are not the direct creators of continuing jobs in Canadian industry.... Hence, it is more correct to refer to jobs and other benefits as being associated with IRAP rather than due to IRAP" (Senate, 1969, p. 41). He pointed out that sustained employment is the result of satisfied customers' continuing to buy better and cheaper goods and services that result from improved technology, which contributes to customer satisfaction.

C. Incrementality

The major studies that have been most critical of IRAP have all approached assessment from a neoclassical perspective, and have used a narrow definition of incrementality as their main tool of analysis and criticism. These are the three studies by Usher (1983), the Economic Council of Canada (1983) and Tarasofsky (1984). While not attempting an exhaustive coverage of three such major works, we shall try to present some of their major assumptions and criticisms.

Usher provides "a study of a technique of economic policy with special reference to five Federal programs where the technique has been employed. The technique is the firm-specific investment grant designed to influence the amount, location and distribution among industries of investment, broadly defined to include capital formation, research and development. Firm-specific policies may be contrasted with general policies and with market-specific policies" (Usher, 1983, p. 313). Of particular interest here is Usher's designation of IRAP as a "firm-specific policy," and of investment tax credits as the contrasting "general policy." He provides a general criticism of the class of firm-specific policies, including IRAP.

After a brief description of each of the five policies, Usher turns to a discussion of incrementality. He has two lines of criticism. In the first, he uses the ideal definition of incrementality. He concludes that it is impossible to determine incrementality for the programs that he studies, mainly because of the difficulty of measuring social benefits (a criticism that would seem to apply to all programs and policies whenever the ideal definition is used). His second line of criticism strart by reporting that IRAP's supporters claim narrow incrementality on the grounds that "the existence of IRAP enables research projects to commence sooner than otherwise and that the program may be incremental in some especially risky projects" (Usher, 1983, p. 318). Using this narrow definition, Usher argues that IRAP's focussed expenditures should be compared with tax-expenditures under an alternative framework policy such as R&D tax credits. On the benefit side, he argues that any legitimate objective that can be achieved by firm-specific measures, such as those used by IRAP, can also be achieved by framework policies such as tax credits. On the cost side, he argues that focussed policies are more expensive than framework policies because they involve higher bureaucratic transaction costs and more deadweight losses and are more prone to capture. He concludes from these considerations that there is no evidence that would "justify the inclusion of firm-specific investment grants among the instruments of economic policy" (Usher, 1983, p. 378).

Although Tarasofsky's discussion is mainly theoretical, he deals with IRAP in particular at the end. He criticizes other attempts at measuring incrementality for looking at the wrong variables, such as total sales or total employment, when the real issue is the re-allocation of total expenditure from other investments and consumption to the investments that IRAP seeks to promote. For all intents and purposes, his definition of incrementality is the same as Usher's. Whereas Usher argues that it is impossible to apply the incrementality test, Tarasofsky wishes to apply the test rigorously. He criticizes IRAP for not collecting information that would even allow an attempt at applying an appropriate measure of incrementality. He reports that the NRC offers the justification that "much of the R&D activity subsidized by IRAP is of a kind that does not lend itself to the *a priori* application of [Tarasofsky's and Usher's] incrementality criteria" (Tarasofsky, 1984, p. 60). In response, Tarasofsky acknowledges, "There undoubtedly exists a 'level' of research ... that is concerned with problems that are so fundamental and, so to speak, so nebulous that it is difficult to project their fruits into future flows of revenues and costs" (Tarasofsky, 1984, p. 60). He accepts that there is a strong case for the government to support these activities but then goes on to argue that it is not clear that these activities should be undertaken by firms rather than non-profit laboratories. He concludes that proposed projects not meeting the incrementality criterion "on the grounds that they involved research at too basic a level, should not be considered under IRAP" (Tarasofsky, 1984, p. 61).

III. A Structural Assessment

In this section, we check IRAP's performance against each of our design and operation criteria.

A. Uncertainty

1. Large leaps are dangerous. 2. Successful policies often pursue incremental innovation and (where possible) aid in the acquisition of tacit knowledge. Incremental technological innovation and structural change have always been central to IRAP's activities. Furthermore, it does not have the resources to finance large technological leaps even if it were inclined to do so. On the technology side, IRAP emphasizes such incremental efforts as giving technical advice and assistance, assisting in the acquisition of tacit knowledge and of existing best-practice technologies, and focussing attention on niche R&D opportunities. All of this is assisted by the high degree of technical expertise of IRAP's field officers, as detailed in section I.

On the structural side, IRAP has tried to effect facilitating structure changes that may have significant effects on R&D and innovative capacity without entailing major expense. In particular, it has done much to build and strengthen co-operation between industry, government and university R&D infrastructures through such elements as IRAP-P, LabNet and IRAP-R, and through the creation of linkages, partnerships and collaborative efforts. These elements provide the basis for increasing the technological capacity and competence of SMEs.

The Research, Development and Adaptation element (RDA), which was created under the 1991 Strategic Plan, might appear to introduce some changes of emphasis.[56] In particular, the "graduation" of firms that are clients under this element to larger, more technically sophisticated and complex projects is a key objective. It is not clear, however, as some have suggested, that the element marks a significant departure from the past. While more concerned with projects involving significant technical risk, RDA also assists client firms by facilitating the adoption and adaptation of technologies of proven merit, as well as through the provision of technical expertise (NRC, 1994, pp. 6–7).

Whether or not RDA represents a change in focus, the new Technology Enhancement (TE) element maintains IRAP's previous focus on small-scale initiatives and the development of an R&D capability in firms with little or no previous experience in this area. The initiative allows IRAP to address a segment of the economy that otherwise would not participate in R&D.

IRAP's design, administration and delivery are fully consistent with these criteria. No critic has ever suggested a tendency to encourage large leaps or a deviation from the program's mandate of encouraging incremental innovation and the acquisition of tacit knowledge. Thus we rate it as a success in these respects. On criteria 1 and 2, it receives a grade of *S*.

3. Pushing the development of a technology off its established trajectory is dangerous. The focus of IRAP has always been to improve the technological competence of Canadian industry in a manner that is consistent with this criterion. The program initially sought to do this by increasing the number of

[56] Some of these changes may be a response to expected revisions to IRAP's budget. Its funds are being reduced both for contributions to firms as well as for operations. In 1997/98, contributions for operations are being reduced by 15% or $12 million, while contributions to firms will suffer cuts of 5% (NRC, 1995, p. 8). The contributions budget faced the possibility of a further $20-million cut in 1997/98, "when certain supplemental funds IRAP now receives are terminated" (NRC, 1996, p. 269), but this sum was restored in the government's 1997 budget.

research workers employed in Canadian industry, and later by creating technology information networks and diffusion mechanisms (NRC, 1969, p. 8).

IRAP has typically supported existing activities of firms; it has encouraged them to exploit their current technological development trajectories by installing better machines and electronics, and has supported them in their bids to employ and exploit larger research units. Through the Technical Information Service (TIS) and in various other ways, IRAP has sought to support and assist industry's R&D efforts and technological connectedness by increasing access to, and use of, the informational, technical and human resources of the government S&T infrastructure, universities and the private sector. IRAP's encouragement of firms to access the expertise of other researchers has assisted them in exploiting the development trajectories of the technologies created with their own R&D.

