

The Value of Changing Your Mind

Capital Allocation, Irreversibility, and the Decision System Behind Long-Term Returns

Recently Jennifer Parmelee Witt, Managing Director, interviewed Mark Hayes, Founding Partner of Breakwater Capital Markets, about a capital allocation problem that extends well beyond choosing the project with the highest forecast return. In an environment shaped by technological uncertainty, geopolitical fragmentation, resilience requirements, large AI investments, and faster shifts in competitive advantage, management increasingly has to decide not only where capital should go, but how much should be committed before uncertainty resolves, what should remain reversible, what information is worth paying to acquire, when waiting creates value, and when waiting destroys it. The central proposition is that superior capital allocation is becoming less about forecasting every decision correctly at the outset and more about building a system that learns, stages commitments intelligently, preserves valuable choices, scales evidence, and reallocates capital before deteriorating economics become permanent value destruction.

JENNIFER: Mark, capital allocation is hardly a new subject. What do you think sophisticated investors are still getting wrong about it?

MARK: I think capital allocation is still too often described as a ranking problem: identify the projects, estimate the returns, compare them with the cost of capital, and fund the highest risk-adjusted opportunities. That is an essential part of the discipline, but it assumes management knows enough at the moment of commitment for the ranking to be reliable. In many consequential decisions, that is exactly what management does not know. The deeper capital allocation problem is temporal. How much capital should be committed now when more information will arrive later? Which decisions are reversible if the thesis proves wrong? Which choices close off future alternatives? What is the economic value of learning before scaling? What opportunity disappears if management waits too long? A company is not simply allocating capital among projects. It is allocating capital across commitments, options, and learning experiments, each of which leaves management with a different set of choices afterward. That is why learning and reallocation speed, capital discipline under uncertainty, scenario-based decision-making, kill-and-scale discipline, capital-intensity foresight, and balance-sheet optionality become increasingly important capabilities. The quality of the capital system is not revealed only by whether management chose the right project. It is revealed by whether management designed the commitment intelligently given how much it actually knew at the time.

JENNIFER: Are you saying expected IRR becomes less important?

MARK: No. Expected return remains fundamental because capital has an opportunity cost. The mistake is treating expected return as though it exists independently of uncertainty, reversibility, and the timing of information. Two projects with the same expected IRR can be very different allocations if one requires an irreversible commitment today while the other can be staged as evidence improves. Suppose management has an opportunity to invest \$2 billion in a new platform. One approach commits the entire amount immediately. Another commits \$100 million to answer the three uncertainties that determine whether the larger investment creates value. The first approach may show the higher modeled IRR because it reaches scale sooner. The second may create more shareholder value because the company retains the right not to invest the remaining \$1.9 billion if the evidence disproves the thesis. Conventional ranking can miss that because it focuses on the return conditional on proceeding rather than the economic value of preserving the right not to proceed.

JENNIFER: Then is reversibility itself a capital allocation variable?

MARK: I think it should be considered much more explicitly. A dollar committed to a buyback, a small experiment, a long-lived physical asset, an acquisition, or a liquidity reserve leaves the company with radically different future choices. Economically, those dollars are not interchangeable simply because they all fall under capital allocation. Irreversibility matters because uncertainty resolves over time. If management expects important information to arrive six months from now, the value of waiting or staging can be significant. Conversely, waiting can also destroy value if the opportunity is scarce, network effects matter, a customer standard is being established, or competitors can lock up the market. The decision therefore becomes a comparison among the value of information, the cost of delay, and the degree of irreversibility. That is a much richer problem than comparing two point-estimate IRRs.

JENNIFER: Let's stay there. Is the right to wait itself an economic asset?

MARK: Yes, but only when information is likely to arrive and the opportunity remains available while management waits. That distinction is crucial because waiting is not automatically prudent. If the uncertainty is high and the opportunity is durable, postponing an irreversible commitment can create substantial value because management can learn before risking large amounts of capital. If the opportunity is perishable, waiting may destroy value because competitors secure distribution, talent, capacity, customer mindshare, or network position while management continues analyzing.

