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TELECOM AND WIRELESS VENTURE CAPITAL

Leading VCs on Identifying Opportunities, Managing Teams, Financing, and Exit Strategies

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Risk-Adjusted Returns in Telecoms: People, Product, Markets

Bart Stuck & Michael Weingarten
Managing Directors
Signal Lake
http://signallake.com/

Telecom Investing: An Overview

Those investing in telecommunications are investing in innovation, and from there must work on marketing that innovation in order to put in place everything necessary for the large markets. The focus is on products that solve real problems—products that are typically high-margin and therefore potentially highly profitable. To make that happen, once the product has been identified, it is necessary to attract people who are strong in marketing, sales, and customer support. In the mid-1990s, the focus was on network infrastructure: long distance and metropolitan networking using optical components and optical network equipment, wireless cellular networking using microwave components and microwave network equipment. In the mid-2000s going forward, the thrust in telecoms investment will be moving up from raw transport and switching to higher-level, software-based services (voice, data, and video) all built on Internet infrastructure, and toward the edge of the network (broadband access, a combination of wireless, coaxial cable, copper wire pairs, powerline, and freespace optical). In all likelihood, wireless will win as the lowest-cost (capital and operational) infrastructure.

Finding the Right Product

Businesses exist to serve the needs of their customers: the customers have the money, the customers generate demand for products and services, and all the focus in finding products should be customer centric. The customer pull is met by a product push, because with the technologies of today, things can be built that customers could not really articulate, but in fact, once they see it, they want it! This business development dynamic generates the early stage gyrations all too common in all startups, even the most successful ones. It goes beyond listening to customers, because it requires being proactive in suggesting capabilities that customers might not suggest but in fact really want, and want to pay real money immediately to solve their problem.

In looking for technologies that may potentially evolve into a successful product, many think it is important to look for significant intellectual property, strong technology and trade secrets, and a strong technical team that can work together synergistically. Beyond this, however, it is important to be able to package the technology into different types of products, often in modules that can be rapidly reassembled to achieve different features at different prices in response to customer needs. From there, it should be determined what the process will be to create the products, which will be created first, second, and so on, and what sort of sales capability will be necessary as a result. These determinations dictate what sorts of people need to be involved. For example, when looking at manufacturing-related products, a company would need to look at how to scale and grow large numbers if the product becomes successful. One way to do this is to look for a president that will run the business, and that person will recruit the other people to assist him or her.

Qualities to Look for in a President

The right president to head a telecommunications company should be cognizant of the engineering of product development activities, sales, marketing, customer support, the internal hiring, human resource issues, and the finance aspects of running the business and fundraising with investors. He or she should be willing to spend time with customers, to close on deals, and to recruit other people. Peter Drucker said there is innovation and then there is marketing and that's it; it's very rare that you get both, so often it is necessary to go after the innovation piece and simply try to fix the marketing end of things when investing in a company. Others do it the other way around. Both ways can work; there is no single way to commercial success.

Sending a Clear Message

Ultimately, successful marketing comes down to having a very clear, simple message that can be explained in ten words or less and that will stay consistent over a period of years as the product line grows and features multiply. In order to attain that, it takes a great deal of brainstorming: What is the message and how will it differentiate this company from competitors or other ways to solve a problem? The existing solution may be very expensive and time-consuming, but if the customer is doing it already, why should they change? It must be a huge benefit; the actual price and everything that goes with it has to be a great improvement. All of these things go into crafting a position in the company that is unique and will create a real market to sell products or services that do real things.

The Face of Telecommunications Today

Optical Fiber Transmission versus Wireless

In telecommunications, there are certain things that occur in waves. Historically, in the 1990s, the wave was building new carriers that started doing long-distance communication inside and outside the United States using optical transmission over glass wires or optical fibers, which had far greater capacity than any other known transmission technology for comparable cost. Many of the existing local carriers decided to make optical communications their backbone network, so from that point of view there was a huge upswing in optical communications at that time. This was in part due to the reliability of the components and in part because there was a new generation of equipment put together by venture-funded startups that actually commercialized a lot of the technology. As a result, a huge amount of money went into optical communications at that time.

At the end of the 1990s, it became apparent that wireless was actually a compelling way to provide network access. There were enough glass wires in the ground that it was possible to get close to many locations via optical fiber, but to run optical fiber the rest of the way is very expensive because there's a great deal of digging, people must be paid to put the conduit in, and so on. Consequently, a large amount of money gets tied up in the hope that whoever is having the optical fiber run to their location is going to be wiling to pay enough to recoup the investment.

Wireless is far more compelling, because all that's required are that some towers are built; the towers can permit radio coverage over a large area (e.g., at least 1 mbps symmetrical transmission up to ten miles away), there's far less need for digging the ground up to put in conduit and pull cable, and when it's time to start, all that is necessary is some wireless gear or a high-speed modem of some sort. From there, as customer revenues start coming in, costs go up (with each customer there is a transceiver, phone, Blackberry, etc.) that has a capital cost and the base station transmitter needs additional power to reach these subscribers, but the cost can be scaled with the subscriber far better than the much more capital-intensive approach of putting glass or copper wires in the ground.

While wireless has primarily consisted of mobile phones thus far, going forward that same approach will lead to enhanced technologies where wireless transport speeds will run at tens of megabits per second over significant distances, ten to fifteen miles using cellular telephony technology, and tens of gigabits over short distances of hundreds of meters. While there is a huge opportunity for equipment, which also leads to new services that handle voice, data, and increasingly video, there are also interesting software or service opportunities.

Ease of Use

Software controls the telecom services that are used day in and day out. With broadband networking offering the capability to handle voice, data, and video, both switched immediately and store and forward, there will be a huge upsurge in software that will integrate these services. Examples range from ringtones for customizing your phone, to customizing the ringtones when a particular person calls, to entering text via voice into a form (e.g., "Find me the arrival time of the flight from Seattle to San Jose on a given airline"), to sharing audio (podcasting), to sharing video (videocasting), all intended to permit the end user to have access to information in the most appropriate, easiest-to-use format when the end user wants to do this. Semantic processing on the Web will lead to even more interesting applications, with a huge impact on entertainment, advertising, and news media.

Changing with the Times

People seem to point to the United States companies—Cingular, T-Mobile, and Sprint—as large cellular carriers. But in Asia, which actually has more cellular phones than the United States (in fact, China has twice as many cellular phones as the entire United States, and the gap is widening, but Asia is underserved), there are literally hundreds of carriers that have spectrum but don't have any equipment and are looking for options. In addition, many other countries that are large in scale will be looking for the latest technology, so if there is a condition where there are four to five carriers in each of these places who need to know what to do, that is very compelling for venture-funded companies. These companies are looking to get into big markets, but it is better to search internationally than to count on the Verizons of the world to do something they won't. There are huge barriers to cracking a big-market United States company like Verizon or Cingular; these companies might do some trial research, but they've done very little innovation in the last few years.