New program tools introduced since 1981 suggest that IRAP officials have identified and developed new ways of assisting firms to exploit established technology trajectories. Under IRAP-L, short-term funding is provided for subcontracting by small firms to laboratories, institutes and consulting firms. Two other important functions are clearly in keeping with encouraging and facilitating technological connectedness, technology fusions and the exploitation of established trajectories. First, the TE element enables small enterprises to build up and improve their technical competence and capability. Second, RDA supports the adoption and adaptation of "technologies of proven merit" and the provision of "specific technical expertise or a crucial technology" (NRC, 1993, pp. 3–6).

For exactly the same general reasons noted under criteria 1 and 2, we rate IRAP as a success in this respect. On criterion 3, it receives a grade of *S*.

4. Flexibility is important. Flexibility has two aspects: the need to keep the program flexible as unforeseen experience accumulates about its efficacy, and the need to alter support as experience accumulates about current projects.

Design flexibility **–** According to historical documents, "From its inception [IRAP] has been regarded by the Government and NRC as experimental in nature. Accordingly, the NRC has been flexible within its terms of reference, in adapting policies and procedures to meet industrial requirements as they have been revealed during the development of the program" (NRC, 1969, p. 58). It is also stated, "New initiatives are encouraged and assessed. If successful, they are incorporated into the program, if not, they are phased out" (NRC, 1990, p. 50). IRAP's flexibility can be seen on two levels: its delivery process and its repeated efforts to re-invent itself, both of which are documented in detail in Table 4.1. The latest example of this design flexibility is the Canadian Technology Network (CTN), established in 1994 in response to a perceived need for market research when small firms bring out new products (Tait, 1995, p. 1). Its mission is "to provide integrated, accessible pathways to [domestic and international] information and services relevant to small- and medium-sized enterprises using technology," including benchmarking and business assistance (Tait, 1995, p. 1).

Since IRAP has been highly flexible in its own internal structure and specific instruments, we rate it as an unqualified success in this respect. On the criterion of design flexibility, it receives a grade of *S*.

Delivery flexibility **–** IRAP reserves the right to terminate projects in cases where there is "unsatisfactory performance by company research staff ... inadequate support provided by the company ... [a] declining prospect of worthwhile research and exploitation through development and production ... [and] loss in conformity of the project to the objectives of the IRA Program" (IPO, 1973, pp. 14–15). Further, the terms and conditions of IRAP grants provide administrators with discretionary power to conduct on-site inspections and require reports from client firms (IPO, 1973, p. 17). Simmonds (1983) examined the results of 475 IRAP-P projects. Between 1974 and 1977, some 91 of 475 projects (19%) were terminated.[57] The 91 projects received $13.4 million in funding, representing 12% of the total of $115.9 million contributed to the 475 projects. IRAP also has feedback mechanisms that encourage adaptation to experience with specific projects, although many are so limited in scope that they are best viewed as one-event undertakings.

Several bodies have offered more general evaluations of IRAP's ability to manage its funds and learn from the experience of its projects. At an early stage, the AG had reprimanded IRAP for its lack of funding control as well as for management, monitoring and control deficiencies. In subsequent reports, the AG stated that these criticisms had been largely dealt with. More recently, the AG has stated that IRAP's budgetary controls are adequate, as are its project assessments, approval practices and controls.

The Standing Committee on Industry Science and Technology, Regional and Northern Development (1991) criticized IRAP for poor budget management because it had on occasion run out of allocated funds before the end of the fiscal year. But this may have been evidence of flexibility rather than incompetence. The Liberal members of that Parliamentary committee noted that IRAP chose to allocate funds with timing appropriate to their clients, rather than maintaining a budget over the full fiscal year solely for the sake of adherence to bureaucratic conventions.

In view of its budgetary flexibility, the existence of design characteristics that allow projects to be reviewed and cut off where necessary, and the evidence that these mechanisms are used, we rate IRAP as a success in this respect. On the criterion of delivery flexibility, it receives a grade of *S*.

5. Diversity is one of the best protections against uncertainty. The current program elements are consistent with this criterion. The TE element is "designed to allow for support of a variety of small-scale initiatives enabling [SMEs] to demonstrate and improve their technical competence as a basis for more substantive endeavours" (NRC, 1992, p. 3). "Feasibility studies, technical analysis and problem-solving by consultants" are eligible for funding (NRC, 1994, p. 7). While the RDA element's focus is on more complex projects, it allows for the adaptation of technologies of proven merit (NRC, 1993, p. 3). RDA (like its predecessor IRAP-L) seeks to meet the needs of firms that lack "a specific technical expertise or a crucial technology to undertake a project" (NRC, 1994, p. 7). Thus, IRAP's contributions can allow firms to conduct experiments that they may not have otherwise performed because of a lack of funding or technological capability.

Through its risk- and cost-sharing activities, IRAP attempts to assist SMEs, "for which the failure of even one research project (and it may be their only one) to produce profitable results might have serious financial consequences" (NRC, 1972, p. 34). In doing so, IRAP encourages diverse experiments. Because firms are forced to bear some of the costs of projects, they have incentives to conduct diverse market experiments as well as purely technological ones.

More generally, by not engaging in big technology pushes, IRAP has avoided the temptation to put its limited funds into too few baskets. Since its mandate is to encourage the capacity for, and the undertaking of, many technology experiments and transfers, it would be difficult for IRAP to fail in this regard. We rate it as a clear success. On criterion 5, it receives a grade of *S*.

6. Exposure to uncertainty can be reduced by exploiting the interrelation between users and producers. We deal with three aspects of the information exchange relevant to this criterion: (1) between producers

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[57] We do not know how these termination rates compare with those of other, less successful programs. On the other hand, we are not sure that meaningful comparisons could be constructed — for example, because the failure rate is a function of the efficacy of the selection process.

and users of new technologies, (2) between government and university laboratories as producers and IRAP clients as users, and (3) between IRAP as producer of support and its clients as users.

The CTN deals with all three of these types of information flow through a "network of technology and business advisers" (Tait, 1995, p. 1). CTN complements IRAP's technology orientation by linking firms to related business service fields such as finance, marketing and regulation. The primary contact points for this element include IRAP and Industry Canada, technology centres, regional and economic development agencies, business information centres, and industry or trade associations.

In the case of the first of the three specific information flows, IRAP seeks to foster and facilitate interrelations between users and producers through its funded contributions. The program restructuring that began in 1984/85 resulted in the inclusion of market research studies as eligible costs under IRAP's terms and conditions. This change is intended to assist clients in identifying and fostering interrelations between themselves and actual/potential customers.

IRAP facilitates the second type of information flow by fostering links between industry laboratories, the NRC and other government departments (OGD) that produce information and the firms that use it. The program's relationship-building sub-objectives seek "to foster a mutual understanding and co-operation between industry and government research and to establish close personal co-operation between scientists in both fields" (NRC, 1969, p. 2). The goal has been to "acquaint the Government with the nature of [Canadian industry's] supply and manufacturing problems," and at the same time to provide industry with "the latest scientific and technical information available regarding raw materials, new techniques of manufacture ... methods of overcoming operating difficulties," as well as guidance regarding available R&D facilities and resources (NRC, 1995, p. 2). Over the more than thirty years of the program's operation, these links have been created and strengthened through such elements as LabNet, IRAP-R, IRAP-L and the IRAP technology network.

