FORECASTING THE DEMAND FOR NEW BROADBAND SERVICES

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1. Introduction

This paper puts forward the argument that we cannot solve the problem of forecasting the demand for the new services that IBNs will make possible. It is impossible to predict with any confidence that the demand will be sufficient (however defined) to make such an infrastructure economically viable; it is also impossible to predict that it will not be sufficient. In the final section of the paper, a possible escape from this impasse is suggested.

To place the need for forecasts in context, it should be noted that the traffic carried by IBNs, if and when they are deployed, can be categorized in two dimensions: whether it is generated by today's applications or new ones; whether it could be carried by a less sophisticated infrastructure (in particular, POTS, ISDN, or CATV systems) or would require the additional features of IBNs. The case for IBNs rests on the value of the new applications which could not be supported by less advanced networks, relative to the additional costs of the more advanced networks. This is so whether one is concerned with the profitability of the investment for the private sector or the economic merit from a national perspective, taking externalities into account. It applies despite the fact that the benefits of IBNs may extend beyond new applications: for example, they may bring lower prices to large customers for aggregated narrowband traffic and they may bring increased competition to the cable television market (though whether the latter would be beneficial is subject to some controversy).

Claims have been made from within the telephone industry that, for residential customers, an optical fiber network may soon cost no more than the combination of twisted copper pairs and coaxial cable represented by separate telephone networks and cable television systems.[1] It should be emphasized, however, that these networks would have much less functionality than IBNs: in particular, their broadband capability is unlikely to be two-way and is unlikely to be fully switched. Although this paper is

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directed towards IBNs, it may be noted in passing that there is an interesting demand forecasting problem associated with these lesser fiber optic networks. It concerns the network operator's share of the total market for cable television services when there is competition from a cable television company. Even though it may not be easy to solve this problem with confidence, it is a far easier problem to solve than those considered here; at least we can be certain that there will be demand for cable television services.

Much of the literature on IBNs gives the impression that broadband services are new. While the concept of an IBN may be recent, many of the associated services are not. What are now called video conferencing and business television go back to at least the 1960s. The National Science Foundation supported ambitious field trials of two-way cable television applications in the mid-1970s, a time when there were also many broadband telemedicine trials.[2] In addition, overseas, a variety of ambitious experiments with the *wired city* concept were initiated more than ten years ago.[3]

There are lessons to be learned from the past attempts to test and deploy such services. If we ignore them, we increase the risk of repeating past mistakes. Faster progress is likely if we make new ones.

Notice, too, that we need not restrict ourselves to broadband services when examining the methodological problems of forecasting the demand for new services. Experience in forecasting the demand for narrowband services, such as videotex, is relevant as well.

On the whole, attempts to test and launch new broadband services during the last quarter of a century have not been successful, although corporate video conferencing and business television can at last be regarded as firmly established. It is tempting to dismiss the failures with the observation that the technology was too expensive or too immature. One should, however, ask why, if this was the case, it was not appreciated before large sums were spent. One should also ask why, as Carey notes in this volume, expert forecasters were misled.

More was involved than the cost and maturity of the technology. Of particular importance is the fact that it was much harder than it seemed to identify needs to which users considered it worth applying the new services. We will consider this subject next, before turning to the forecasting problem as such.

To avoid possible confusion, our use of three terms should be explained. By *services*, we mean what users regard as services. Thus, here, video conferencing is a service, although what a telephone company may be selling is transport on a T-1 carrier. There is an important logical distinction between a particular service offered by a particular vendor and the class of such services — for example, the difference between AT&T's Picturephone® service and video telephony in general. The probability that any member of the class will fail is higher than the probability that the class as a whole will fail; for the latter to occur, *every* member of the class must fail. The discussion below will center upon whole classes and it is to these that the term will usually refer. We will, however, sometimes use the same term to refer to a single member of the class. The context will make the meaning clear.