I think the timing of commitment deserves to be treated as an economic variable in its own right. Management is constantly choosing between committing with imperfect knowledge and preserving the ability to know more later. The optimal point depends on three rates: how quickly useful information is arriving, how irreversible the commitment becomes once made, and how quickly the opportunity decays if management waits. A company that understands those three rates can behave very differently from one that treats every project as a static NPV comparison.

JENNIFER: That sounds like real-options theory. Companies have talked about optionality for years, but it rarely seems to discipline actual behavior.

MARK: I think that is because optionality is too often treated as an attractive qualitative property rather than as a decision right with a carrying cost. A real option should give management an economically meaningful choice that it would not otherwise possess. The company may pay a relatively small amount today to preserve the right to scale, enter a geography, access a technology, secure capacity, or make a larger investment later. But preserving options is not free. Cash earns less than productive investment. Redundant capacity costs money. Partnerships involve commitments. Experimental programs consume capital and management attention. The investor should therefore ask what specific future decision the option enables, what it costs to preserve, what information management expects to learn, and which future states make exercise rational. If those questions cannot be answered, optionality is probably narrative rather than value.

JENNIFER: You are introducing the idea that some capital earns a return by producing information rather than cash. Isn't that dangerously easy to abuse?

MARK: It would be if management could describe every failed project as "valuable learning" after the fact. The discipline has to exist before the investment is made. A learning investment should specify which uncertainty it is purchasing information about, what evidence will be generated, what subsequent decision that evidence informs, and what threshold justifies the next stage of capital. That distinction between learning capital and scaling capital is extremely useful. Learning capital buys evidence. Scaling capital expresses conviction after the evidence improves. A \$25 million experiment that prevents a company from making a \$2 billion mistake can create enormous economic value despite generating no standalone profit. The same experiment can be wasteful if management never defines what it needs to learn or continues funding the project after the evidence turns negative. The return on learning capital therefore appears in the quality of the subsequent allocation decision rather than in the income statement of the experiment itself.

JENNIFER: That raises another question. Should great capital allocators be trying to minimize the number of mistakes?

MARK: No, not necessarily. They should be trying to minimize the economic cost of being wrong while maximizing the value captured when they are right. Those are different objectives. A company that refuses to make small uncertain investments may have a very low visible failure rate and still miss enormous opportunities. Another company may run many inexpensive experiments, kill most of them quickly, and scale a handful into extraordinary businesses. The second organization may look less “accurate” project by project while being a far better allocator of shareholder capital.

The asymmetry between being wrong early and being wrong late is particularly important. Being wrong early with a small reversible commitment can be cheap and informative. Being wrong late with billions of dollars, a large fixed-cost base, and executive reputation attached to the project can be catastrophic. Conversely, being right but too late can also destroy substantial value if the opportunity is time sensitive. The capital system therefore should not be optimized around avoiding visible mistakes. It should be optimized around making cheap mistakes where learning is valuable, avoiding expensive irreversible mistakes where evidence is weak, and moving aggressively enough when the evidence becomes strong that the company captures the upside before it decays.

JENNIFER: Why are companies so much better at starting investments than stopping them?

MARK: Allocation decisions become embedded in organizations. Once a project has employees, an executive sponsor, board attention, compensation objectives, external commitments, and a strategic narrative attached to it, stopping the project becomes psychologically and politically harder than funding the next phase. The original question, “Is this still the best use of capital,” gradually gets replaced by “How do we make this strategy work?” Sunk-cost bias is part of the problem, but institutional identity is often more powerful. If management publicly described an initiative as strategically essential, reversing the decision can look like failure even when the evidence has changed. That is why sophisticated capital systems establish stopping conditions early. What would make adoption inadequate? What financial threshold would cause management to reconsider? What competitor or regulatory development would invalidate the thesis? Predefining those questions does not eliminate judgment, but it reduces the organization’s ability to rewrite success criteria every time results disappoint.

JENNIFER: Then should investors judge capital allocation partly by deallocation?