To foster the third type of information flow, IRAP's delivery mechanisms are based on a process that has become progressively more iterative between administrators and clients. For example, changes to the project selection/approval process after 1972/73 called for a project's scientific merits and feasibility to be assessed before the preparation of a formal proposal rather than after its submission. Further, added as a requirement was an evaluation of a project's business feasibility by a "business adviser or business advisory group" before the submission of a formal proposal. Both the Scientific Liaison Officer and the Project Officer who acted as liaison between IRAP and the applicant-firm were available, at the firm's discretion, to assist in the preparation of a formal proposal. The intention has always been to involve the client to the fullest extent in the selection process (NRC, 1990, p. 10). In these ways IRAP has sought to set up an information feedback mechanism that will assist it in revising its own procedures.

For its structure in encouraging flows of information between users and producers of many types, and because of the evidence that these mechanisms are extensively used, we rate IRAP as a success in this respect. On criterion 6, it receives a grade of *S*.

B. Design Pitfalls

7. Multiple objectives are dangerous. 8. Multiple objectives may be sustainable if there are multiple tools. 9. Multiple objectives may be sustainable if they are clearly prioritized. These criteria are important for all blanket policies since such policies typically incorporate multiple objectives. IRAP has largely avoided the associated dangers.

One reason for IRAP's success in meeting the criteria is that technology enhancement has remained its primary objective, with other objectives definitely subsidiary to it. Even where IRAP has sought to meet additional objectives, such as regional development and international competitiveness, the pitfalls associated with multiple objectives have been avoided because meeting the overriding objective — that of increasing the technical capability of industry — has been seen as the means of meeting any of the subsidiary objectives. In other words, all other objectives have been pursued in a manner consistent with the main objective. For example, IRAP seeks to contribute to regional development by increasing the technical competence and capability of local industry. Beginning in 1984/85 but particularly after 1991, sensitivity to regional needs and a commitment to utilizing a given region's "resources and developing [the local] network expertise so as to be able to better support the wealth creating and growth areas of the local economy" have been central features of IRAP services and their delivery (NRC, 1994, p. 10). Further, the objective of increased international competitiveness is interpreted on the assumption that competition in technological advancement dominates inter-firm competition, which in turn is supported by an increase in the "calibre and scope of industrial research and technological development in Canada" (NRC, 1994, p. 10).

A second reason for IRAP's success in meeting these criteria lies in the way it has handled sub-objectives that might have come into conflict with each other. When these sub-objectives evolved, IRAP's solution was to develop separate policy tools to achieve them. For example, IRAP-M was introduced in response to the perceived need to increase assistance to small- and medium-sized enterprises (SMEs) with little or no in-house R&D capability and experience. Working through methods outlined in section I, the long-term objective was to motivate SMEs to build up their own in-house R&D capabilities.

We rate IRAP as a success in meeting these criteria because it kept to only one main objective; where it had other objectives that could have conflicted with its main objective, these were implicitly prioritized by assuming that technological enhancement was the means to meeting them; and where its sub-objectives might have conflicted with each other, separate tools were developed for each. On criteria 7, 8 and 9, it receives a grade of *S*.

10. National prestige should be an outcome, not an objective. Rather than being administered by a high-level council, IRAP was set up within the NRC in order to benefit from that organization's 45 years of experience with "problems ... associated with making 'grants in aid'" (NRC, 1969, p. 7). For the most part, IRAP has been outside the political limelight. It has never been associated with pronouncements about major economic transformations or "new industrial ages." According to one author, "Such pronouncements and the pressures and the insatiable expectations they generate are poison" (Spence, 1989, p. 29). Thus, despite its repeated celebration by government committees and task forces, IRAP has remained low-key and low-profile. As Spence comments, "It is about as far from the political playground as you can get." He quotes a description of IRAP as "'always administered by a group of technocrats that wasn't interested in publicity, just in getting the job done'" (Spence, 1989, p. 29).

The basic design of IRAP has assisted it in avoiding the political limelight. IRAP provides non-targeted, relatively small-scale contributions in support of incremental changes in technology and structure. In contrast, many S&T programs, such as DIPP, often require project approval by the minister responsible, and they tend "to carry much higher 'political' profiles because of their 'leading-edge' approach, the profiles of the firms and the sectors assisted, and because of the significant and discretionary funding available" (Goss Gilroy, 1993, p. 18). In other words, IRAP simply does not have the scope in terms of money or publicity to become a suitable vehicle for big, prestigious projects.

In this regard, and especially in contrast to some resounding failures in other programs and policies, IRAP rates as a strong success. On criterion 10, it receives a grade of *S*.

11. Policies and programs should avoid capture. IRAP has avoided capture from both its private-sector clients and its funders in the political wing. Among the major reasons why it has been able to do so are the following: it is not a very prestigious program; its funding is relatively modest; it targets unspectacular, incremental advances; its officials are professionals with a scientific background far removed from the political sphere; and it has a focussed objective that has never been politicized.

In terms of prestige, DIPP's relatively high-profile cases contrast with IRAP's. In terms of funding, IRDP's relatively large contributions stand in contrast to IRAP's modest budget. The IRDP, the federal government's principal policy instrument for promoting regional and industrial development, exceeded $1 billion in total contributions (to 3,588 projects) only three years after its inception (DRIE, 1986/87, p. 5). In contrast, the IRAP-P and -M elements together provided funding to 499 projects totalling $19 million in 1986/87. The professionalism of the delivery officers and the focussed objectives sharply contrast with the experience of IRDP.

Among the reasons that IRAP has avoided capture by its clients are its diffuse client groups that spread across several sectors and activities of the economy, the technical expertise of its delivery officers, the overriding objective on which all activities of the program are conditioned, and the absence of mechanisms allowing for local political interference.

Concerning diffusion of clients, "IRAP clients are found among industries that represent many major sectors of the Canadian economy, and industries dependent on strategic technologies such as electronics and software; manufacturing; services; machinery; agriculture/food; construction; and chemicals" (NRC, 1993, p. 5). Given the nature of the collective action problem involved in co-ordinating thousands of firms of varying sizes and with disparate interests located throughout the country, IRAP is not likely to fall victim to concerted blackmail or manipulation by its client firms. In terms of expertise of delivery officers and focussed objectives, IRAP's experience sharply contrasts with that of the IRDP. These design features of IRAP allow it to reduce the possibility of client capture because the officers have independent knowledge of the technology problem, and the objective of technological enhancement must be satisfied in all cases. IRDP also provides a contrast on the issue of localized political interference. It contained explicit mechanisms for informing local MPs of projects that were initiated in their ridings, and thus encouraged interference at the local level. Such mechanisms were not built into IRAP.

IRAP has avoided capture largely because of the clarity and continuity of its objective over the course of its long history, the expertise of its staff, the low-profile incremental activity it seeks to induce, its small budget and its broad client base. It thus clearly rates as a success in this respect. On criterion 11, it receives a grade of *S*.

C. Structural Relationships

12. Attention needs to be paid to the relationship between technology and structure. IRAP's prime objective has always been to effect incremental adjustments in the facilitating structure by helping Canadian firms to increase their technical capacity. Initially, IRAP made grants to larger firms to assist in ongoing technological advances, with the condition that the firms develop their in-house R&D capacity. Since then, IRAP has pursued many structural objectives such as encouraging links between university and firm researchers, and making SMEs more aware of technological possibilities.

IRAP's policies and procedures have also targeted market-level and local economy–level structure. Since the initiation of efforts to decentralize delivery in 1984/85 (and again in 1991), the provision of IRAP services has been focussed on regional needs and a commitment "to utilizing [a given region's] resources and developing [the local] network expertise so as to be able to better support the wealth creating and growth areas of the local economy" (NRC, 1994, p. 10).