The term *new service* is problematic. It is a matter of judgement when a service is sufficiently different from its predecessors to be described as new. For example, facsimile transmission was demonstrated before the telephone. When it eventually took off, was it a *new* service? Finally, we will sometimes describe applications and services as *successful* or *unsuccessful*, without further explanation of either term. The following analysis is intended to be valid whatever reasonable meanings are ascribed to *successful* and *new*.

2. Users' Needs

Underlying a new service are marketers' ideas of the purposes for which intended users will use it and the significance of these purposes to them. The same ideas are likely to underlie models used by forecasters. If these ideas about users' needs are wrong, the launch of the service is unlikely to be successful and it will be a matter of luck if the forecast turns out to be approximately correct.

A clear lesson of the last quarter of a century is that it is harder than it seems to understand users' needs. A simple, but instructive example is provided by facsimile transmission. In the late 1970s, neither it nor electronic mail yet enjoyed wide acceptance in the business community. Given the rapid upward trend in the number of documents produced as originals in electronic form, it seemed to technical experts that electronic mail would comfortably outperform its rival: for text it was enormously more efficient, requiring many fewer bits per alphanumeric character. But technical efficiency is not the same as users' efficiency. The latter would reflect such factors as complications due to different standards and ease of training operators. Presumably factors such as these help account for why facsimile transmission gained more rapid acceptance and, with it, the greater penetration and the improved price-performance ratio which further increased its superiority.

Video conferencing is particularly rich in important lessons. One of the assumptions underlying the case for it was the assumed need for people to see and be seen in business "meetings".[4] Otherwise, many of the potential "needs" could be met by audio conferencing, supplemented perhaps by narrowband services for exchanging graphics. By the time that laboratory research had shown that the incremental value of the visual channel was much lower than had been assumed, major financial — and presumably, psychological — commitments had been made to video conferencing. It seemed that, for the most part, the results of the laboratory work fell on deaf ears. Nevertheless, audio conferencing has continued to enjoy considerable, if low key, success in the marketplace, where it still generates much more revenue than its grander rival.[5]

Video conferencing was also based on the idea that it would substitute for business travel. This fixed the attention of forecasters, as well as marketers, on the business meetings that happened to take place, especially those that involved longer distance travel. Implicit *ceteris paribus* assumptions were made: the number and mix of

participants, the lead time till the "meeting" took place, the duration of the "meeting", and its agenda were considered to be independent variables in the users' decisions whether or not to use video conferencing. In reality, however, much of the value of video conferencing derives in situations when, in its absence, there would be no in-person meeting, or a delayed one, or one which key people would be unable to attend. In this respect, the original concept for the service was far too limited.

There is some empirical evidence which suggests that the concepts which underlay telemedicine and two-way cable television in the mid-1970s were, if not incorrect, certainly too limited. The usual way in which attempts were made to improve access to health care in remote areas was to provide audio and video links between these communities and distant physicians.[6] None of these broadband applications has survived. The contemporaneous work of Conrath and Dunn, in particular, suggested a different approach. Unassisted, nurse practitioners can deal effectively with more than 90% of the presenting cases in rural communities.[7] Even with a broadband telecommunications link, nurse practitioners would probably be necessary to use their sense of touch for the physicians, since observational study has shown that touch is of considerable importance in diagnosis.[8] In the minority of cases in which nurses may need assistance, a choice has to be made among three decisions: urgent referral of patient to physician, non-urgent referral, and no referral. As Conrath and Dunn, and others, have shown, much can be done with an audio link (supplemented, perhaps, by freeze-frame television) to enable nurses to consult with physicians about such cases, thus reducing errors in making referrals.[9]

The focus of NSF's two-way cable television trials was the delivery of public services — for example, social services for senior citizens. The usual way of thinking about such applications is to focus on using telecommunications to link those who need the services with the appropriate experts or with relevant programming. Arguably the most successful field trial — in that the service has continued long after NSF's grant expired — was that in Reading, Pennsylvania. Its primary approach was to release resources that were latent in the community, with only a secondary emphasis on importing expertise.[10]