MARK: Absolutely. I would go further and say that the quality of capital allocation is often revealed more clearly by what management stops funding than by what it begins funding. Every organization can produce a pipeline of attractive investment proposals. The harder capability is removing resources from something that once had internal sponsorship and redirecting them toward a better opportunity. That does not mean a company should celebrate frequent strategic reversals. Constant churn can indicate poor initial judgment or weak conviction. The important distinction is whether capital moves when the underlying economics change. An organization that never stops anything may look consistent while quietly compounding opportunity cost. A world-class allocator therefore needs a credible deallocation system alongside its investment process: evidence thresholds, exit criteria, accountability, and an organizational culture in which changing a decision because the facts changed is understood as discipline rather than humiliation.

JENNIFER: How would you know whether management really has a deallocation capability rather than simply claiming one?

MARK: I would look at history. Which programs were reduced, sold, partnered, or shut when returns deteriorated? How quickly did management respond relative to the evidence? Were resources actually redirected toward higher-return uses, or did the capital simply disappear into the general budget? Did management explain which assumption had changed, or did the exit occur only after the economics became impossible to defend? I would also look at organizational asymmetry. Some companies have highly formal processes for approving new capital but almost no equivalent process for questioning existing capital. Once an asset or program enters the base, it becomes normal and effectively escapes the competition for resources. That is dangerous because legacy capital competes with new capital just as surely as proposed projects do. A serious allocation system periodically asks whether the existing portfolio still deserves the resources attached to it.

JENNIFER: You are making the decision system itself sound like an asset. Can a capital allocation process really have economic value independent of the assets being allocated?

MARK: I believe so. Imagine two companies with identical balance sheets, identical market opportunities, and identical information at the start of a decade. One organization updates beliefs quickly, stages uncertain commitments, scales projects when evidence strengthens, stops deteriorating programs early, preserves liquidity when option value is high, and reallocates capital toward improving returns. The other commits more rigidly, defends sunk costs, confuses consistency with discipline, and allows organizational politics to slow reallocation. Even if both begin with the same opportunity set, the distribution of shareholder outcomes can diverge dramatically. That suggests the decision system has economic value because it changes how capital compounds across a sequence of decisions. The research places capital discipline under uncertainty, learning and reallocation speed, kill-and-scale discipline, and balance-sheet optionality among increasingly important management capabilities. Those are characteristics of an allocation operating system, not of any one project. A superior system raises the probability that future capital, capital not yet committed to opportunities management does not yet know about, will be deployed intelligently.

JENNIFER: If that decision system is genuinely valuable, why do companies disclose so little evidence that would allow investors to judge it?

MARK: I think it is partly because conventional capital allocation communication is transaction oriented. Companies explain the acquisition, the buyback authorization, the CapEx program, or the dividend decision. Those disclosures tell investors what management decided but often reveal very little about how management ranks alternatives, what assumptions carry the decision, or what would cause the allocation to change. A more informative framework asks six questions around every material allocation: why this capital, what risk is reduced, what return is delayed, what metric proves progress, what would trigger a change, and what disclosure matters. The research argues that best-in-class capital allocation communication makes the decision rule visible rather than merely the decision. That does not require publishing competitively sensitive hurdle rates or internal debates. It means giving investors enough evidence to understand the philosophy governing future decisions, because valuation depends heavily on capital that has not yet been allocated.

JENNIFER: Let's test that with an acquisition. How would the decision-rule approach change the way management explains M&A?

MARK: Instead of beginning and ending with purchase price, synergies, and accretion, management would expose the architecture of the choice. Why was ownership economically superior to partnership, licensing, or internal investment? Which source of value justifies the premium? How much depends on standalone economics, how much on synergies, how much on strategic control, and how much on future options that are not in the base case? What return threshold did management require, and what assumption is most capable of destroying that return? The most important question may be the one rarely discussed publicly: what would have caused management to walk away? That reveals whether the decision rule survived the emotional dynamics of the transaction. Acquisitions become dangerous when executive commitment, auction competition, sunk diligence cost, or strategic excitement causes management to increase the price faster than the value. A credible allocator knows the walk-away condition before it falls in love with the asset.