IRAP's early activities included promoting an awareness of industrial research needs through personal contact and co-operation between government and scientists, and also making university scientists more aware of available government resources (NRC, 1964, p. 12). By the broadening of eligible costs to include the salary costs of university professors and researchers in 1966, administrators sought an additional means of upgrading the competence of industrial teams. A mutually beneficial exchange was emphasized: industrial projects were to provide university researchers with practical applied science or engineering experience, while university researchers were to offer guidance to young, inexperienced teams and provide their industrial colleagues with the benefit of a broader (basic) research perspective. This activity, which targeted a specific part of the structure in order to induce technology change, has evolved into a technology information network.

The IRAP network further illustrates IRAP's handling of the interrelation between technology and structure. The many actors drawn from various domestic and international technology sectors provide IRAP with multiple perspectives on the facilitating structure, from the level of the firm all the way up to the level of the international economy, and its interrelation with technology. In 1994/95, the network was composed of 255 Industrial Technology Advisers (ITAs) located in 90 communities across Canada, 44 Technology Development Officers in 19 countries, plus a group of 300 Scientific Advisers from OGDs, the NRC, universities and industry. ITAs were located in member-firms of the Association of Consulting Engineers of Canada, in Canadian hospitals and universities, in OGDs, PROs, the Canadian Construction Association, the Royal Architectural Institute of Canada, the Canadian Home Builders Association, and 90 specialized technology centres found in every province and territory (NRC, 1996, p. 4; NRC, 1993, p. 3).

The discussion of this criterion shows why neoclassical criticism of IRAP as lacking adequate incrementality misses a key issue. Having no model of the facilitating structure, neoclassical economists tend to look for incrementality exclusively in changes in investment in technology; they completely ignore IRAP's main focus on structural changes. For recognizing the importance of the facilitating structure, and for seeking to alter it in ways that could produce major responses to relatively small incentives, we judge IRAP to be a major success in this respect. On criterion 12, it receives a grade of *S*.

13. Policies and programs can play a useful role in inducing and co-ordinating pre-commercial R&D efforts. A review of IRAP's history shows that it has focussed on supporting consortiums and collaborative research efforts, thereby bringing together similar research activities and co-ordinating these efforts. Such consortiums provide a means by which private firms can credibly commit to jointly performing pre-commercial research (as has been demonstrated by Japan's MITI). The joint research projects tend to be of most value at early stages of product or process development, although they do not necessarily entail an exclusive focus on pre-commercial R&D. (In any case, joint research may allow for the exploitation of clustering externalities, learning and diffusion.)

IRAP initially permitted a "group of companies, not in a position individually to support a research organization ... to set up a research facility on a continuing basis to operate on behalf of the group" (NRC, 1967*b*, p. 6; NRC, 1969, E-1, 3). The creation of LabNet reflected the NRC's evolving desire to promote technology diffusion, joint projects and multi-sector co-operation. A key objective was to encourage technology transfer from government laboratories to industry, "seeking to draw private sector firms into contact with product opportunities arising in the technological environment of government laboratories" (Tarasofsky, 1984, p. 61). The IRAP-R element, Contributions to Major Projects Involving Technology Transfer, was focussed on "complex, collaborative R&D projects" aimed at identifying and exploiting technology transfer and development opportunities in three broad technology areas: biotechnology, agriculture and food; informatics and electronics; and materials and advanced manufacturing (NRC, 1990*a*, pp. 9, 18). According to the NRC, the program helped by "identifying science and technology of potential use to the firm, [and] by facilitating ... co-operation with ... federal or provincial government laboratories, Canadian universities or with other qualified Canadian or foreign sources of technology" (NRC, 1990, p. A-7). IRAP-R eligibility criteria clearly reflected the importance of diffusion, joint projects and collaboration. To be eligible, projects had to involve some element of collaboration with federal, provincial, university or international partners; there had to be "a group of organizations either informally co-operating or in a formed consortium" (NRC, 1990*a*, p. 9).

Under the 1991 IRAP Strategic Plan, RDA effectively replaced IRAP-R and IRAP-M. The new element does not explicitly encourage or favour the co-ordination of R&D. IRAP appears to have come full-circle with respect to consortiums, limiting its efforts in the area to simply permitting collaborative efforts. This can be interpreted as (1) a change in objective with respect to certain types of collaborative research, (2) an assessment that the objective has been achieved and further active encouragement is unnecessary, or (3) a sign that other programs (such as PRECARN) have taken over the role of active encouragement.

Since we cannot be certain how much of IRAP's collaborative research effort was directed at pre-commercial research, we can only rate it as a qualified success with respect to our explicit criterion (although these collaborative activities will have had payoffs in upstream R&D even if it was not explicitly pre-commercial). Since we do not have enough evidence to determine the reasons why IRAP ceased to actively encourage this activity, we rate it as uncertain in this respect. Taking the lower bound of these evaluations, we give it a formal grade of *U* on criterion 13.

14. Policies and programs should seek to maximize positive spillovers – Most of the output evaluations of IRAP adopt the view that technologies generate spillovers and that a program such as IRAP, which is designed to support innovation and technological competence, will help to generate positive spillovers. Furthermore, there is evidence that IRAP sought to produce and exploit positive spillovers by bringing together research efforts of universities, government and industry, by supporting research personnel within firms, by informing firms of existing best-practice technologies that might be adapted to their uses, by providing some level of funding to R&D projects, and by providing support for seminars and discussion groups in order to facilitate trade in technological knowledge. Motivating these activities is the belief that more R&D and higher levels of technological competence within Canadian industry would generate more positive spillovers.

IRAP has attempted to internalize or exploit the spillover effects introduced by the complementarity between elements of technology (as discussed in Chapter 1). IRAP's information co-ordination /dissemination function is vital in this respect. The expertise and connectedness of ITAs and the program elements they deliver, together with the IRAP support network (which fosters domestic and international linkages), facilitate both technological fusions and collaboration, providing access to "specific technical expertise or a crucial piece of technology" (NRC, 1994, p. 7). IRAP-R, for example, assisted in the "identification [of] science and technology of potential use to the firm, by facilitating ... co-operation with ... federal or provincial government laboratories, Canadian universities or with other qualified Canadian or foreign sources of technology" (NRC, 1990*a*, p. A-7).

Technology policies are largely predicated on the existence of positive spillovers. Accordingly, most of these policies are designed to exploit such spillovers. However, not all policies are effective at inducing and exploiting positive spillovers, because of such problems as design flaws, inappropriate objectives or capture. We have already noted that IRAP has avoided these problems and has focussed on incremental changes in structure intended to induce changes in technology (an activity that implicitly seeks to exploit spillovers). It is, however, difficult to identify the quantity of positive spillovers induced or their value. For operating in such a way as to induce and exploit positive externalities, but producing results that are difficult to assess, we rate IRAP as a qualified success in this respect. On criterion 14, it receives a grade of *QS*.

D. Market Forces and Information

15. Market forces and the market expertise of private-sector agents should be utilized wherever possible. IRAP's objective has been to support rather than supplant the decision-making capacity of innovating firms. IRAP seeks to achieve with its clientele a relationship that is friendly and personal rather than supervisory. Every effort has "been made to keep administrative requirements on the applicant ... and the grantee company to a minimum" (NRC, 1969, p. 12). In its 1969 report the NRC stated, "The average application should not require more than one man week in preparation and discussion. The cost should not exceed the range of \$500–\$1,000" (NRC, 1969, p. 12).[58] Furthermore, IRAP's clients have full control over the definition, design and undertaking of projects, while IRAP mainly performs a support role. To make a "permanent impression ... it is essential that roughly half the cost be borne by industry itself" (NRC, 1969, pp. 6–7).