That the original concepts underlying new services are often not well matched to users' needs can be illustrated in a different way. The content of electronic communication services is either generated by end users (as was the case with telephony) or produced by third parties for their consumption (as is the case with broadcast television). How good are we at placing the concepts for new services in the correct part of the dichotomy? It may seem quaint that the telephone was originally seen by some as a means for relaying sermons and concerts, and television originally as a person-to-person medium,[11] but we should not feel too superior to earlier generations. Video conferencing was originally conceptualized as a service for content generated by end users, yet it first established itself as a viable service in its point-to-multipoint form (now called business television) — i.e., with content created primarily *for* users rather than *by* them. And videotex was originally seen as a service for delivering produced content, but communication among end users has turned out to be very important.

When disappointing results indicate that the concept underlying a new product or service is flawed, a search process may continue: both the concept and the design of the associated technology may undergo a succession of revisions in an attempt to find a viable market before patience and funds run out. (The value of providing from the start the necessary flexibility and funding for such search processes merits serious consideration.) We should not be optimistic about forecasts made before this process has ended, let alone those made before it has begun.

In sum, difficulties in understanding users' needs in advance of trials make it likely that many proposed concepts for new services will fail, or, more accurately, will take much longer than expected to succeed in some modified form. These same difficulties make for failures in forecasting. Probably understanding will improve in the future, particularly if we learn from past mistakes rather than merely write them off. Nevertheless, we should not expect a major improvement overnight. Without such improvement, we should have little confidence in forecasts.

Moreover we should continue to expect failures. They occur in all markets when new products and services are launched and there is no reason to believe we will understand the needs of users of telecommunications better than those of other types of customers; if anything, the reverse seems more likely. If there were no failures, we could be certain that the industry was far too timid.

3. Methods of Forecasting

In this section we discuss the adequacy of the tools available to those who forecast the demand for new telecommunications products and services. We turn first to analytical methods, by which we mean here simply those methods which do not rely primarily on new data generated by tests or surveys. Next we consider field trials. Following this, brief mention is made of laboratory experiments and surveys. (It should be emphasized that, although we will treat them separately, the foregoing are not necessarily alternatives. They may be complements, as when surveys are used to estimate some of the variables included in mathematical models.) Finally, we draw attention to two complications. One arises because there are likely to be narrowband alternatives to the future use of some broadband services. The other is the likelihood that organizational structures and relationships encourage biassed forecasts.

Analytical Techniques

Hough (1980) provides a review of methods used to estimate the future demand for new telecommunications services.[12] The *historical analogy method* is based on striking regularities in the time series data on the early growth of new products and services in the past. *Income and expenditure analysis* is based on established patterns for household expenditures, combined with forecasts of future household incomes. *Competitive and complementary analysis* examines, somewhat subjectively, how demand for the product or service in question might be affected by demand for other

products or services. He uses the first approach to estimate future demand for Telidon, a Canadian technology for videotex and teletext services, and the others to provide cross-validation of the results. As he makes very clear, however, all that can be obtained from the first two methods is an upper bound for the demand, *since both assume that the product or service will be a success.* The third method can do no more than provide some discipline for subjective assessment of the chance of success.

The key problem in estimating demand for a new product or service is estimating the probability that there will be any significant demand at all within an appropriate period of time. It makes no sense to assume implicitly that this probability is unity.

Two approaches do not make the prior assumption of success. One is the Delphi method. This is a process for developing a consensus among experts who provide their personal, subjective forecasts. Although it brings some discipline to informed guesses, it is not systematic in its use of available data; nor is it clear how to define an expert in such a way that he or she can be expected to perform well in such an exercise. Indeed, those who choose to become expert in a particular field of application may well have a vested interest in the rapid growth of the field.

The other approach involves building some behavioral model of the process by which users can be expected to choose, or not choose, the new option. Unfortunately, however, these models usually cannot be validated in realistic environments before the option becomes available. For reasons that will be discussed below, field trials do not provide easy opportunities for such validation.