Often, it is best to not focus on the push side of things and instead to look at what customers truly want: What is the demand? Conceptually, the communications market is broken up like a pyramid. At the top are a small number of customers like Exxon, GM, IBM, and GE, and a tenth of a percent of those customers typically generate 20 to 25 percent of all the revenues. Then, the next 1 percent of the customers—the large, regional businesses, or the second tier below the Fortune 200—typically generate 35 to 40 percent of the revenue. At the bottom of the pyramid, 99 percent of the customers generate 60 to 65 percent of the revenue, and those revenues are coming from residential customers or small businesses.

Historically, new entrants in communications went after the top of the pyramid, which meant the residential and small business markets were neglected: MCI was set up to offer point-to-point microwave transmission to businesses between St. Louis, Missouri and Chicago, Illinois; Teleport Communications Group put optical fiber rings into large United States cities to connect businesses directly to multiple long distance carriers as well as other local carriers; McCaw Communications went after cellular telephony for business customers first, and only much later residential customers. Those markets make up the bulk of the money, but what is required is a way to reach large numbers of potential customers. Wireless is a great way to do this in a number of different areas of technology. That means more services like TiVo and a huge amount of customized content. Whoever can get there first with the right software, price points, and billing abilities will make a significant amount of money.

The Future of Telecom Investing

New Technologies

In an effort to allow for greater capabilities in wireless technology, there is a great deal of activity in materials. Right now, people spend $3,000 for liquid crystal displays; in the future, there may be new material that makes it possible to create $300 displays with the same or better resolution. At this point, however, it's a materials issue. That kind of technology would mean a different dimension of storage, since storage will continue to grow as the demands for it increase. The investment themes over the past ten years looked at voice, data, and video; over the next ten years, increasingly the focus will be on processing (grid and Web services), on storage (magnetic, optical, and hybrids, and how to virtualize this storage), and on communications (how to interconnect LANs, SANs, and WANs in a seamless manner). The lingua franca will be Extemsible Markup Language (XML), which provides the rich content control to manage these services, but none of the currently available network transport and switching can effectively handle XML, leading to yet more interesting opportunities. With all of this new content, this will invariably lead to some interesting new search technologies or capabilities and a great deal of voice, data, video, graphics, and not just in two-dimensional displays, but increasingly with three-dimensional displays.

Changing Telecom Equipment Business Models

The computer industry underwent two sea changes in business models in the 1980s. One was due to the IBM PC, which was based on open standards and over time led to a very different horizontal business model rather than a vertical business model. The second was high-speed local area networking, which permitted computers to function as if they were tightly coupled shared memory systems when in fact they were loosely coupled processors with a high-speed interconnect. Twenty-five years later, the impact of those trends is still roiling the computer industry: Dell is now the largest single-computer vendor, albeit with profit margins below 30 percent, while the vendors of critical components, Intel with processors, and Microsoft with software, enjoy 70 percent gross margins. The evolution of the data center has been away from mainframes and toward servers or blades, interconnected via high-speed interconnect at a 100 times greater rate than twenty-five years ago, and still climbing.

These trends are starting to spill over into the network equipment business. The Personal Industry Computer Manufacturer Group, which sets industry standards through its more than 500 corporate members (all in personal computers), in 2001 chartered a subcommittee, Advanced Telecommunications Computer Architecture (ATCA), to develop a set of industry standards for network equipment in telco end offices or switching centers. This resulted in two standards, a carrier card that would be eleven inches by fourteen inches, the size of a blade in a data center, and an advanced mezzanine card (AMC), which would be 2.75 inches by seven inches with up to four AMCs on a carrier card. In 2004, a new group, microTCA, was charted to develop outside plant (Digital Subscriber Loop Access Multiplexer, Remote Terminal Concentrator, Wireless Base Station, Cable Modem Mux, etc.), which is still in flux, but more than likely will consist of a shelf of a dozen AMCs with two or more control and switch fabric cards in the shelf. The impact of this could be significant for network equipment business: IBM enjoyed more than 70 percent profit margins and mainframes, and was unable to compete in personal computers with 30 percent profit margins; Cisco today enjoys 70 percent profit margins, but will the Cisco of today be able to compete in five to ten years with profit margins dropping to 30 percent? What about Lucent, NorTel, Alcatel, Siemens, Ericsson, Motorola, NEC, Fujitsu, Juniper, Marconi, and others, who are now faced with competition from a new ATCA-compliant vendor, Huawei, out of China? The high margin opportunities will migrate to semiconductors and to software for network equipment over the next five to ten years, while the high volume will be enjoyed by the low-cost, most agile, standards-compliant manufacturer, one whose strengths are more in packaging and in physical design than in basic software or semiconductor intellectual property innovation. In fact, the semiconductor business could see an evolution to far greater programmability, aka Field Programmable Gate Arrays, and to functional cores to handle different portions of communications, from SerDes to base band signal processing to packet processing (parsing, classification, switching, and traffic management).

Everything Old is New Again: The Changing Face of Investors

The skills required for investors or management teams, given changes in technology on the horizon, are more a matter of returning to what used to work. A company or small group of people is put together, and the investor or management team dribbles the money in and watches how things progress. Over-funding isn't beneficial: Often, the company staffs up too quickly without really understanding what customers' needs are for products, what engineering talents are needed, and how to best serve those needs. As an alternative, starving a company for funds often helps focus the attention of its employees on success and efficiency, and on how to pull together as a team. This also means that successful companies typically end up changing their direction in the first year from that of the initial business plan. That kind of evolution is just the nature of business; there may not have been enough primary market research, or there may have been a lack of understanding when it came to the challenges the company would be facing, and the company changes direction accordingly.

Just as used to be the case, success going forward will not be about relying on checklists like managing teams, working prototype, or multiple first-year customers. Rather, it will be a matter of having the right people and products, and searching markets for products that will solve real problems for the consumer. These are potentially big markets, but typically these markets are just emerging; the operating plan that comes out of the process is going to change, and the investor must be willing to fund it accordingly.

Typical Problems

A typical mistake made by investors or management teams is over-funding the company. A number of funds have hundreds of millions under management, and every two or three years they raise another $200 to $300 million fund. After a ten-year period, there is well over $1 billion under management. With funds of this size, it then becomes necessary to look for investments where large amounts of money can be parked for a period of time. Take, for example, one well-known first-tier fund that invested $25 million into Google out of a single $2 billion fund. On that one deal, there was a $2 billion return to investors, but this raises the question of where the other $1.75 billion went.

Numerous companies that raised $60 to $100 million in aggregate capital didn't necessarily need all that money as soon as it was raised. They should have been scaled back earlier in order to regroup and try to figure out what they could do that would make sense, rather than just spending large amounts of money with no restrictions and no definitive plan. In fact, having a lot of money can lead to spending that money rapidly, and building up a team before it is clear what the true business model is and where the profits really are, so the company runs out of money before it has addressed these issues and is forced to raise more money on very onerous terms for earlier investors.

