

Is it Time to Rethink Project Management Theory?

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Consider this a transition point in my various thinking and writings about program management and by extension management of the projects that comprise these programs. It is driven by a simple glaring fact that our industry more often than not “fails” in our delivery of large projects. I will leave the debate on whether failure is the right term to use until another time but it would certainly be safe to say that large projects “underperform” with respect to the baselines upon which final investment decisions are made and projects “sanctioned”. This performance issue has been well documented by others.

- ◆ **IPA**
 - **“Mega Projects executed around the world have a failure rate of 65%...” IPA**
- ◆ **E&Y**
 - **64% of Oil/Gas Mega Projects suffered Cost overruns**
 - **73% of Oil/Gas Mega Projects suffered Delays**
- ◆ **PMI**
 - **“...2/3 of projects fail to meet their goals... and 17% fail outright...”**

This persistent performance challenge drives me to question whether the theoretical foundations of project management theory as it is widely practiced today are sufficient to meet the challenges of large projects. After all, various management approaches have evolved over time to implement any of a number of management theories. Perhaps large projects, and especially large multi-project programs, require a different theoretical foundation than the traditional theories that underpin our management practices currently afford.

One of the luxuries I have had is to be able to work around very large programs (multiple large projects). This scale tends to “blow up” things that are easily unseen in more routine projects but likely present. My observations have been leading me to a

shift in thinking as it relates to the management of projects, especially the largest ones. This thinking is still evolving but some of the key aspects include:

1. Uncertainty begins at the earliest stages of addressing business needs within the owner's organization. **The owner's inability to clearly articulate his strategic business objectives (SBO), namely what he wants to achieve (outcomes), creates uncertainty in the very foundations of a project.** From my experience in dealing with "underperforming" programs this is the number one contributor. Even if defined, these SBOs must be agreed to by key stakeholders (internal and external) and continuously communicated since project team members change over time.

2. Well founded projects (clearly defined, agreed to and well and continuously communicated SBOs) still are prone to underperformance due to a number of foundational factors which are overlooked or rationalized away. Some of these foundational factors impact the project from the outset while others emerge over time.

Some of these foundational factors include:

- **Optimism bias**, driven by our susceptibility as good engineers to being baited by "framing questions". Daniel Kahneman's Noble Prize speaks directly to these susceptibilities. We fail to use reference class forecasting to provide a check on this bias and even more disturbing really fail to mobilize data which we have to undertake those reference class forecasts.
- **Poorly founded estimate/risk modeling.** Even with excellent average values as a starting point, we assume that the uncertainties around these means reside in a Gaussian world. Results suggest this is not the case and non-Gaussian behaviors and models are increasingly used in modeling of natural (catastrophe) and financial systems. Project management performance is a function of two factors, the validity and resilience of the baseline we are managing to and the effectiveness of our management to it. The industry does much to put baselines in place and improve project execution against those baselines, but what if these baselines are fundamentally flawed (think about a mistake in a line of code in a program that only becomes obvious in extremis but misleads, ever so slightly, in all calculations it does)
- **Assumption migration** that draws on confidence we gained in the past with respect to the "fixed" nature of certain assumptions when project durations were 12 - 18 months. Should our confidence on these assumptions remaining

fixed over a 7, 10 or even 20 year program be just as high? My observations suggest not. I could say much more on assumptions, some relating to optimism bias, but at very fundamental levels we make literally 1000's of assumptions that we don't write down (they are implicit) and as such don't track. Assumptions must be made explicit and tracked.

- **Failing to fully understand the level of complexity** in our project execution strategies and even worse, having no effective tools to measure the relative complexity of approach "A" vs. approach "B". In prior work we were able to take two years out of a ten year schedule by simplifying the project execution approach. In effect we minimized inter-dependencies between main projects to de-risk the program and actually started some elements later than originally envisioned accruing other benefits in the process. Individual construction activity durations were unchanged.
- Inadequate identification of **constraint coupling**. Think of this as second or third order coupling that is present, not easily visible in "complexity", and that can result in non critical path activities without direct critical path dependencies, impacting critical path performance. I've used examples of this in the past
- **"White space risks"** that are not well considered (or one would see elements of constraint coupling) and grow with complexity. It is in this white space that Black Swans nest and breed.

3. Recovering to the baseline, drives a set of management decisions and approaches which may fail to clearly understand the systemic changes in project environment acting on the project until a much later stage when "normal" corrective actions have failed to yield desired results. These systemic changes may be the result of combinations of factors such as "assumption migration", "constraint coupling", "white space risks", inherent complexity and inadequate baselines based on Gaussian modeling in an increasingly non-Gaussian environment. **The effected corrections may act to increase inefficiency of execution as resources are "jerked" from one task to another.**

Is it time to rethink project management theory as it is applied to large programs? I think so, much in the same way that thinking in the broader field of management continues to evolve.

References:

Industrial Megaprojects Concepts, Strategies and Practices for Success; ECI's 23rd Annual Conference 2012; "The European Construction Industry Facing the Global Challenges"; Ed Merrow

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About the Author



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Bob is a member of the ASCE Industry Leaders Council, National Academy of Construction, a Fellow of the Construction Management Association of America, a member of the World Economic Forum Global Agenda Council and several university departmental and campus advisory boards. Bob served until 2006 as a U.S. presidential appointee to the Asia Pacific Economic Cooperation (APEC) Business Advisory Council (ABAC), working with U.S. and Asia-Pacific business leaders to shape the framework for trade and economic growth and had previously served as both as Chairman of the Engineering and Construction Governors of the World Economic Forum and co-chair of the infrastructure task force formed after September 11th by the New York City Chamber of Commerce. Previously, he served as Chairman at Parsons Brinckerhoff (PB). Bob can be contacted at Bob.Prieto@fluor.com.