JENNIFER: And buybacks? Management often says its shares are undervalued. How should investors know whether a repurchase is disciplined allocation or simply support for the stock?

MARK: A buyback should compete against every other use of capital. The expected return depends on the gap between the purchase price and intrinsic value, but intrinsic value is itself uncertain. Management therefore needs enough humility to ask why the market disagrees with its own estimate. A repurchase can be extraordinarily attractive when the company has a durable business, excess capacity, few superior internal opportunities, and a meaningful valuation discount. It can also destroy value when management overestimates intrinsic value or buys aggressively while the business itself requires capital. The decision rule matters more than the announcement. How does management compare buybacks with internal reinvestment, M&A, debt reduction, or preserving liquidity? What balance-sheet conditions constrain repurchases? How wide does the valuation gap have to become before buybacks dominate? A company should not need a mechanical formula, but investors should be able to understand the logic by which buybacks become more or less attractive.

JENNIFER: The hardest challenge may be the statement that the highest financial return is not automatically the best choice. Isn't that exactly how bad management teams justify empire building?

MARK: It can be, which is why the burden of proof should increase whenever management asks investors to accept a lower apparent financial return because of resilience, sovereignty, strategic control, or optionality. The research describes capital allocation as a multi-constraint optimization problem and notes that the highest-return option may not always be the economically best one when strategic control, resilience, sovereignty, and optionality materially affect future outcomes. The discipline is to identify the state of the world in which the additional consideration creates economic value. If a lower-return regional facility preserves a major revenue stream under a plausible fragmentation scenario, that benefit can be examined. If owning a critical asset materially reduces dependence on a supplier whose failure would cripple the enterprise, that risk reduction can be evaluated. If management cannot identify the economic consequence, the strategic rationale deserves very little valuation credit. "Strategic" should never end the analysis. It should trigger a more demanding one.

JENNIFER: How do you value resilience when the investment is designed to prevent a loss that may never occur?

MARK: By distinguishing a permanent cost from an insurance premium and from an investment that creates strategic advantage. Those three uses of capital can produce identical near-term cash outflows while having completely different value implications. Some resilience spending depresses near-term free cash flow while increasing the reliability and duration of future cash flows, and a disciplined framework examines cost, payback, duration benefit, and the failure mode being removed. The analytical difficulty is counterfactual. If management spends money to prevent a disruption and the disruption never occurs, investors cannot directly observe whether the capital worked. That means the company has to be unusually precise about the exposure being mitigated, the probable severity of failure, and why the chosen solution is economically efficient. Resilience cannot become a label that immunizes spending from return scrutiny. At the same time, an investor who values only the base case can systematically underprice investments whose principal value appears in adverse states.

JENNIFER: Geopolitical fragmentation creates exactly that problem. How should boards decide whether to accept lower efficiency today in exchange for more strategic control tomorrow?

MARK: They should refuse to decide using one assumed future. A concentrated supply chain may have superior base-case economics and inferior economics under fragmentation. A regionalized system may look inefficient in the base case while protecting revenue, access, and continuity in another plausible regime. The decision therefore depends on the distribution of outcomes, not simply the return under the expected one. Geopolitical exposure increasingly enters financial models through regional revenue, tariff-sensitive margins, duplicate capacity, working capital, country risk, and terminal opportunity. That gives boards a more rigorous way to debate strategic control. What revenue is being protected? What additional capital is required? What failure state becomes less damaging? How much optionality does the investment create? What happens if fragmentation recedes and the additional capacity becomes redundant? Those questions convert geopolitical strategy into capital economics without pretending the future can be predicted precisely.

JENNIFER: Let me force the timing question. How does management decide whether to move early and risk being wrong, or wait for better information and risk being too late?