Another problem can occur when people rely solely on checklists. A company may already be successful based on stock prices, but a preconceived checklist of what should be done will only get in the way. Consider it this way: At 9:00 a.m. Monday morning, an executive thinks of five things to work on that week. He or she might realize that none of those five ideas are good ideas, and so at that point it's time to come up with five more ideas by lunch. There has to be a willingness to be flexible, a desire to change that checklist as necessary in order to take into account the highs and lows of a business. If this does not suit your personality or temper, you should not do this: It is the nature of the beast, intrinsic to making significant decisions with limited time and information and resources to analyze consequences.

Past Problems in the Telecom Industry

One of the problems in telecommunications came with the telecom boom, which started with the 1996 Telecom Act in the United States. This law was a compromise: Local telephone companies were barred from long distance, while long distance carriers needed local access that was controlled by the local telephone companies. The Gordian knot was cut as follows: In return for allowing local telephone companies to offer long distance telecom services to their customers, the local telephone companies would permit others to rent pieces of their network (so-called Unbundled Network Elements, or UNEs) such as the copper wires connecting a home to a telephone company end office, or a portion of the voice switching system in the end office, or operator services for a customer, over a dozen in total. With the dotcom investment wave, there was an initial investment of $10 to $20 billion, but when people founded the so-called competitive local exchange carriers investing in long-distance carriers through optical fiber transmission, it meant at least $500 billion in both equity and debt, or an order of magnitude more money than the dotcom investments.

At that point in time, from 1996 to 2000, multiple private and public placement memoranda were circulating every week asking for more than $100 million to fund a service provider to begin offering residential phone service. Didn't the local phone company already do that perfectly well? The new competing companies' answer was, "Yes, but now that would happen much faster." And who would lead the charge? Great teams of people from the phone companies, who would buy their equipment from the same manufacturers the phone companies had been using all along, so the startups had no competitive edge in feature or in price or cost, because the startups wanted to show they had the same quality equipment as the more established incumbents. And to compound this, equipment vendors such as Lucent and NorTel were willing to finance this equipment purchase. It was essentially circular thinking leading to a loss of hundreds of billions of dollars.

In another instance, large numbers of both optical component and optical network equipment companies were funded. People didn't realize that the reason no one had ever funded this type of project before was due to the huge technical risks in building the components. Invariably, projects would fail because it was too difficult to build the component to begin with or impossible to manufacture in quantity. Everyone got caught up in looking at the carrier markets and the new technology, but it is always important to recognize the risk in those kinds of ventures.

Structural Problems with Private Equity Funding of Innovation

Private equity is by its very nature illiquid: A limited number of entities can buy and/or sell private equity. This leads to a variety of issues that make private equity very different from public equity, or from debt. There is no practical way today to hedge in a private equity investment, yet this long-short mechanism is the single most widely used tactic for public equity hedge funds today around the globe. This means private equity needs to attract investors that can take a long-term view of waiting for the illiquid investment to become liquid, and that invest knowing upfront that they can lose all the investment.

From 1983 to 2003, early funds enjoyed a 60 percent internal rate of return (IRR); later-stage funds enjoyed 30 to 40 percent IRR, in return for a far smaller amount of risk, and later-stage mezzanine funds enjoyed under 10 percent, resulting in a blended average of 16.1 percent IRR for all private equity funds from 1983 to 2003. The early-stage funds that are cited here have a survivor bias: If the fund failed, it does not make it into these statistics. Furthermore, to achieve a 60 percent or greater IRR, consider investing $1 million in each of ten companies, and then at the five-year mark, finding that two failed completely, three recouped the original investment, four recouped two times the investment, and one ten times the investment. This is, in fact, a 16 percent IRR, but the last investment enjoyed a 58 percent IRR. However, think from the vantage of management in these companies: Nine of the ten companies are not mega-successful and will be bought at terms far below what the original investors and management team had in mind (except the management team can only bet on one company, while the investors can diversify across many funds and many companies).

There are geographical issues that arise with funding innovation using private equity. For example, a great deal of telecom knowledge was developed at Bell Labs, and yet there are relatively few funds in the New York City area that are willing to fund businesses staffed by those formerly at Bell Labs On the other hand, there is significant money in the San Francisco Bay area that is risked in semiconductor and in computer and software investments, but relatively speaking, funding far fewer networking-based businesses. With the advent of the Internet and reliable global telecoms, one can expect businesses to have multiple locations (United States East Coast, United States Midwest, United States West Coast, Taiwan, Bangalore, Israel, Europe), because it is now cost-effective to manage these companies with multiple locations and multiple groups, with each geographic area having a certain skill set (e.g., United States East Coast, telecoms; United States Midwest, boards; United States West Coast, software and semiconductors, etc.). Over the next ten years, there will be an explosion of funds outside of the San Francisco Bay area to fund innovation in new geographic regions.

Death of Innovation?

In April of 1997, we wrote an article called "The Death of Innovation?" (downloadable at www.signallake.com/publications) questioning the continuing ability of the United States electronics high-tech industry to innovate.

The article was prompted by the October of 1996 spin-off of Lucent from AT&T, and with it, Bell Labs—an organization that was funded by cash flow from the telephone monopoly. Bell Labs used that money to invent the traveling wave tube, the transistor, lasers, UNIX, as well as 800/700/900 services. It also was a major innovator in loading coils, the coaxial cable, millimeter waveguide, fiber optics, and cellular telephony. In the absence of monopolist cash flow, we wondered where new innovation was going to come from, particularly since our review of the top twenty telecom innovations of the previous twenty-five years suggested that many of them originated from these labs (Table 1).

On the other hand, we recognized that the emergence of the venture capital model potentially could be a replacement (at least in part) for the monopolist corporate lab. Clay Christensen's book *The Innovator's Dilemma* (also published in 1997) points out that corporations over-emphasize line extensions and synergy opportunities, and avoid disruptive technologies. In theory, venture-capital-backed startups, with nothing to lose and everything to gain, might be the ones leading the charge with brilliant innovations.

It's now 2005. What's happened since then? Has the growth of venture capital funding taken care of things, or Houston, do we have a problem?

Post-1996: Lots of VC Funding…

On the positive side, we have seen a major surge in venture funding. Average annual venture funding went from $5.0 billion in 1992 to 1996, to $52.8 billion in 1997 to 2002 (Table 2). That's more than enough to offset a funding drop at Bell Labs.