Field Trials

Field trials are undertaken for various reasons: to address engineering questions and provide a basis for better projection of costs, to influence the thinking of customers or regulators, to provide experience in working with new partners, and to explore demand. The last of these does not seem to be taken seriously in current or announced trials of residential fiber optic networks. Virtually no customers will receive any more functionality than is provided by today's telephone network together with a cable television system. The only exceptions would appear to be four households in GTE's trial in Cerritos, California.[13] While trials which are under way and soon to be started may resolve important issues, they will tell us little of significance about new applications.

In connection with future use of field trials for this purpose, the following initial requirements must be met.[14] First, the scale of a trial must be sufficient. If interconnection with other users is involved, there must be a sufficient number of possible connections for users to have an incentive to use the service. Second, the duration must be long enough. It takes time for problems which arise in implementation to be overcome; it takes time for learning effects and novelty effects to wear off. Third, there must be sufficient advance preparation. This will involve research in the intended user community to improve site selection and identify obstacles and opportunities which will be important at the implementation stage. If new content must be developed, it makes sense to develop and test it before the field trial. Fourth, it will be necessary

to resist the temptation to spend too high a proportion of the project funds on system development and too low a proportion on implementation of the trial. Often, these four requirements have not been met in the past.

Another mistake to avoid is merely tacking evaluation onto a field trial. If one is interested in exploring new applications, it makes more sense to see field trials as components of a larger process of evaluation. The earlier discussion of concepts underlying new applications suggests that a single field trial is likely to be too short for a process which will probably involve refining or replacing service concepts after they have first been explored, and then trying out the revised concepts. A second important issue in the organization of evaluation arises from the conflict between the objectivity required in evaluation and the commitment necessary to make the trial a success for the users concerned. A strong case can be made for evaluation being separated from implementation of the service and for its reporting at a level of management where it is reasonable to expect some objectivity.

Also worth noting is the tentative nature of a trial. What will become of the system in question when the trial ends? Unless there is some assurance that, if successful, it will continue to be available, will there be sufficient incentive for users to reorganize so as to obtain advantage from it? This question illustrates the fact that even field trials are artificial. Whether and how to price trial services can be a further complication for those who seek to create a realistic environment.

Except in connection with audio conferencing, there have been very few field trials in the last 25 years which have demonstrated significant demand for the service in question. One success was the trial of the picture telephone in the criminal justice system in Phoenix, Arizona which was funded jointly by AT&T and LEAA.[15] Although AT&T discontinued marketing Picturephone® after the trial ended, response to the service was encouraging and was predictive of the niche market for less sophisticated video technologies which has subsequently started to emerge.

A second success was the two-way cable television service for senior citizens in Reading, Pennsylvania. In neither case, however, was the limited success of the field trial followed by subsequent diffusion of the technology in question. This raises the question whether the settings and/or the resources devoted to implementation may have been atypical. It also shows that even success in a field trial does not predict successful diffusion.

Another of the rare examples of success was NASA's point-to-multipoint video conferencing conducted as part of its Communications Technology Satellite program;[16] these projects were forerunners of today's business television services. It may be significant that NASA's trials were components in a research *program*, not isolated research projects.

Isolated, small-scale trials are unlikely to be of value in exploring demand for new IBN services. Large-scale trials, embedded in research programs, may be helpful, but results will not come quickly, nor cheaply. There may be a case for a government initiative to enable more to be learned from field trials of new broadband services.

Other Methods

Two other approaches require brief comment. Sometimes attempts are made to base predictions of demand on surveys of the opinions of potential users. There is no evidence to suggest that these have any value at all.

The remaining approach is the use of laboratory experiments. These cannot be used to provide predictions of demand, but they can be used to explore some of the assumptions underlying a concept for a new service. Thus laboratory experiments have been useful in exploring the effectiveness of audio and video conferencing relative to one another and relative to in-person meetings;[17] and to explore how accurately physicians can diagnose cases in a telemedicine context.[18] They have also been used, in a different vein, to evaluate design principles for new content prior to a teletext trial.[19]

Although it is dangerous to generalize from the artificial setting of a laboratory to the world outside, controlled experiments can be used as an economical means of testing a subset of the assumptions about users which underlie new services. They also build understanding which can be helpful in field trials.