MARK: That is one of the central capital allocation problems under uncertainty because both errors have economic cost. Moving early can expose the company to an incorrect thesis before enough evidence exists. Waiting can allow the opportunity to decay. The decision therefore depends on the relationship among information arrival, commitment irreversibility, and opportunity decay. If useful information will arrive quickly and the opportunity will remain available, waiting has value. If the commitment is highly reversible, the company may not need to wait because being wrong is cheap. If the opportunity is disappearing quickly, because of network effects, scarce capacity, customer lock-in, regulatory windows, or competitive preemption, the cost of waiting can exceed the value of additional information.

The best allocator therefore does not ask simply, “How certain are we?” It asks, “How expensive is it to be wrong now, how much will we learn by waiting, and what will waiting cost us if we are right?” That creates a different tolerance for error. A small reversible experiment can be rational at relatively low conviction because the downside is bounded and the information gained is valuable. A multibillion-dollar irreversible acquisition deserves much higher conviction because the cost of being wrong is enormous. Conversely, a company facing a rapidly disappearing strategic opportunity may have to commit before certainty is comfortable. Superior allocation is not the elimination of uncertainty. It is matching the size and timing of the commitment to the economics of being wrong and the economics of being late.

JENNIFER: How should investors distinguish a company that is genuinely adaptive in capital allocation from one that simply changes strategy frequently?

MARK: Adaptability is disciplined updating; inconsistency is unstructured reaction. The difference lies in whether changes can be traced to evidence and a stable decision philosophy. A company can change allocations frequently and still be highly disciplined if the environment is changing rapidly and management can explain which assumptions moved. Another company can maintain the same allocation for years and call that consistency even while returns deteriorate. I would look for a stable decision rule operating on changing evidence. Does the company consistently rank opportunities by expected value, return, risk, reversibility, strategic fit, and balance-sheet capacity? Does capital move when those variables change? Does management scale projects when evidence strengthens and reduce them when evidence weakens? Are the reasons for changing course intelligible? The underlying philosophy should remain coherent even when individual allocations change. That is what separates learning from drift.

JENNIFER: If you had to define the best capital allocator in the world, what would that organization be capable of doing that most companies cannot?

MARK: It would be exceptionally good at deciding not only where to allocate capital, but when to resolve uncertainty through commitment. It would understand that a decision has at least three economic dimensions beyond the headline return: how much useful information is likely to arrive, how irreversible the commitment becomes once made, and how quickly the opportunity decays while management waits. In some situations, the rational decision would be to preserve the right to wait because the information value is high and the opportunity durable. In others, delay would be the greater error because competitive advantage is perishable. The organization would therefore treat timing as part of the economics rather than as an administrative detail.

It would also be unusually sophisticated about being wrong. It would not try to maximize the percentage of projects that succeed because doing so can produce an organization that avoids uncertainty and therefore misses exceptional opportunities. It would try to make small, reversible mistakes when learning is cheap; avoid large irreversible mistakes when the evidence is weak; and scale aggressively when the evidence becomes compelling and the opportunity is decaying. In other words, it would minimize the economic cost of being wrong while maximizing the value captured when it is right. That produces a very different capital culture from one obsessed with defending the original forecast.

The same organization would have a genuine deallocation capability. Stop rules would be designed before sunk costs accumulate. Executive reputation would not determine project survival. Capital would move toward improving returns and away from deteriorating economics even when the losing investment had once been strategically important. Existing programs would continue competing for capital rather than gaining permanent entitlement simply because they were already funded. That capability is rare because companies generally build much stronger systems for approving capital than for withdrawing it.

Most importantly, investors would be able to see enough of the decision architecture to believe that future capital, not merely the assets already on the balance sheet, will be handled intelligently. They would understand how management ranks reinvestment, acquisitions, buybacks, resilience, strategic control, partnerships, debt reduction, and balance-sheet optionality; what evidence causes capital to scale; what causes it to stop; and how the organization thinks about uncertainty, irreversibility, information arrival, and opportunity cost. The economic advantage would not come from perfect foresight. No management team has that. It would come from an institutional system that learns faster than competitors, preserves valuable choices when uncertainty is high, commits decisively when conviction deserves it, recognizes when the cost of waiting has become greater than the value of more information, and reallocates capital before uncertainty hardens into permanent value destruction.