TABLE 1 Fundamental Telecommunication Advances, 1960–Now

Advances	Conceptual Originator	Leading-Edge Commercialization
Transmission—Wireline:		
1. Laser	Bell Labs	SDL, Lasertron
2. Optical Fiber	Corning	Corning, AT&T
3. Optical Amplifier	Polaroid, Univ Southampton (UK)	Pirelli, Corning
4. Optical Amplifier	Bellcore, Bell Labs	Ciena, Pirelli, Lucent, Nortel
Transmission—Wireless:		
5. Cellular	Bell Labs	Ericsson, Motorola
6. Spread Spectrum	UK Military, MIT Lincoln Labs	Qualcomm
7. Satellite Transmission	Bell Labs	Hughes
Transmission—Digital Signal Processing:		
8. xDSL	Bellcore	PairGain
9. Video Compression	Bell Labs	PictureTel
Switching—Circuit:		
10. Time Slot Interchanger	Collins, NTT	Rolm, Mitel, Intecom, Nortel
Switching—Packet:		
11. SNA	IBM	IBM
12. TCP/IP	DARPA	Sun, Cisco Systems
13. Ethernet	Xerox	3Com, Bay Networks, Cabletron, Cisco
14. Frame Relay	Bell Labs	Cascade
15. ATM	Carnegie Mellon	Fore Systems
16. Multiprotocol Router	Stanford Univ	Cisco Systems
17. World Wide Web	CERN, Univ of Illinois	Netscape, Sun
Control:		
18. Operating System: Unix	Bell Labs	Sun, Hewlett-Packard
19. Language: C, C++	Bell Labs	Borland, Sun, Microsoft
20. Language: Java	Sun	Sun

Legend:
- Large Integrated Equipment/Network Provider
- University
- Government
- Large Equipment/Software Company
- Network Provider
- Small Entrepreneurial Company

Table 2: Annual United States venture capital funding

Year	Number of Companies	Venture Financing
2002	3,134	$30,438
2001	5,267	52,212
2000	8,859	131,984
1999	4,890	63,990
1998	2,860	24,822
1997	2,122	13,194
1996	1,797	10,457
1995	1,133	6,417
1994	746	2,990
1993	671	2,469
1992	751	2,846
	Per Year	**Per Year**
97-02	**4,522**	**52,773**
92-96	**1,020**	**5,036**

Source: Venture Economics quoted in Morgan Stanley *Technology IPO Yearbook*; Signal Lake Analysis

… But Not Much Innovation to Show For it

Despite all that spending, the record on innovation from venture-capital-backed startups is underwhelming. Morgan Stanley's *Technology IPO Yearbook* lists 778 high-tech initial public offerings (IPOs) for 1997 to 2002 (six years). After eliminating IPOs that were spin-offs and/or well-established businesses rather than true startups (i.e., Accenture, Agilent, Agere), we ended up with 763 high-tech companies we examined in greater detail, ranking them on a scale of 1 to 5 (with 1 being high and 5 being low):

Our criteria for ranking degree of innovation was as follows:

- We reserved our highest rank (T1) for new technologies that represented a fundamental departure from anything previously existing, and whose commercialization made possible an entirely new (and important) business market. A good example is the invention of xerography.

- Moving down one notch, we ranked a company as T2 if it was able to demonstrate a fundamental technology improvement in an existing product category. These would include Clay Christensen's "disruptive technologies" (i.e., new technologies that supplanted old technologies in already-established markets, rather than creating new markets).

- Our T3 designation was reserved for companies able to demonstrate non-trivial technical improvements in existing product categories. However, the nature of the improvement was largely one of extending existing technologies (i.e., by using ASICs with .13 rather than .18 traces). The result of T3 innovations could well be the next Moore's Law jump in speed/computing capability. However, we see these as obvious (if non-trivial) serial extensions in existing technologies rather than truly disruptive innovations. We also tend to see T3 improvements as substantially less defensible in the long term than T1s or T2s. After all, a first-mover Moore's Law announcement by Player A invariably is matched within months by Players B, C, and so on.

- Our T4 designation was used for companies able to demonstrate modest improvement in existing technologies, perhaps by repackaging a combination of already-commercialized technologies in novel ways. In many ways, T4 is like T3 but with less significant improvement over what came beforehand.

- Our T5 designation was used for companies that essentially did not create new technology, but were able to successfully market existing technology. Alternatively, companies developing new business models using well-established Internet technologies (i.e., eBay or Amazon) would receive T5 designations.

The results of this process (Table 3; for a complete review by company, see www.signallake.com/innovation) were startling, in that they indicated a surprisingly low degree of technological innovation:

- Only two companies (Akamai and Verisign) could boast of developing T1 technologies.

- Only three companies (RealNetworks, Ciena, and Inktomi) had developed T2 technologies.

- Together, T1 and T2 companies only represented 0.7 percent of all IPOs.

- In contrast, there were forty-one T3 companies (5.4 percent of all IPOs). These included at least three companies that were acquired for very large premiums (E-Tek by JDS Uniphase; MMC by AMCC; and Galileo by Marvell) and ten companies that were worth $1 billion or more as of December 31, 2002. The latter include: Juniper, Marvell, Broadcom, Netscreen, BEA, nVidia, RFMD, AMCC, and Maxtor.

- At the lower end, there were 402 T4 companies and 315 T5 companies (together, 94.0 percent of all IPOs). Some major T4 companies include: Network Solutions, Tycom, Alteon Websystems, Verio, broadcast.com, Arrowpoint, LHS Group, and L-3 Communications. Some major T5 companies include: eBay, Amazon, software.com, Geocities, Ameritrade, hotels.com, and Expedia.

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Table 3
IPO Companies for 1997-2002
By Technology Ranking

Technology Ranking	# of Companies	% of Total Companies
T1	2	0.3%
T2	3	0.4%
T3	41	5.4%
T4	402	52.7%
T5	315	41.3%
Total	**763**	**100.0%**

Source: Morgan Stanley *Technology IPO Yearbook*; Signal Lake Analysis

Examples of Successful T3 Companies

Given that our rankings are surprisingly low, we thought we should illustrate why some specific companies (generally considered to be high-tech success stories) deserve to be ranked T3 rather than T1/T2.

Our first example is Juniper. Juniper is an excellent company with a market value of $2.55 billion as of December 31, 2002, and $11 billion as of February of 2004. That's not bad, particularly when compared to Lucent's $18 billion. As venture capitalists (VCs), we would have been happy to have been Series A investors.

So why did we rate Juniper as a T3? The answer is that we see Juniper largely as a successful execution play rather than as a poster child for brilliant innovation. After all, what did Juniper do? It took well-established routing technology (already commercialized by Cisco, Ascend, and Cascade) and created custom ASICs that allowed it to sell the first 1 gbps router, filling a gap left open by Cisco. That's nice, but it's not an example of developing fundamentally new technology or opening up a new product area. At best, it's a T3 Moore's Law advance.

What really made Juniper a success was not its technology per se, but rather its ability to get funding/support from Lucent, Nortel, Siemens, and Ericsson simultaneously, and a skilled management team able to fill a market need quickly to the exclusion of others like Redback and Avici (who had similar technology but failed to gain traction).

A second example is Broadcom. Broadcom began by working with the CATV industry on the detailed specs for its DOCSIS cable box technology. It then was able to leverage that detailed knowledge by creating chipsets for cable boxes. There wasn't anything particularly innovative about that; simply the leveraging of asynchronous knowledge.

A third example is E-Tek. E-Tek was developing a variety of optical networking components based on innovative views of market needs and materials and technical capabilities. As such, E-Tek was not innovating per se, but using a sharp focus on market needs to drive product engineering.

A final example is BEA. BEA was originally set up to provide to the Global 200 the open-standards-based software needed to run enterprises rather than relying on proprietary IBM-oriented offerings. As such, BEA originally bought the Tuxedo transaction processing monitor from Novell, which provided a customer base, and then extended this in a very astute acquisition of Web Logic to move into the Internet-based enterprise market. As such, BEA was not innovating per se, but again, using a sharp focus on market needs to drive product development and acquisitions.