The attempt to increase technological capacity is predicated on helping firms do something that they want to do, conditional on the technological focus of the program. The support of firms' hiring of research personnel is a good illustration. IRAP provides funding support for firms engaged in R&D so that they can expand the scope or size of research projects, or change agendas to different technological research. The choice of research direction is left in the hands of the firm.

As Lipsey and Carlaw (1996) argue, this is an important criterion since several European projects, including the British nuclear energy program (AGR) and the British-French Concorde, as well as a Canadian project, Key Edit, have failed largely as a result of ignoring it. By making detailed decisions concerning a firm's R&D and innovating behaviour, bureaucrats suspended the influence that market forces would have had on the firm and ignored market signals themselves.

For allowing firms' own market expertise to play the leading role in everything IRAP does, and for avoiding the pitfall of suspending the influence of market signals, IRAP rates as a clear success in this respect. On criterion 15, it receives a grade of *S*.

16. Information co-ordination and dissemination are important. As detailed at length above, IRAP has consistently sought to bring together scientists and researchers from universities, government and industry. The intent has been twofold. First, IRAP has sought to encourage co-operation and collaboration on specific research activities, with the objective of inducing information trading and tacit knowledge transfers between all three groups. The co-operation has taken several forms, including direct project participation, discussion groups and seminars. Second, IRAP has sought to build these linkages into more permanent systems of communication. The end result of this second push is IRAP's technology information network, which has evolved over the life of the program. The desired end result of all the efforts has been that industrial research and development in Canada has increased in scope and calibre.

Because IRAP has successfully created the technology information network and other related information mechanisms, we rate the program as a success in this regard. On criterion 16, it receives a grade of *S*.

17. Commercial viability should be sought. The NRC's *A Practical Perspective* states, "Lack of market research when bringing out new products has been widely identified as a major weakness in the business activities of small Canadian companies" (NRC, 1986, p. 47). The cost of market research studies was

[58] The turnaround time for Technology Enhancement (TE) projects is 10 days. For RDA projects under \$100,000 it is normally 40 days, while for those in excess of \$100,000 it is 90 days (Goss Gilroy, p. 11).

subsequently included among eligible costs under IRAP's terms and conditions. As we have already noted, the latest development in this area is a "network of technology and business advisers" called the Canadian Technology Network (Tait, 1995, p. 1).

High-tech for its own sake has driven a number of often-disastrous policies and programs (discussed by Lipsey and Carlaw, 1996), but IRAP has not been allured by this siren. One main reason is that IRAP is client-driven, with projects proposed, designed and carried out by industry rather than government administrators. As a result, the bulk of IRAP's contributions go to supporting incremental technological advances usually associated with private-sector R&D. IRAP was given a cost- and risk-sharing structure because of the belief that R&D expansion in line with industry's own needs and interests was essential, and that "no permanent impression [would] be made on industry unless it [was] spending its own money on research" (NRC, 1969, p. 6–7).

By focussing on commercial viability and avoiding the technology push that has plagued so many other programs and projects, we rate IRAP as a clear success in this respect. On criterion 17, it receives a grade of *S*.

18. Policies and programs should exploit as much expertise as possible. For the many reasons presented earlier in this document, IRAP has been highly successful in meeting this criterion. Of those reasons, foremost is the technical expertise required of IRAP program delivery officers. From the outset, IRAP has been delivered by S&T and industrial research experts. At present, the Industrial Technology Advisers must, at a minimum, have "an engineering and scientific background with at least 10 years of industrial experience at a middle or senior management level" (Goss Gilroy, 1993, p. 15). IRAP's expert staff provide client firms with technical advice and information, transferring their own tacit knowledge. In addition, they serve as points of access to the federal government's S&T infrastructure and to the manifold technology sectors and actors (both domestic and international) linked by IRAP's ever-expanding technology network.

As noted earlier, IRAP has always sought to co-ordinate the expertise of university, government and industrial researchers and scientists. It has also set up program elements designed to help IRAP and its clients seek out expertise where they lacked it. The recent revisions to IRAP's program design and delivery reflect its use of feedback from experts such as the firms it supports, the members of the technology network, NRC's own internal evaluation and the evaluations of others.

Because of its design and operation, judged both on their own merits and in comparison with other programs that have been much less successful in exploiting expertise, we rate IRAP as a success in this respect. On criterion 18, it receives a grade of *S*.

19. Competition-inducing mechanisms increase the chances of success. In IRAP's goals or practices, there is little or nothing to explicitly encourage competition in technology that enhances technological change and economic growth. However, the program increases firms' levels of technical competence and their ability to create their own R&D results, and also to use the results generated by others. IRAP's activities thus are competition-inducing, even if indirectly. This induced competition shows up in two places. The first is in the domestic market, as firms adapt technologies to their own needs and create competitive commercial applications. The second is in the international market, as more technologically capable Canadian firms compete with foreign firms. Indeed, almost everything IRAP does can be seen as increasing the ability of client firms to compete by innovating in product and process technologies.

Because what it has done is only a by-product, although an almost inevitable one, and because the results are hard to determine, we rate IRAP as a qualified success in this respect. On criterion 19, it receives a grade of *QS*.

IV. Conclusion

A. Our Method Applied

We now apply our method of evaluation to IRAP.

Step 1 – On our criteria, IRAP achieves 16 clear successes, 2 qualified successes and 1 grade of uncertain. Note that there are no clear failures.

Step 2 – We have assessed IRAP on the assumption that its overall objectives are worth pursuing and can be achieved. The facilitating structure changes pursued by IRAP are those that are highlighted by our structuralist theory. It is clear from many studies that information linkages between universities, government research laboratories and private-sector firms vary enormously from country to country. Students of technological change argue that great benefits have been reaped by countries where the linkages were strong and able to evolve as technologies changed. This is particularly true of Germany and the United States (Rosenberg, 1994; Nelson, 1996). IRAP's attempts to strengthen these linkages in Canada, where they have always been weaker than in the United States, must be judged as potentially very valuable. The linkages forged by IRAP and by some highly innovative university administrations (taking their lead from the University of Waterloo in Ontario) show that what IRAP set out to do was entirely feasible.

Other structural changes pursued by IRAP are in line with several of the successful policies noted by Lipsey and Carlaw (1996). The following are the examples they give: In the early 1950s, the U.S. military helped create the software industry in the United States, and helped set its standards. The government of Taiwan created a micro-electronics sector by building up a publicly owned company, for which foreign licences were purchased, and then handing the company off to private industry as production became internationally competitive. Korea's electronics sector was developed in a manner similar to Taiwan's; again the government played an active role, inducing private firms to alter the facilitating structure to accommodate the new industry.

The evidence cited indicates that IRAP was trying to induce important structural changes, and that it has succeeded. Our overall judgment of the program is that IRAP is a strong success in almost all the structural dimensions covered by our criteria:

1. IRAP has consistently maintained its overall objective of increasing technological capacity and developing the tools to realize it as the need for them became apparent.

2. IRAP has been flexible in recognizing its own design shortcomings and evolving solutions.

3. Fundamental to the program's success are IRAP's administrative expertise giving it overall institutional competence, its market focus, its relatively small contributions (often on a shared basis) and its lack of capture.