Narrowband Alternatives

IBNs face potential competition in new markets from infrastructural technologies which will almost certainly be deployed before they are. HDTV can be distributed by a cable television system which may not even need upgrading with optical fiber trunks. And direct broadcast satellites may siphon off potential revenues from video-on-demand. Less discussed, but probably at least as important, are narrowband alternatives.

As the term is used in connection with IBNs, video conferencing is now a *narrowband* service, usually requiring a quarter or a half the capacity of a T-1 carrier. But as far as end users are concerned, it provides essentially the same functionality as its broadband ancestors. (The main differences are that networks are larger and multi-site conferencing has become much easier. In both respects, the narrowband service of today is better than the broadband service of the past.) This raises another problem. We may correctly predict that a new application will be a success, but that need not mean it will generate *broadband* demand.

Consider telecommuting. At present it is held back not so much by lack of technology, as by cultural and legal constraints, ranging from certain managers' lack of confidence that they could adequately supervise unseen staff to the way the Internal Revenue Service distinguishes between employees and independent contractors. This makes it even harder to speak with confidence about the value to telecommuting of services beyond those that could be supported by narrowband ISDN.

Bandwidth compression has implications beyond video conferencing. It appears likely that it will be used to fit many more channels of entertainment television onto the transponders of direct broadcast satellites.[20] As a result, DBS operators will be able to offer multiple transmissions of popular movies at conveniently staggered starting times on a pay-per-view basis. Though not the video-on-demand service anticipated for IBNs, it may siphon much of the demand away from the latter.

Narrowband alternatives will be relevant even in the absence of competition from *new* means of transmission. The example of telemedicine noted above shows that, in some cases, service delivery may be redesigned so as to allow narrowband services to meet a demand that initially seemed to require broadband service. In future computer applications, there will be a relationship between the decentralization of computer memory and processing power, on the one hand, and requirements for high-speed transmission on the other hand. Trade-offs between using public broadband networks to access remote supercomputers and very large databases and investing more in purchasing technology and data will not necessarily favor the former.

Institutional Factors

There are built-in sources of bias which cause the probability of failure to be understated when a new product or service is to be launched. Was it likely that, in making the case for funds to develop or to launch it, the manager concerned advised superiors that the chances of success were only, say, 40% but that, relative to the costs, the rewards arising from success would make this a risk worth taking? Do entrepreneurs address venture capitalists in this way? Do corporations thus make their case to regulators, when the latter's approval is necessary? Besides, launching new products and services is very difficult. The person at the helm may be more likely to succeed against the odds, if that person is unrealistically optimistic about the odds in the first place.

There is another reason for this bias. In discussing network externalities, Allen has examined whether it is rational for customers to subscribe to a new network when there are still too few other subscribers to justify the cost.[21] He has concluded that the decisions are rational when these early customers expect a sufficient number of others to follow them. Similarly, there are start-up problems for new services which deliver content, rather than interconnect users. Will third parties invest in developing the necessary content? Or, if the content is already on the market, will vendors risk antagonizing those to whom they sell already and who may be threatened by a new competitor? In either type of situation, in order to be successful, one may need first to create strong expectations of success.

This creates a paradox. For the reasons discussed in connection with users' needs, we should expect the majority of new services to fail initially. Yet, we should expect the majority of forecasts to predict their success.

There are two reasons why this situation is likely to persist. One is that decision-making structures assume — and reinforce the idea — that forecasting demand for new products and services is possible. Business cases require revenue projections. Regulators require demand forecasts in order to approve a new service and its pricing. So internal departments and independent firms specialize in meeting these needs.