Conclusion

After looking at these successful high-tech companies, we come away with the conclusion that even during the peak venture capital years, venture-capital-funded companies were not responsible for all that much innovation. The issue, then, is why aren't we seeing more innovation? In the balance of this chapter, we consider three possibilities:

A. True innovation is less profitable than lower-risk "singles" and "doubles." Therefore, smart VCs will avoid investing in high-risk T1/T2 plays.
B. Innovation goes through cycles rather than continuous change. The late 1990s was a period of improvement, not fundamental change. There simply weren't many true innovations to be funded in the late 1990s.
C. There are structural reasons why VCs tend not to fund fundamental change.

A. Is True Innovation Less Profitable?

On one hand, innovation costs money, takes time, and involves higher risks. On the other hand, the higher the hurdle, the greater the degree of competitive advantage once the hurdle is overcome.

To help answer this question on an empirical basis, we reviewed the 763-company IPO database and asked ourselves the following questions:

- Do companies with T1/2 rankings enjoy higher IPO values than companies with T3 rankings? How do these compare to companies with T4/5 rankings?
- Post-IPO, which companies enjoy higher valuations?

Looking first at company valuations at the IPO date (Table 4), we note that the mean IPO value peaks for T1/2 companies at $1.327 billion versus $1.078 billion for T3 companies and around $500 million for T4 and T5 companies. If we use median data to avoid over-representation of outliers, the results are even more favorable for T1/2 companies. (The T3 mean in particular reflects a few extremely high IPO values, such as Corvis at $11.8 billion, Transmeta at $2.6 billion, Avenex at $2.3billion, and Axcelis at $2.1billion. By December of 2002, these four companies with a combined IPO value of $18.8 billion were worth $1.1 billion).

So, at least as of the IPO date, high-tech companies generate higher returns.

Table 4
IPO Companies for 1997-2002
Mean and Median Per-Company IPO Values

Technology Ranking	Mean IPO Value $MM	Index	Median IPO Value $MM	Index
T1/T2	*$1,327*	252	*$1,327**	490
T3	$1,078	205	$398	147
T4	$506	96	$229	84
T5	$470	89	$274	101
Total	**$526**	**100**	**$271**	**100**

Source: Morgan Stanley *Technology IPO Yearbook*; Signal Lake Analysis
* Due to small sample size, mean data was used in place of median

What about a few years later? Looking first at the mean (Table 5), we now see that T3s had the highest per-company values ($1.239 billion as of December 31, 2002) and the best performance versus original IPO price (+15.0 percent). In contrast, T4/5 companies declined 24 percent; T1s experienced a –21.5 percent decline.

Table 5
IPO Companies for 1997-2002
Mean and Median Per-Company Values
As of 12/31/2002*

Technology Ranking	**Mean IPO Value $MM**	**Index**	**Mean 12/31/02 Value $MM**	**Index**	**Percent Change**
	$1,327	252	*$1,042*	246	-21.5%
T3	$1,078	205	$1,239/ $726**	292	15.0%
T4	$506	96	$383	90	-24.4%
T5	$470	89	$360	85	-23.5%
Total	**$526**	**100**	**$424**	**100**	-19.4%

Source: Morgan Stanley *Technology IPO Yearbook*; Signal Lake Analysis
* Companies acquired prior to December 31, 2002, are valued at year-end 2002 at their acquisition price
** After excluding Corvis, Transmeta, Avenix, and Axcelis

However, the T3 means are skewed by three companies that were sold (for stock) at the peak of the Internet frenzy at very high valuations (E-Tek to JDS Uniphase at $15.4 billion, MMC to AMCC for $4.5 billion, and Galileo Technologies to Marvell at $2.6 billion). Even though Morgan Stanley lists the December 31, 2002, valuations for these companies at the acquisition price, we believe this substantially overstates their long-term value as part of the parent company (after all, at the end of 2002, the parent valuations were down over 90 percent). If we exclude E-Tek, MMC, and Galileo, the T3 mean for 2002 drops from $1239 million to $726 million, which is substantially lower than the T1/2 mean ($1.042 million).

This is reinforced by looking at the medians for the end of 2002 (Table 6). T1/2 companies outperformed the median T3 company by a factor of 3.6 times and outperformed the median T4/5 company by around twenty times. T1/2 companies also lost less value than T3/4/5 companies post-IPO.

Table 6
IPO Companies for 1997-2002
Mean and Median Per-Company Values
As of 12/31/2002*

Technology Ranking	**Median IPO Value $MM**	**Index**	**Median 12/31/02 Value $MM**	**Index**	**Percent Change**
	$1,327	490	*$1,042***	1,555	-21.5%
T3	$398	147	$286	427	-28.1%
T4	$229	84	$69	103	-69.8%
T5	$274	101	$32	48	-88.3%
Total	**$271**	**100**	**$67**	**100**	-75.3%

Source: Morgan Stanley *Technology IPO Yearbook*; Signal Lake Analysis
* Companies acquired prior to December 31, 2002, are valued at year-end 2002 at their acquisition price
** Due to small sample size, mean data was used in place of median

To conclude, we don't see that the small number of T1/2 companies is the result of inherently lower returns. The median data (and mean data adjusted for specific outliers) suggest that T1/2 companies have higher, not lower, returns. Thus, there is no inherent reason why VCs should avoid investing in T1/2 companies.

B. Does Innovation Go Through Cycles Rather Than Continuous Change?

Our next hypothesis is that there simply weren't many good T1/2 deals out there to invest in.

On one hand, that seems strange, given the amount of capital that came into the market during the 1997 to 2002 period. Obviously, some people thought there were prospective innovations worth investing in.

One reason for this was the omnipresent belief in Moore's Law, and in particular the idea that technology improvement is continuous, automatically doubling capability every eighteen months. Even the idea of "disruptive technology" was transmuted into one of the driving factors supporting continuous Moore's Law change (i.e., the idea that one can "plan" for a continuous stream of discontinuous change).

However, that isn't the way things actually work. In biology, paleontologists have pointed out that there are periods of rapid evolution and periods of slow evolution—triggered either by climactic changes and/or random genetic mutations that show immediate natural selection value.

The same thing arguably occurred in telecom. In the 1980s, there were some profound changes in the high-tech world: the development of the PC, TCP/IP routing, the World Wide Web, photonics, and cellular telephony. These changes enabled the formation of some truly innovative companies: Apple, Cisco, Netscape, Corning Fiber, Sun, and Qualcomm (Microsoft was less an example of true technological innovation than brilliantly executing against a horrendous IBM error).

Once these changes occurred, there was less need and less opportunity for additional T1 improvements. Instead, what was needed were Moore's Law improvements that could support five to ten times annual bit stream increases without the entire Internet crashing and burning. We also needed (and got) e-commerce plays leveraging the new Internet capabilities in different ways.