Step 3 – Other assessors who relied mainly on impressionistic assessments have reached uniformly favourable conclusions that agree with ours. In contrast, those who used neoclassical approaches have been critical. In particular, Usher argues that IRAP money would have been better spent on a framework policy, such as a generalized investment tax credit, and that many of IRAP's projects fail his incrementality test. At the end of a long study of five of what he calls "firm-specific" programs, which include IRAP, he concludes, "The potential benefits of firm-specific investment grants are too speculative and uncertain, and the potential costs are too large, to justify the inclusion of firm-specific investment grants among the instruments of economic policy" (Usher, 1983, p. 378).

Step 4 – We must reconcile the disagreement between the favourable assessment reached by our method and the unfavourable assessments reached by the several observers who have approached their task from a neoclassical perspective. This we do in the next section.

B. Reconciliation

We reconcile our conclusions with those of the critics by noting that their definition of incrementality is narrow or ideal, that they require the test of incrementality to be applied rigorously in all cases of subsidy, that they do not apply a similar test to the framework policies they advocate, and that there is some question as to their calculation of the relevant costs of each type of program. We also note that these assessors do not take account of the types of non-linearities characterizing systems of endogenous technological change, nor do they take account of diffusion.

1. Definition of incrementality

We saw in Chapter 1 that incrementality has two aspects: the policy makers' objectives and the criteria by which an outcome is judged to be socially valuable. On the first, Usher and the other critics take a narrow view: "Specific investment grants are intended to promote projects that would not be profitable otherwise" (Usher, 1983, p. 319). On the second, they apply what we have called the ideal test of incrementality, which amounts to saying that a project is incremental only if the use of the government funds is optimal.

Ignoring major objectives – By assuming that IRAP's objectives are focussed exclusively on specific changes in targeted technologies, the critics miss IRAP's major objectives of incrementally changing the facilitating structure. IRAP has placed particular emphasis on changing research and technological capabilities within firms and on creating new channels for information flows between private industry, university and government researchers.

In phase I of IRAP's development, project approval was based on "government interest ... in establishing competent research teams in industry on a permanent basis"; accordingly, grants were provided "only if new people were hired directly for the project or indirectly for internal staff transferred to the project" (NRC, 1969, p. 2). Until 1972/73, special consideration was given to firms new to research (NRC, 1969, p. 2). The narrow test of incrementality does not take into account these attempts to alter structure. Following is a description of some of the changes:

> Of the 131 companies involved in IRAP [between 1962 and 1967], 68 were new to research. Forty-four of these currently in IRAP have created 300,000 sq. ft. of laboratory space, employ 197 professionals including 52 PhDs and [were] spending a total of $6 million on applied research in 1968/69. The remaining 43 companies had previous research experience. Between January 1962 and December 1967 they expanded laboratory space from 93,000 to 165,000 sq. ft. (77%), increased total capital investment in research buildings and equipment from 31.3 million dollars to 74 million dollars (136%) and increased annual research operation expenditures from a total of 25.3 million to 58.6 million dollars (132%). (NRC, 1969, p. A-1)

For another illustration of how the critics ignore structural aspects, consider this observation of Usher: "Since a substantial proportion of the grants go to big firms with large research labs, it is difficult to claim that the project for which the grant is applied would not be undertaken if the grant were refused" (Usher, 1983, p. 318). This does not seem to us a valid criticism, for two reasons. First, we have already noted that one of IRAP's early objectives was to alter the research capacity of large Canadian firms. IRAP's way to do this was by offering assistance on projects that were in accord with private industry's research agenda, and to add riders concerning the creation of more permanent R&D capacity. Second, IRAP has always supported small enterprises and its support for them has been growing as a proportion of its budget.[59]

To look solely at funded projects (as its critics have done) ignores, for example, the "more than 11,000 significant client interactions" in 1993/94 alone, undertaken by IRAP in providing technical assistance, information and referrals with a view to improving the technical competence of Canadian firms as well as changing their behaviour and attitudes toward technology (NRC, April 1994, pp. 8, 16). In 1991/92, "Up to 67% of all requests within a geographical region [were] for non-funded assistance" (Goss Gilroy, 1993, p. 22). Even where "money is the primary motivating factor in contacting IRAP," technical advice and assistance is provided in the preliminary definition and design stages as well as throughout the project (Siddiqi et al., 1984, p. 10). The narrow test of incrementality is not fully appropriate in relation to these rather general objectives or to the multiple and interdependent instruments used to achieve these ends, many of which resist quantification and aggregation.

Criteria of incrementality – While all the assessors reviewed in section II accept the broad test of incrementality (i.e., that the policy maker's objectives are realized in some way), many apply the ideal test (i.e., that something optimal must happen). Usher's view is that this optimality cannot be demonstrated in the case of IRAP. He goes on to argue that framework policies are less costly than focussed policies and can achieve the same objectives, and that they should therefore be preferred. In contrast, Tarasofsky believes that the ideal incrementality test should be applied rigorously; according to him, programs that cannot meet it should be allowed to proceed only as rare exceptions when strongly favourable evidence has been produced.

It is quite impossible for this ideal test to be met exactly, even operating within the neoclassical framework. In a structuralist view it is an impossible test even in principle, since there is no optimal level of investment in R&D. Accordingly, while we accept the broad incrementality test, we believe that judgments must be made about externalities. The evidence seems strong enough to conclude that it is possible to make such qualitative judgments with sufficient confidence so that reasonable decisions can be made about which lines of R&D to encourage with public policy.

Passing incrementality tests – Most of the critics make the correct observation that focussed and blanket policies will support some firms and technologies that would not pass a narrow incrementality test. However, they seldom ask how many of the initiatives assisted by a framework policy, such as R&D tax subsidies, would also fail a similar test. Because framework policies are generally applicable, they necessarily support everyone. In contrast, it is in the nature of focussed and blanket policies to customize their assistance to the particular structural characteristics of the firm or industry being assisted. For example, focussed and blanket policies can exclude broad classes of firms, industries and types of R&D activities that clearly have few externalities, as well as those that can capture enough of the benefits they create to stimulate adequate R&D. They can then focus coverage on those with large externalities but too little benefit capture to stimulate adequate R&D. It seems more likely to us, therefore, that framework policies would do poorly in any comparison of incrementality with focussed and blanket policies —

[59] Note that in 1969/70 the number of small enterprises assisted by IRAP exceeded the number of large enterprises for the first time, while in 1982/83 the dollar value of total grants to the two categories of firms was, for the first time, equal.

although, of course, all need some suitable ex post incrementality assessment for reliable retrospective evaluations.[60]

2. Outcomes compared

Usher asserts that framework policies can do anything that IRAP can do. From our discussion of IRAP's targeting of several important aspects of the facilitating structure, it should be clear that this is not correct. Nonetheless, it may help to enumerate some of the further reasons why we disagree with Usher.

Non-linearities – Usher argues that the five programs he reviews are the equivalent of a 2.8% investment subsidy. The overall effects of IRAP's contribution would thus be extremely small in a neoclassical model, in which everything is marginally variable.

IRAP's expenditures are indeed small compared with those of other Canadian programs, to say nothing of those in other industrialized countries. But the expenditure level is in keeping with a mandate to alter structure in ways that will encourage invention, innovation and diffusion, rather than to directly alter the overall volume of investment. In theory, in the type of non-linear dynamics characterizing systems with endogenous technological change, there are many situations in which small causes have large effects. Given these non-linearities, IRAP's small, focussed expenditures in several directions can have large impacts.