The second reason why the situation is likely to persist is that processes of natural selection, which would help weed out weaker methods and weaker practitioners, are limited by the amount of time that must pass before many such forecasts can be evaluated.

4. Conclusions

In forecasting the demand for new services, the possibility of failure must be taken into account. (Failure can be considered to occur if it would be a sensible business decision to withdraw from the market. Of course, the service might be reintroduced again later, perhaps in a modified form.) Estimating the probability of success or failure is less ambitious than forecasting demand, but there is no reason to have confidence in our ability to solve even this problem.

When forecasts are introduced into the public debate, they are presented in a form which suppresses the uncertainty surrounding them. Usually the form is a single curve predicting demand at future points in time; needless to say there are no confidence limits. If the probability of failure is incorporated at all, it enters via an *expected value*. Consider an oversimplified example. A new service has a probability of .5 of succeeding. If it does, the forecast demand at time t is $D(t)$. The expected value approach would provide a forecast of $.5D(t)$, which is inadequate for informed discussion. While corporate planners may use forecasts in a way that is appropriate to their uncertainty, those outside the corporation will be unable to do so unless these forecasts are presented in a more informative manner.

In predictions about new services for IBNs, there is the added complication that we are dealing with a new infrastructure, rather than a particular service. This increases uncertainty. New services will come from users and third-party service providers, not only from IBN operators acting alone. Relative to IBNs, Touchtone® service was a modest upgrade of the public network, but it brought forth a wide variety of successful new services — services which were not discussed at the time of the upgrade. Should we not anticipate similar entrepreneurial creativity, if and when IBNs come into being? And should we not expect corporations such as Citicorp and American Express to apply substantial skill and resources to seeking competitive advantage from developing new services or applications to ride on these networks? Even if we could confidently predict failure for the new services proposed to date, we have no basis for generalizing the prediction to those that are as yet unidentified.

That we are dealing with a new infrastructure also worsens the complication of narrowband alternatives. Suppose that a very attractive new broadband application were identified now and that skill similar to that directed to bandwidth compression for video conferencing was directed to turning it into a narrowband application. It would be rash to predict that, no matter what the service, given some 15 years or so, such a development effort would fail.

We should not, however, read too much into this. Supporters of IBNs could object that, although narrowband competition is possible, the value of IBNs lies, in part, in their making it far easier, quicker, and cheaper to develop new applications; in future, we need not accept such lengthy lead times for new applications. (Shorter lead times would bring disadvantages, as well as advantages. The problem of protecting society from potentially adverse side-effects and second-order effects would probably become more severe.) Although this is entirely reasonable, it is not a counterargument to the

point that narrowband alternatives further complicate the forecasting problem. Indeed, it moves the attempts to forecast demand for particular services away from center stage; which is what should happen to them.

The most important conclusion to be drawn from this discussion is that public policy cannot be based on forecasts about new applications. Rather, it must be based, in part, on our *inability* to forecast them, hence on our inability to draw confident conclusions whether deployment of IBNs would be a sound investment from either a private sector or a societal perspective.

There appear to be two broad choices. One is to acknowledge that we are basing policy on an act of faith. We can *choose* to believe that short to medium term demand will be sufficient to justify the investment, either with or without positive externalities. Alternatively, we can *choose* to believe that the time is not yet ripe or, perhaps, will never be ripe for such an investment. Reconciliation of the conflict between these acts of faith cannot be expected to come from further analysis and research in advance of the technology's deployment. It can come only from experience, our own or, possibly, that of other countries.

To adopt the proposals of those who oppose a public policy which would permit deployment is to deny us that experience (within the U.S.). To adopt the proposals of the technology's champions may be to risk a decidedly negative experience and, perhaps, a societally costly one. Either way there is risk.

A principal objective of public policy should be to minimize these risks. To this end a third choice merits serious consideration. We could adopt an evolutionary approach. This would allow the deployment of the technology on such a scale and at such a rate that we could slow down or pull back if projections on the basis of experience were negative, proceed or speed up if they were positive. This, however, is much more easily said than done. Four broad questions must be considered.