If so, then one reason for the lack of T1s and T2s is that the 1997 to 2002 period wasn't a time for fundamental change. It was a time for T3 Moore's Law improvements and T5 e-commerce plays.

On the other hand, we can't help thinking that out of 763 IPOs, there couldn't have been more than five T1s/T2s. We also can't help thinking that the industry didn't really need a Juniper, Foundry, Yuri, Redback, and Avici (along with Pluris and others) each trying to do the same thing. With all the new money coming into the venture capital industry, why wasn't there more real innovation?

For that, we turn to venture capital industry structural factors.

C. Are There Structural Reasons Driving the Lack of Innovation?

As VCs ourselves, we've had ample opportunity to see how a broad range of our colleagues make investment decisions. Based on that experience, we believe a non-trivial part of the problem is structural, not cyclical. Here is our top fourteen list of contributing factors:

1. **Key metric is IRR, not innovation:** One important reason for lack of innovation is that this isn't really what VCs are trying to do. They're in it for the money. If they can do this without innovation, they don't really care all that much. So while by our standards, Juniper may be an execution play rather than a technological tour de force, we doubt the people at Kleiner care all that much. Ditto for the VCs who funded Amazon or eBay.

2. **E-commerce bubble returns**: When you can make triple-digit returns on e-commerce plays in six to eighteen months based on a bubble market "sell" (typically that Company X would seize a nontrivial fraction of United States gross domestic product from brick-and-mortar incumbents), why invest in real technologies with non-trivial risk?

3. **Limited time horizon (A):** During the height of the Internet mania, VCs got used to putting together groups of ten to twenty engineers with nifty PowerPoint decks and then selling the companies to Lucent, Nortel, Cisco, or PMC Sierra for $250 to

$500 million without even a working prototype. That's not conducive to doing fundamental research.

4. **Limited time horizon (B):** Paradoxically, the post-2001 high-tech depression has had a similar effect. There's a lot less money out there to invest; even where VCs are sitting on large piles of cash, they are afraid to invest it in the current climate. As a result, startup companies rarely get more than $5 to $10 million in a single round, and the money generally is expected to last for two years. On $200,000 to $400,000 per month, you can't do all that much real research and development (remember that all venture-capital-funded companies spend lots of money on non-engineers (i.e., the chief executive officer, chief financial officer, vice president of sales, vice president of business development—we've always found it amusing that startups with ten people need a chief financial officer). And no one is going to fund a development project that takes four to five years with significant development risk.

5. **Limited time horizon (C):** Most venture capital funds have six- or seven-year lives, with the investor expectation that things will be wound down by then. So how can a venture capital fund invest in something that might take longer than this to generate a payback?

6. **Limited time horizon (D):** In reality, the time horizon is even shorter than this. Most successful venture capital funds are fully invested in three to four years, at which point the venture capital general partner needs to raise the next fund. That's a lot easier to do if the fund valuation is showing upward valuations, particularly if there have been some good liquidity events. It's a lot harder to do if you've invested in a bunch of long-term high-risk/high-reward deals, some of which have already folded and none of which are anywhere near liquid.

7. **Limited time horizon (E):** *The Innovator's Dilemma* points out that in the early years of a disruptive technology, the new technology may have higher cost with lower functionality than

the mature technology it is seeking to supplant. If so, then venture-capital-backed companies will not have the financial ability or interest in nurturing the new technology through the early years of the experience curve.

8. **Lemming mentality:** VCs love to invest in deals that are fashionable. No one likes to invest in anything that seems particularly daring to investors. As a result, we see lots of otherwise indistinguishable deals for whatever is hot at the moment (right now, that's security and wi-fi, and perhaps nanotechnology). We tabulated the electronic high-tech funding announcements in *VentureWire* for February of 2004. There were numerous security deals; wireless LAN deals, and so forth. There was not a single unexpected business deal.

   The problem with this? If you are going to invest in fashionable companies, by definition these are going to be companies that are variations on the same technology themes. Because of this, it is highly unlikely that these fashionable companies are going to be doing anything particularly innovative or revolutionary. The result? Along with Juniper, you get Foundry, Redback, Avici, and Pluris, not earth-shattering new technology like xerography. (Paradoxically, investing in "me-too" deals heighten the odds of failure: Look at storage area networking or network interface cards for servers, for example. But the added comfort level can sometimes overwhelm rationality!)

9. **Business school mentality:** Most VCs are M.B.A.s, not engineers, and very few are visionaries. Many of them were investment bankers before they became VCs. They over-rely on market demand forecasts and under-rely on vision and gut feeling. This stacks the decks against truly innovative ideas.

   A good example of business-school-type thinking getting in the way of visionary thinking is the apocryphal story about the McKinsey study in the 1930s that looked at the market demand for xerography. McKinsey decided that xerography competed with carbon paper, and that the market was way too small.

When you extrapolate off the past, you rarely hit mega-home-runs.

10. **Emphasis on serial entrepreneurs:** VCs love funding companies with a management team from other successful startups. The problem with this? First, it isn't necessarily clear that a successful first-time entrepreneur was smart as opposed to lucky. Being at the right place at the right time does wonders for your apparent intelligence, and the bankruptcy courts are filled with entrepreneurs who made millions, then doubled down with large loans for Startup Number Two secured by the assets of Success Number One, only to find that they were lucky, not smart, the first time out.

    Second, even assuming that the entrepreneur was smart as well as lucky, a serial entrepreneur by definition will be looking to do logical extensions of existing technologies. (Clearly, there are exceptions that prove the rule). These are the kinds of opportunities they are being exposed to by their colleagues. So while Startup Number Two may indeed be well funded and even commercially successful, it is unlikely to do so based on truly disruptive technologies.

    Third, lightning typically does not strike twice in the same place. If we're looking for truly innovative ideas, we're probably not going to find it with someone who has already had their moment in the sun.

11. **Disdain for pure researchers:** If VCs love funding serial entrepreneurs, they hate funding pure researchers with no business expertise. So, even if the pure researcher has invented the next xerography, they won't get serious attention from most VCs.

12. **Limited technical expertise:** Many VCs are populated with M.B.A. types who understand business but don't really understand technology at an in-depth level. To the extent that true innovation involves complex technologies (some of which

may be ready for primetime, some of which may not), the VCs are not well equipped to handle truly revolutionary technology.

13. **Bloated venture capital fund size:** The A-list VCs with $1 billion or more under management clearly have the wherewithal to fund larger innovation projects for longer periods of time. Unfortunately, the large sums of money appear to have pushed many of them in the direction of trying to find investments that can "use" $25 to $50 million (or more) at a time. This inexorably makes these funds look less and less like early-stage VCs, and more like LBOs. In the process, new innovation opportunities get short shrift, because they can't use up enough of the fund's capital for years to come.

14. **Corporate VCs**: *The Innovator's Dilemma* highlighted how normal corporate processes move research and development (and corporate venture capital funding) toward line extensions and ideas with synergy potential rather than investing in disruptive technologies that cannibalize the base business. And the internal venture capital general partner equivalents typically have the same short-term-oriented financial incentives as standalone VCs. So, corporate VCs are not likely to be the source of innovation either.