Specifically, IRAP is trying to alter the structure of R&D capability on the part of firms, to disseminate knowledge and to alter attitudes (something that has no place in neoclassical economics but is advocated by structuralist theories). Simmonds argues that incrementality cannot be fully measured but accepts that IRAP is probably incremental under what we have called the weak test because it alters firms' behaviour and modifies technological trajectories: "The more [IRAP's] expertise modifies the initial project and the course it then takes, the more [incremental it becomes]" (Simmonds, 1983, p. 10). He also notes that the program speeds up innovation: "IRAP funding gives a competent research director the chance to initiate research sooner than would normally have been possible with only company support" (Simmonds, 1984, p. 16).

Diffusion – Particularly in its efforts to assist small firms, IRAP has played a major part in diffusing technologies and adapting them to firm-specific uses. By ignoring the issue of diffusion, many of the critics implicitly accept the usual neoclassical assumption that new knowledge diffuses instantaneously throughout the entire economy. This assumption is manifestly wrong. Studies show that technological knowledge diffuses slowly within one country, to say nothing of internationally. One reason is that the acquisition of knowledge is a fixed and sunk cost at any particular moment but that over time, as more and more knowledge accumulates, these costs are rising. The costs of acquiring existing knowledge are thus an increasing burden on small firms, more and more of which are unable to bear them.

Risk does not equal incrementality – Many critics associate incrementality with high-risk projects, implicitly arguing that risk is the main reason why socially valuable projects will not be undertaken by private firms. For example, Usher reports, "What is claimed is that ... IRAP ... may be incremental in some especially risky projects" (Usher, 1983, p. 318). While we agree that pushing high-risk projects is usually incremental, we note that such projects often fail and we do not believe that this should be IRAP's

[60] In a neoclassical model with continuous, negatively sloped functions relating expected payoffs to R&D in each line of activity, a general R&D subsidy produces *incremental* R&D expenditure in all lines of activity. As pointed out in section IV.C.1 of Chapter 1, this result is an artifact of the models' assumptions. It is not found in structural models nor is it likely to be found in practice.

main justification. Instead, its justification is to be found in the mechanisms it has used to deal with some of the additional reasons that may impede socially desirable technological activities. These reasons include attitudes to R&D; ignorance of existing technologies; high private fixed costs of acquiring such knowledge; R&D network externalities, which increase with the number of researchers and with the ease of communication between them; the structural and technological complementarities causing externalities (reviewed in Chapter 1), and the costly structural adjustments required by changes in technology. All these activities can be incremental in terms of the broad test, while not being high-risk.

3. Costs compared

A common argument of the critics is that the costs of firm-specific and blanket programs are higher than those of framework programs. Let us grant this point for purposes of argument. However, both types of policy support projects that would not pass even the broad incrementality test. The relevant comparison, therefore, is in cost per effective dollar, i.e., per dollar that goes to support incremental activities. We have already noted that framework programs, such as R&D tax credits, distribute many rents to firms without altering their behaviour in any significant way, while focussed and blanket policies can in principle direct their spending to areas where the incremental effect is large. For this reason, the cost per effective dollar may be higher for framework policies than for focussed and blanket policies. We have no way of knowing whether this is true in practice, but the view presented by IRAP's critics — that the costs of focussed and blanket programs must be higher than those of framework policies — is misleading.

4. Uncontrolled gifts

Critics often worry that support not constrained by strict tests of incrementality can easily lead to expenditures that are wasteful, because they either lack effective control or are captured for purposes other than those of the policy. We have no doubt that a propensity to waste money is common to all too many government policies, programs and projects, and we have provided our own list of examples in Lipsey and Carlaw (1996). Nonetheless, it follows from our structuralist analysis in Chapter 1 that there is no strictly scientific method of determining who should receive support. Almost everyone accepts the broad incrementality test, but we argue that the narrow test is too constrained while the ideal test cannot be applied. (Usher says we can never know enough to apply it; we maintain that, because of the absence of a clearly defined optimal allocation of resources, it is indefinable in principle.)

5. Conclusion

In the final analysis, good policy is founded on a judicious mixture of theory, fact and judgment. Our structuralist analysis is designed to increase the chances of success by assisting the judgment required to choose policies and design programs. Structuralist theory helps identify areas where public policy has an opportunity to effect desirable changes; our design and operation criteria identify good design characteristics that are to be encouraged, and bad ones that are to be avoided, at the levels of program structure, administration, staffing and control. The purpose of these criteria is to provide operational tools that help policy makers avoid creating wasteful programs.

Tarasofsky, Usher and the Economic Council of Canada outline some of the key issues concerning incrementality, and suggest that IRAP is largely non-incremental or that its costs probably exceed its benefits; however, they offer no conclusive proof that IRAP is throwing money away. On the basis of the assessments of others, as well as IRAP's own modest expenditures, one might argue that the burden of proof lies with the program's detractors.

We conclude, first, that no strong case for failure has been established by others, and second, that there is a very strong case, on our criteria, for regarding IRAP as a success in terms of the desirability of its objectives and its performance in achieving them (even though there may be room for improvement).

5. AN OUTLINE OF A STRUCTURALIST TECHNOLOGY POLICY PACKAGE

Now that our detailed studies are finished, we can say in general terms a little more about what a structuralist plan for a good technology policy would be.

First, associated with technological change are potentially massive externalities created by technological complementarities. The externalities differ in the amount of social benefit following from the string of technical changes necessitated and/or made possible by the innovation in question; they also differ in the proportion of that benefit capable of being appropriated by the innovator. In addition, markets fail to provide sufficient stimulus for invention and innovation because of fixed costs of creating and acquiring knowledge, and of setting up production facilities for new products and processes (and other similar sources of non-convexities). The extent of these market failures varies with the strength of the forces that cause them. The externalities and other market failures provide a major part of the argument in favour of encouraging more technological change than would result from the unaided market.[61]

Second, because of the uncertainties associated with technological change and the discrete nature of many of the adjustments caused by such change, we cannot expect to find a unique optimum policy for encouraging technological advances, nor can we expect to find a neutral one that increases R&D across the board without distorting incentives from some optimal level. Instead, we search for a broad spectrum of projects that can enhance technological change in ways that informed judgment suggests will be beneficial.

Next, the objective should be clear, unambiguous and single-minded: *to raise the level of technological change*. It should not be muddled by stating expected benefits as policy goals. For example, the makers of a policy may expect it to increase international competitiveness and domestic employment. These and other expected benefits can be touted on the political platform but they should not become part of the policy's objectives. Very often, the benefits expected to flow from a policy are stated as a part of the policy's explicit objectives. Doing so causes nothing but confusion — for example, by leading to divided perceptions of programs and projects that should be supported.

There may be adverse side effects to pursuing the single-minded objective of increasing the rate of technological change. The objective is acceptable, however, if the social benefits, such as increased productivity in the long run, are greater than these costs. Other policies may be designed to address the costs of the technology objective. It is important, however, that the policies be set up separately and administered under separate structures. This approach protects technology policy from being sidetracked by having to temper its thrust with potentially conflicting considerations. For example, retraining and relocation schemes, as well as subsidies to slow the rate of regional adjustment, should (insofar as they are desirable at all) be established as separate policies under separate programs and administrations.

The single objective should be achieved through a combination of framework, blanket and focussed policies. Each directed at a particular means of advancing the overall objective.

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[61] Of course, governments also express other reasons for wanting to encourage technological change, including the wish to protect against loss of competitiveness and to encourage the creation of comparative advantages in industries that sustain high-paying jobs.