First, who should bear the risks of the investment? Given current national commitment to relying more on the market and less on regulators' decisions to fashion our future communications environment, we should look to private sector champions of the technology. Senior figures in the telecommunications industry claim that the necessary risk capital is available,[22] so we should look first to them.

Two conditions will need to be met for this approach to be acceptable. One is that end users who choose not to adopt new services should not have to contribute towards the costs of upgrading the infrastructure through higher rates (or lower quality) for the services they were using before. This would, for example, rule out passing on higher depreciation charges based on the premise that existing plant was becoming obsolete earlier as a result of optical fiber technology. The other condition is that companies who choose to experiment with IBNs should, if successful, be allowed a return on their investment which is commensurate with their risk. Presumably, this return would result, primarily, from revenues generated by new services.

Additionally, we should recognize that this approach may fail. Either sufficient private capital may not be made available after all, or revenues projected on the basis of future experience may be too low, unless asserted positive externalities are used to

justify subsidies. If we were to reach this point, we would at least be more informed than now in resolving the consequent policy dilemma.

Second, what is the right scale and what is the right rate at which to proceed? With the constraint that neither ratepayers who choose not to avail themselves of the new services, nor taxpayers, should be worse off and with regulators carefully monitoring this constraint, it seems unlikely that the private sector will move too rapidly. It may, however, move too slowly, with years elapsing before networks are deployed which offer more functionality than is available from a combination of ISDN and cable television. In another ten years or so, some may argue that IBNs were not given a sufficient chance to prove themselves. There are also risks that large corporate users will move ahead independently, thus reducing the revenue potential of future public IBNs, and that our trading competitors will preempt us.

Third, how much importance should be placed on movement towards greater competition in the marketplaces for telecommunications and cable television services? Today's legal, regulatory and judicial barriers could be removed in such a way as to promote more competition.[23] For example, telephone companies could be allowed to deliver entertainment television services on a common carrier basis, provided they did not acquire the assets of cable television companies, nor enter into joint ventures with them. Additionally, disincentives to cable television companies entering the markets of telephone companies could be removed. However, it could be argued that, if one wished to foster experimentation with IBNs, it would be better to allow joint ventures between telephone companies and television companies. This would allow them to share their complementary expertise. Also, experimentation might be more adventurous in an environment free from the risks of competition, though the converse could also be supported.

It may be that promotion of competition and moderate experimentation with (or towards) IBNs are conflicting objectives. If this is the case, it is far from clear how to resolve the conflict.

Finally, what adverse consequences in other areas may result from removing barriers to the deployment of IBNs? Changes in law and regulation which would be preconditions for an evolutionary policy for IBNs would encourage or enable other changes as well. Such changes must be evaluated in a variety of contexts, not just in relation to IBNs. For example, some of the anticipatory changes in law discussed by Meyerson in this volume would probably have consequences for suppliers and users of services other than just those provided via IBNs.

In sum, we may be able to escape the impasse which arises from inability to solve the demand forecasting problem if we can provide adequate safeguards for today's users of telecommunications and cable television services. This is a far more fruitful subject for policy debate than argument about how much future demand will be generated by new applications.

Finally, it should be made clear that we are not arguing against ongoing research exploring new applications; far from it. The kinds of research presented in the

preceding chapters by Carey, Curtis and Means, and Saffo and Johansen will be valuable in providing guidance on which new services and applications should have the highest priority for development and trial. Such research may also be helpful in suggesting how to design them. And it may indicate problems that will arise in their implementation, or thereafter. But, in the foreseeable future, it cannot provide an adequate basis for forecasting.

Notes

1. For an extensive analysis of the costs of the different types of optical fiber residential network, see David P. Reed and Marvin A. Sirbu's, "An Engineering Cost and Policy Analysis of Proposed Fiber Optic Telephone Networks in the Residential Subscriber Loop," in this volume.