One Solution to Private Equity Funding of Innovation

Today in the United States, we're seeing the same type of incrementalism highlighted in *The Innovator's Dilemma*, a behavior attributed in that book to established corporations who have a lot to lose from disruptive technologies. One would have thought that VCs, who have nothing to lose and a lot to gain, would be more supportive of new technology. If the source of the problem is structural, we need to look to structural solutions for the answer.

In particular, what we think we need is a new kind of fund focused on developing important new technologies as opposed to conventional VCs funding incremental improvements to established technologies. Just as the industry has for many years distinguished between early-stage and late-stage VCs (allowing major private equity investors to invest in a balanced package of funds), so should there be special venture funds focused exclusively on high-innovation opportunities.

As we pointed out in "Innovation and Profitability," high-innovation investments will not necessarily result in low returns. Just the reverse. So, such a fund could foster innovation and be profitable at the same time.

Naming the Fund

What to call such a fund? In "Death of Innovation Revisited," we established a ranking methodology for assessing the degree of technological innovation using a 1-5 scale:

- We reserved our highest rank (T1) for new technologies representing a fundamental departure from anything existing previously, and whose commercialization made possible an entirely new (and important) business market.

- Moving down one notch, we ranked a company as T2 if it was able to demonstrate fundamental technology improvement in an existing product category.

- Our T3 designation was reserved for companies able to demonstrate non-trivial technical improvements in existing product categories. However, the nature of the improvement was largely one of extending existing technologies.

- Our T4 designation was used for companies able to demonstrate modest improvement in existing technologies, perhaps by repackaging a combination of already-commercialized technologies in novel ways. In many ways, T4 is like T3 but with less significant improvement over what came beforehand.

- Our T5 designation was used for companies that did not create new technology, but were able to successfully market existing technology. Alternatively, companies developing new business models using well-established Internet technologies would receive T5 designations.

Since the top two rankings clearly reflect important new innovation, we have decided to call our prototypical high-innovation fund the T1/T2 Fund, or "T12F."

Key T12F Elements

Our idealized T12F would look something like this:

1. **Investment portfolio:** T12F would invest in ten to fifteen different investment opportunities, beginning with seed and Series A investments. It would continue to invest in portfolio companies so long as they are making acceptable progress against defined milestones. Companies not making acceptable progress will be pruned.

   By the end of the fund, there may only be one to three companies remaining in the portfolio, each progressing toward major success.

2. **Degree of diversification:** As the fund matures, there will be low diversification by normal venture capital fund standards, since the odds of developing multiple major successes in a single fund are low. Institutional investors will need to obtain diversification by investing in other funds.

3. **Fund lifecycle focus:** As a result, T12F would start as an early-stage fund, but evolve over time into a late-stage fund. Fund II, III, etc., would continue the cycle with new portfolio opportunities so the T12F general partner would continue to look for major opportunities at any point in time.

4. **Fund life:** Given the sole focus on major opportunities, it may take longer than the normal two to three years to be fully invested. Also, since it may take three to five years for a T12F company to have commercialized product, the fund life needs to be longer than the normal six- or seven-year venture capital fund. We think ten to twelve years may be needed.

5. **Fund size and reserves:** Given that T12F will need to be able to reinvest in multiple successive rounds of successful portfolio

companies and to sustain ten to twelve years of operations, the fund will need to be at least $50 to $100 million in size.

6. **Round leader:** Given that T12F by definition will be looking to invest in major opportunities other funds may be overlooking, in most instances, T12F must be prepared to lead or co-lead each round.

7. **Market size hurdle rate:** The market opportunity must be at least $1 billion annually, with company revenues of $500 million annually.

8. **Competitive advantage:** The market opportunity must have significant competitive advantage, and hopefully few competitors. This means the T12F will not be investing in "fashionable" companies unless the investment candidate has a clear path to market share leadership based on superior technology (not simply based on the prospect of superior execution).

9. **Links with labs:** Most VCs emphasize close contacts with serial entrepreneurs, and they develop business plans around a core seasoned business team.
   For T12F, the key source of business deals must be the (remaining) corporate research labs and university labs, since this is where the fundamental research continues to take place. As Willie Sutton used to say about why he robbed banks, "Because that's where the money is."

   There are two problems with a lab-centric approach, however, that we deal with below:

   - A lot of research is not ready for primetime. Some technologies take years or even decades to mature and require fundamental advances in multiple disciplines before commercialization is possible. In dealing with labs, one needs to be able to distinguish between "ready for primetime" and "science project."

- Researchers are not businesspeople. They don't know how to run businesses.

10. **Need in-depth technical (as well as business) expertise:** T12F needs the ability to distinguish between science projects and technologies whose time has come and can be commercialized. This means having GP members with technical degrees in relevant areas who can communicate effectively with researchers and understand the technical issues without over-filtering.

    It also means having people who understand the business side and can make appropriate business decisions. Indeed, until a professional business team can be recruited, the VC effectively **is** the business talent.

11. **Higher investment multiple:** T12F portfolio companies must have substantially higher investment multiple potential than seen in normal venture capital fund investments. For the overall private equity industry, the twenty-year IRR (as of Q2 2003) was 16.1 percent. Most VCs use the "ten times your money in five years" standard, which is equivalent to a 58 percent IRR. That's wishful thinking except for the successes. In a portfolio of ten companies, if you have one with a ten-times return, four with a two-times return, three breakeven returns, and two write-offs, you have a weight-averaged 16.0 percent IRR, which is close to the twenty-year average (Table 1). Even a **normal** venture fund does not generate great returns for most companies.

Table 1
Pro Forma Standard VC Fund

	Per Company: Investment with 10x Return	Per Company: Investment with 2x Return	Per Company: Investment with 1x Return	Per Company: Investment with 100% Write-off	Total
Number of Companies	**1**	**4**	**3**	**2**	**10**
Yr 0	-100	-100	-100	-100	**-1000**
Yr 1	0	0	0	0	**0**
Yr 2	0	0	0	0	**0**
Yr 3	0	0	0	0	**0**
Yr 4	0	0	0	0	**0**
Yr 5	1000	200	100	0	**2100**
IRR	**58%**	**15%**	**0%**	**N/A**	**16.0%**

In contrast, a T12F relying on one company that might not go liquid for ten years with only one success (with the other deals resulting in 100 percent write-offs) needs investment opportunities that generate something like a forty-four times return in ten years on a single big success (along with two breakeven deals and seven complete write-offs) to generate the same 16 percent overall IRR (Table 2). This means T12F needs to look not simply at companies with new technologies, but at companies with technologies that have the possibility of creating a significant new industry (T1s) or which can transform a major preexisting industry (T2s). By definition, the fund must go after elephants, not one or two baggers.