At one extreme, framework policies should be used to give broad encouragement to all types of technological advances. R&D tax credits and reasonable protection for intellectual property are both desirable. The level of R&D support is a difficult judgment call. Although worries are currently being expressed (particularly by small firms) about the accessibility of the present Scientific Research and Experimental Development (SR&ED) program, the deficiencies, if true, are remediable faults in a basically sound program that is probably about as generous as it needs to be. There are inevitable conflicts in administering a support policy through the tax department, which has a strong interest in raising revenues. For this reason, institutional competence needs to be watched carefully. Does the revenue department have sufficiently knowledgeable and approachable science auditors to ensure that firms obtain the credits to which they are entitled? A strong case can be made for separating the science audit, which determines whether a firm has done R&D within the meaning of the Act, from the financial audit, which determines whether the money was really spent on the eligible categories. The former could be done by a science-based department and the latter by the revenue department. This suggestion may or may not be administratively feasible; however, it highlights the importance of constantly ensuring that programs that look good on paper are administered by departments with the correct collections of abilities, trainings and mind sets.

Intellectual property protection needs to be set within the limits allowed by international agreements, at levels that provide adequate incentives for technological advances while minimizing payments beyond those levels. The current patent life allowed for pharmaceuticals may be too long by that criterion, as was James Watt's patent on the basic design of a steam-driven piston. Watt's patent inhibited the development of the high-pressure engines needed for steam to spread into the transportation industry, a development that happened only after the patent expired in 1800.

At the other extreme, focussed policies and programs should be used where market failures are large and concentrated in localized situations. Lipsey and Carlaw (1996) analyse a number of interesting cases in which focussed policies seem to have been effective. They also deal with at least an equal number of cases in which such policies failed. Focussed policies may be suitable to help new industries develop technologies that will be beneficial to all but that are either hard to appropriate or where appropriation might slow development. A case in point is the U.S. government's support for research into technologies required by the rapidly developing aerospace industry in the period from 1919 to 1939. Focussed policies may also be called for where new industries need consistent standards imposed on them. The U.S. software industry is largely a creature of U.S. military procurement; during the industry's early stages, the military provided monetary support while imposing uniform standards that greatly benefited the industry long after it had become self-sustaining.

Focussed policies may have a role as well in a situation where a particular breakthrough is being sought by several competing firms. In some cases, non-patentable research cannot be kept secret, and so there is too little incentive to do the research. In other cases, the non-patentable research can be kept secret and the competing firms may duplicate each other's research. (When uncertainty calls for many different experiments, "duplication" is not a problem, but when all firms are pushing for the same fairly well-defined breakthrough, duplication may be largely wasteful and may produce results more slowly than when firms co-operate.) In both sets of cases, a focussed policy can create commitment mechanisms that allow firms to co-operate on pre-commercial research and then to start competitive commercial research all on the same footing. Japan's Ministry of International Trade and Industry has been quite successful in operating such policies in the case of several research projects.

Dealing with these and similar cases requires carefully focussed policies. The dangers involved are many. Capture either by politicians or client firms is much easier than with broader-based policies. Mistakes can be very wasteful, particularly when the policies seek to foster national prestige or other non-commercial goals. The wrong sort of technical administrators can easily become enamoured of technological push, creating technological wonders that have little or no commercial value. To avoid these and many of the other pitfalls identified by Lipsey and Carlaw, the focussed policies need clear goals of advancing commercially viable technologies. They also need to be administered by staff with the appropriate institutional competence.

In between, is there a place for blanket policies? In principle, a blanket policy is required where the market failure being addressed is too localized to be covered by a framework policy and too generalized to be met with a narrowly focussed policy. One example is the dissemination of technological knowledge to small firms, for which the fixed costs of scanning all relevant information are prohibitive. Government bodies such as IRAP can acquire and disseminate such knowledge, spreading the fixed costs over many operations — provided the administrators are well versed in the appropriate technologies as well as in the problems faced by small firms.

The biggest problem with blanket policies is that they typically are not given clear enough guidelines regarding objectives and criteria for acceptance, nor are they given well-trained administrators, particularly when specialized technical knowledge or business experience may be needed. All too easily, the programs then become channels for handing out government largesse without clear purpose or selection criteria. Furthermore, poor policy design is much harder to spot in a broad-based blanket policy than in a policy narrowly focussed on one specific objective. The sequence of programs that culminated in IRDP provides a strong cautionary tale showing the extreme dangers inherent in most blanket policies with broad mandates to encourage a number of related phenomena, such as technological change, economic growth and international competitiveness.

The cases of IRAP and DIPP show that, if properly conceived and executed, blanket policies can work, and work extremely well. But these are more the exceptions than the rule. Failures bestrew the history of blanket policies having technology advancement as one of their goals.

Perhaps the most important difference between the evolution of IRDP and its predecessors, on the one hand, and IRAP and DIPP, on the other, may be seen in the objectives of each program. With PAIT, a focussed objective evolved. The program went through a self-revision process to correct problems it had experienced in meeting the objective of providing more funding to support innovation projects. With the creation of EDP, however, different programs were integrated into one package and this single objective was rolled together with others.

The multiple and vague objectives of EDP and IRDP starkly contrast with the single, clearly articulated objectives of IRAP and DIPP. IRAP has the overriding objective of increasing the technological competence of Canadian firms. All activities and project applications for funding support have to help meet this objective. DIPP had the single overriding objective of improving the technological capacity of defence and defence-related industries. All projects supported under DIPP were measured against this objective.

One lesson is that, if there is no clear and single-minded objective to guide decisions taken in conditions of uncertainty, the exposure to uncertainty is thereby increased. IRAP and DIPP were more successful because each of the two programs was looking for projects that met its objective. IRDP and its predecessors were not successful (with the possible exception of the early programs) because, in addition to all the uncertainties associated with supporting innovation projects, these programs lacked an overriding guideline for determining which projects they should support.

IRAP and DIPP were able to build up an internal structure that focussed each program on a particular set of technological problems. They thus were able to exploit the relationship between facilitating structure and technology. Since EDP and IRDP had no strong focus, their internal structures had no co-ordinated connection with the technological problems they were seeking to address.

Furthermore, the single objectives of IRAP and DIPP enabled these programs to recruit well-trained people of like mind in order to meet their objectives. In the case of IRAP, engineers were hired to assess the technical competence of the companies being supported. DIPP recruited technical experts in defence, and had the support of the Department of National Defence to guide project decisions. Since EDP and IRDP lacked clear guidelines and had multiple objectives, they did not discriminate in terms of the kind of personnel employed; the end result was ambiguity about what was an appropriate project, and why. Thus another reason why programs need an overriding objective is to help generate structural consistency, in terms of both program design and the expertise of the personnel administering it.

In summary, the ideal structuralist technology-enhancing policy set has a single aim but multiple policies and programs to achieve that aim. Framework policies provide the general push. Focussed policies cover particular spots where market failures are large and specific. A few blanket policies are cautiously applied when a relatively broad-based single need can be identified and clearly communicated to the administrators. But before such middle-range policies are used, very careful study is needed — much fuller and more careful study than has typically gone into the design of policies that were often hastily thrown together in response to political or other pressures.

Before millions of dollars are spent on any new blanket policy, a few tens of thousands should be spent on clearly defining its goals, selection criteria and administrative structure. In principle, this advice is easier to follow than the neoclassical advice of searching out the optimum level of R&D and instituting neutral policies to achieve it. It may, however, be no less difficult to follow in practice.

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