2. For a review of NSF's two-way cable television experiments, see the Symposium on Experiments in Interactive Cable TV, *Journal of Communication*, Vol. 28, No. 2, Spring 1978. For a review of the telemedicine literature, see David W. Conrath, Earl Dunn and Christopher A. Higgins, *Evaluating Telecommunications Technology in Medicine*, Dedham, Ma.: Artech House, 1983.

3. See William H. Dutton, Jay G. Blumler and Kenneth L. Kraemer, eds., *Wired Cities: Shaping the Future of Communications*, Boston, Ma.: G.K. Hall & Co., 1986.

4. See Martin C.J. Elton, "Visual Communications Systems: Trials and Experiences," *Proceeding of the IEEE*, Vol. 73, No. 4, April 1985. For a survey of the teleconferencing research literature, see R. Johansen, J. Vallee and K. Spangler, *Electronic Meetings*, Reading, Ma.: Addison-Wesley, 1979.

5. According to the International Teleconferencing Association (ITCA), the U.S. equipment, transport and services revenues for 1989 were as follows: audio and enhanced (e.g., freeze-frame television) audio conferencing $380 million; corporate video conferencing (two-way) $203 million; business television (plus ad hoc point-to-multipoint video conferencing) $170 million. The ITCA, in its press release dated June 11, 1990, cited the source of the data as Outlook, a joint research project of the Institute for the Future of Menlo Park, Ca. and TeleSpan of Altadena, Ca.

6. See Conrath et al. op.cit.

7. See, for example, D.R. Foote, E.B. Parker and H.E. Hudson, *Telemedicine in Alaska: The ATS-6 Satellite Biomedical Demonstration*, Department of Communication Research, Stanford University, 1976.

8. See Chapter 3 of Conrath et al., op. cit.

9. See also, R.G. Mark, T.R. Willemain, T. Malcolm, J. Master and T. Clarkson, *Nursing Home Telemedicine Project: Final Report*, Boston City Hospital, 1976.

10. See M.L. Moss, *Two-Way Cable Television: An Evaluation of Community Uses in Reading, Pa.*, Alternate Media Center, New York University, 1978.

11. See I. de Sola Pool, *Forecasting the Telephone: A Retrospective Technology Assessment of the Telephone*, Norwood, N.J.: Ablex, 1983.

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13. See Kevin McGilly, M. Kawahata and William Dutton, "Lessons from the Fiber-to-Home Trials," The Annenberg School for Communication, University of Southern California, 1990.

14. For a survey of problems arising in field trials, see Martin C. J. Elton and John Carey, "Implementing Interactive Telecommunications Services," The Alternate Media Center, New York University, 1979.

15. See C.D. Stockbridge, "Planning Exploratory Trials of New Interpersonal Communications," in Martin C. J. Elton, ed., *Evaluating New Telecommunications Services*, N.Y.: Plenum, 1978.

16. See, for example, Heather E. Hudson, *Communication Satellites: Their Development and Impact*, New York: The Free Press, 1990.

17. See J. Short, E. Williams and B. Christie, *The Social Psychology of Telecommunications*, London, England: Wiley, 1976. Also Johansen et al., op. cit.

18. See Conrath et. al., op. cit.

19. See John Carey and Martin C.J. Elton, "Teletext for Public Information: Research Studies in the Field and in the Laboratory" in Jerome Johnston, ed., *Evaluating the New Information Technologies*, London: Jossey-Bass, 1984.

20. See "Moving up the Timetable on DBS," *Broadcasting*, August 20, 1990.

21. See D. Allen, "New Telecommunications Services: Network Externalities and Critical Mass," *Telecommunications Policy*, Vol. 12, No. 3, September 1988.

22. For example, in a presentation by Raymond W. Smith, Chairman and CEO of Bell Atlantic at the Temple University Conference "Telecommunications Policy Issues for the 1990s," Philadelphia, Pa., December 1989.

23. For a detailed treatment of the possible relationships between telephone companies and cable television companies, see R. Pepper's "Players and Stakes," in this volume.