A commonly cited statistic is that in a ten company portfolio for an early stage venture fund, two will be writeoffs, five will recoup the investment, one will generate 2X generate, and the two remaining firms will pay for the entire fund 10X. If this is indeed the case, then it does not make sense to allocate capital equally to all companies, but rather, to triage those that are not offering significant upside in return for placing more money into the real winners. This is not as easy as it sounds: at the start of investment in any company, it appears to have the potential for mega success, which of the ten will be the ones that really pay for all the rest? We now quantify this notion in the next example:

Table 2
Pro Forma T12F Fund

	Per Company: Investment with 10x Return	Per Company: Investment with 2x Return	Per Company: Investment with 1x Return	Per Company: Investment with 100% Write-off	Total
Number of companies	**1**	**0**	**2**	**7**	**10**
Yr 0	-100	-100	-100	-100	-1000
Yr 1	0	0	0	0	**0**
Yr 2	0	0	0	0	**0**
Yr 3	0	0	0	0	**0**
Yr 4	0	0	0	0	**0**
Yr 5	0	0	0	0	**0**
Yr 6	0	0	0	0	**0**
Yr 7	0	0	0	0	**0**
Yr 8	0	0	0	0	**0**
Yr 9	0	0	0	0	**0**
Yr 10	4400	200	100	0	**4400**
IRR	**46%**	**7%**	**0%**	**N/A**	**16.0%**

11. **Aggressive use of option values:** To reduce overall T12F fund riskiness and to reduce the required investment multiple of T12F investments, the fund will need to employ option value thinking. This is eminently doable. Since T12F will be investing in several rounds of successful portfolio companies and will pruning unsuccessful companies, a large fraction of the overall fund dollar investment could be made in later years of the fund when the incremental risk is reduced substantially, thereby generating a higher IRR.

For example, let's assume that T12F invests $33.3 in year one in each of ten portfolio companies (Table 3; the 1/3 investment level compared to the "normal VC" proforma in Table 1 reflects a need to reserve more of the fund's capital to support follow-on rounds). Two years later, the fund writes off its investments in companies 1, 2, and 3, and invests $33.3 each in companies 4 through 7. Two years after that, it writes off its investment in companies 4, 5, and 6, and invests $66.7 each in companies 7, 8, 9, and 10. Two years after that, it writes off its position in companies 7, 8, and 9, and invests $100 in "keeper company" 10, leaving company 10 as the only remaining company in the portfolio. In year ten, the fund sells its position in company 10 for a 12.4 times multiple on funds invested in company 10.

The net result is that on a T12F fund investing a total of $1,000 (the same as in our Table 1/2 proformas), the fund generates a 16.0 percent IRR with the one successful company having a fourteen times multiple, compared to the forty-four times multiple required in Table 2 without option values. The basic point is that the aggressive use of option values reduces the required investment multiple.

Hopefully, by aggressive pruning and follow-up investments in high-payoff deals, T12F will be able to generate higher returns than this.

12. **Back-ended general partner carry:** If we want our T12F fund to generate high returns, we need to give the general partner the incentive to aim high. In place of the standard 20 percent carried interest on profits, T12F should consider lower carries below 16 percent IRR and higher carries above 16 percent.

13.

Table 3
Pro Forma T12F Fund
Using Option Value Pruning

	Company 1	2	3	4	5	6	7	8	9	10	Total
Year 1	-33.3	-33.3	-33.3	-33.3	-33.3	-33.3	-33.3	-33.3	-33.3	-33.3	**-400**
Year 2	-	-	-			-	-	-	-	-	**-**
Year 3	-	-	-	-33.3	-33.3	-33.3	-33.3	-33.3	-33.3	-33.3	**-233.1**
Year 4	-	-	-			-					**-**
Year 5	-	-	-			-	-66.7	-66.7	-66.7	-66.7	**-266.7**
Year 6	-	-	-			-					**-**
Year 7	-	-	-			-	-	-	-	-100.0	**-100**
Year 8	-	-	-			-	-	-	-	-	**-**
Year 9	-	-	-			-	-	-	-	-	**-**
Year 10	-	-	-			-	-	-	-	2900.0	**2,900.00**
Investment	-33.3	-33.3	-33.3	-66.7	-66.7	-66.7	-133.3	-133.3	-133.3	2666.7	**-999.8**
IRR	**Loss**	**Loss**	**Loss**	**Loss**	**Loss**	**Loss**	**Loss**	**Loss**	**Loss**	**70.10%**	**16.00%**
Investment Multiple	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**12.43**	**2.9**

Keys to Success

The key to achieving a successful track record in telecom investing is in going back to the basics: Try to find small teams of people who are technically very competent. From there, it is necessary to nurture that team and feed them small amounts of money to write a business plan. Do a great deal of research into what the products are going to look like by actually talking to the customers. Typically, this is a highly interactive process during which the investor gets an idea of what problem they will be trying to solve. From there, it's a matter of putting in enough money to earn actual revenues from small numbers of orders with multiple customers.

It is key to get one or more big brothers—large entities to validate the endeavor. For example, Juniper obtained investments from Lucent, Nortel, Siemens, and Ericcson, and this went a long way toward validating their activity. This was a special circumstance: All of these companies wanted to compete against Cisco Systems, and they united to do so behind Juniper Networks.

The end result is that the exit strategy then becomes clear to the investor: They will either be a successful standalone company, or they will be partnering with one of the big brother companies. If that is the case, then the big brother will acquire the smaller company, and at that point the big brother already has experience working with them. Hopefully, this will eliminate one of the biggest reasons why so many of these acquisitions fail, namely that the large company does not understand the smaller one.

Bart Stuck received the degrees SBEE, SMEE in 1969, and ScD in 1972, from the Massachusetts Institute of Technology, Cambridge MA. From 1972-1984 he worked at Bell Laboratories (four years in the Mathematics Research Center on signal processing, six years in the UNIX applications area, and two years in the AT&T Computer Business Unit working on strategic planning for market entry). From 1984-1998 he was a Managing Director of Business Strategies LLC, a network computing consultancy that enjoyed over 450 client engagements involving over $60B in capital commitments. From 1993-1998, he personally invested in over 20 privately held companies on over 40 distinct financings, including Ciena, Covad, Tegic, and Torrent Networks, and was instrumental in moving Uniphase into telecoms to create JDSUniphase. Since 1998, he has been Managing Director of Signal Lake, with offices in Boston, MA and in Westport, CT, an early stage private equity fund concentrating on network infrastructure (semiconductors, equipment, software, storage, and value added service providers); all investments have been made into companies with significant intellectual property as core assets.

Mike Weingarten received the degrees BA, MA in History from Columbia in 1973, 1974, and MBA from Harvard Business School in 1976. From 1976-1983 he worked for Boston Consulting Group as a business strategy consultant; from 1983-1992 he was a bankruptcy trustee, managing a wide variety of highly distressed businesses. From 1992-1998, he was engaged with Monitor Group on telecommunications business strategy consulting. Since 1998, he has beena Managing Director of Signal Lake.

We both want to thank all the wonderful entrepreneurs whom we have been privileged to know and to work with over the years: if you are rational, you would not begin to attempt what they persevere to achieve, to create great companies that solve real problems for customers, that excite their customers in new and different ways, again and again. This is their story.

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