

## Re: Space Access Update #112 9/19/05

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*Source:* <http://sci.tech--archive.net/Archive/sci.space.policy/2005-10/msg01158.html>

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- *From:* "Len" <[len@xxxxxxxxxxxxxxxx](mailto:len@xxxxxxxxxxxxxxxx)>
  - *Date:* 29 Oct 2005 13:54:35 -0700
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Tom Cuddihy wrote:

> Len wrote:

>> Dr John Stockton wrote:

>>> JRS: In article <[ti92m1t86lp51j6pvnsdpoe1cb38pgv08@xxxxxxxx](mailto:ti92m1t86lp51j6pvnsdpoe1cb38pgv08@xxxxxxxx)>, dated

>>> Thu, 27 Oct 2005 19:39:14, seen in [news:sci.space.policy](mailto:news:sci.space.policy), Monte Davis

>>> <[monte.davis@xxxxxxxx](mailto:monte.davis@xxxxxxxx)> posted :

>>>>

>>>> Without such a loan, though, the challenge is to find an economically

>>>> viable path \*from\* where we are today \*to\* the promised land of CATS.

>>>> It's a tough challenge that's almost certainly going to require a long

>>>> series of incremental nibbles at the problem from technology AND

>>>> politics AND alt.space entrepreneurial economics... not a "silver

>>>> bullet" breakthrough from any one angle.

>>>>

>>>> That is why it is wrong to concentrate on designing (which in this field

>>>> means "not very expensive") to do cheaply such as : orbit loads of

>>>> tourists; supply ISS; launch satellites; launch interplanetary vehicles

>>>> or components.

>>>>

>>>> Instead, the aim should be primarily, by showing the design, to get

>>>> enough money to develop, build, and launch it, and secondarily that its

>>>> success should advance the cause of affordable access in some way or

>>>> another. If the first is achieved, the second is near-inevitable.

>>>>

>>>> While I generally agree that the main path should be

>>>> going for the money, one should not dismiss easily the

>>>> importance of conceptual design early in the game. This

>>>> is true for two reasons: 1) A good design may--not always

>>>> --help get the money; 2) Once serious money starts to be

>>>> spent, then all opportunity for thinking on the conceptual

>>>> level is gone; all effort then goes into detail design of

>>>> the existing concept, right or wrong.

>>>>

>>>> This is one of the things that has gone so wrong with

>>>> procurement. There used to be far more opportunity for

>>>> relatively free-structured exploratory study. Now the

>>>> emphasis is on total package procurement, without ever

>>>> figuring out a basically good approach. What passes for

>>>> exploratory study these days tends to be limited to someone's

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- >> preconceived notion of a good approach to a basic need or
- >> problem—rather than a wide-open investigation of the basic
- >> need itself (contrary to OMB A-109).
- >
- > I don't know, from my perspective it is actually an overemphasis on
- > studies and requirements justification that has resulted in a
- > excessively long, underwhelmingly successful process.

.....

I guess I didn't state my case well enough, since we seem to be on the same side of this argument. I agree that there has been an overemphasis on requirements justification—and, I might add, on the \*method\* for arriving at a good concept, rather than the quality of the concept itself. As far as I am concerned, if someone can come up with a really good concept that stands up under rigorous analysis and meets a real need, stated or not stated, then I really don't care how that someone managed to come up with the concept.

- >The exploratory
- > studies that tend to focus on long-range technology forecasts and
- > 'stretch' requirements result in overblown, underbudgeted requirements
- > that end up being undoable. Witness FIA. Witness SBIRS low. SBIRS high.
- > Advanced Wideband Communications Architecture. Joint Tactical Radio
- > System. Other unmitigated disasters are in the making, I guarantee.
- > The hits go on. What they all have in common is that they followed a
- > rigorous, careful 'study and analysis' process, which focused on trying
- > to carefully decide the exact right way to do things, then justifying
- > it, etc—still to find themselves horribly unrealistic with regard to
- > estimating what could be accomplished and how much it would cost.
- >
- > I think what it tends to come down to in the end is two things—first
- > an arrogance that you can somehow predict the future. We build these
- > systems that are going to take 10 years to get into orbit. Knowing
- > anything we start building today will be obsolete by that time, we
- > build in upgrade periods DURING THE DESIGN PROCESS, or we insist on
- > requirements that we're not sure we'll be able to meet, based on some
- > loose parametric analysis of technology trends. This is usually at the
- > technical level, where we say 'we know we can't do X today on orbit,
- > but given current trends, we will be able to do that on orbit before we
- > get to that point in the program...' Then when our technology falls
- > short of expectations, we have to scramble to provide a tenth of what
- > we intended to do. By the same token, sudden upgrades often cause
- > programs to veer well into the building process, which causes even more
- > delays and increases in cost.

The most neglected and poorly supported concepts are the ones that do not require any new technology—just better design. The government gets quite excited about ideas that improve current systems a few percent, or about systems that

depend upon questionable, exotic technology that take the 10 years or more that you mention. However, there tend to be no doors to knock on, when it comes to ideas that can revolutionize the way of doing business, just by putting together things we already know how to do into a better overall system. Years ago, one government agency with only five or six people on its staff managed to come up with: 1) recoverable capsules from the first intelligence satellites; 2) the U-2; 3) the A-11/A-12; 4) Glomar Explorer; etc.—all on rather tightly controlled, well-managed, modest budget. But none of these programs were based upon unobtainium; nor did they take more than about three years. The mainstream bureuacracy could not tolerate such a group, and they were eventually cut down to size.

- > The second thing we tend to do, even as we assume we'll have
- > nearly obsolete satellites even before we launch them, is we somehow
- > think by understanding this concept, we can therefore ignore the
- > reality of that fact. This is what causes us o overreach with
- > requirements. Since we don't know how to do what we want to do, we just
- > make it a 'requirement' to do what we want to do, as if this will
- > magically make the solution feasible economically or technically.
- >
- > What we need in the government is 'reality based requirements,' an
- > understanding from the beginning of the conceptual process that every
- > requirement has a technically feasible corollory. The only way to do
- > this is to limit the design-to-fly time to a period of under 5 years.
- > Anything longer, the technology is obsolete by the time you launch it.
- > Anything that length or less, even if it doesn't work exactly the way
- > you envisioned it, at least you can launch an upgraded replacement
- > before going too much longer.

I think that a better way of saying this would be 'reality based technology' for most, not all, projects. I actually feel that the requirements process is too much bound by the perception of what is capable, because the group stating the requirement does not want to appear silly. Accordingly, I think two levels of requirements might be appropriate. The first level would be tempered by what people think is possible (or real). However, another level might allow for capabilities that may be quickly within reach, although not currently foreseen as realistic.

Best regards,  
Len (Cormier)  
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- > tom

• **References:**

- ◆ **Re: Space Access Update #112 9/19/05**  
    ◇ From: Len
- ◆ **Re: Space Access Update #112 9/19/05**  
    ◇ From: Fred J . McCall
- ◆ **Re: Space Access Update #112 9/19/05**  
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    ◇ From: Monte Davis
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- ◆ **Re: Space Access Update #112 9/19/05**  
    ◇ From: Derek Lyons
- ◆ **Re: Space Access Update #112 9/19/05**  
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- ◆ **Re: Space Access Update #112 9/19/05**  
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