Dear Partners,

This month marks the fifth anniversary of my professional investing career: a little over two years as an analyst at a hedge fund, and a little under three years running Askeladden.

An early mentor once pointed out the difference between five years' experience and repeating one year's experience five times (i.e., making the same mistakes over and over again). So I've always endeavored to carefully reflect upon my experiences and distill their critical lessons.

Today, I'm happy to share five lessons I've learned over the past five years, told through the lens of mental models. To do that, we'll need to start with a fun story about toilet seats.

But first, a word on performance. On the surface, not much has happened during Q3; as of 9/30, our performance was roughly in line with performance at the end of Q2.

	Since 2017-01-01 (Last 21 Months) - Cumulative	Since 2017-01-01 (Last 21 Months) - Annualized	Since Inception (2016-01-08) - Cumulative	Since Inception (2016-01-08 - Annualized)	YTD 2018, Cumulative
ACP Gross	~ + 31%	~ + 17%	~ + 122%	~ + 33%	~ + 33%
ACP Gross Less Mgmt Fee	~ + 27%	~ + 15%	~ + 113%	**~ + 31%**	~ + 32%
S&P 1000 Total Return	~ + 26%	~ + 14%	~ + 66%	**~ + 20%**	~ + 10%
ACP Gross Less Mgmt Fee +/– S&P1000TR	~ + 1%	~ + 1%	~ + 47%	~ + 11%	~ + 22%
Outperformance Allocation (30% of above, if any)	~ + 0%	~ + 0%	~ + 14%	~ + 3%	~ + 7%
ACP, Net of Mgmt Fee & Outperformance Allocation	~ + 27%	~+ 14%	~ + 99%	**~ + 28%**	~ + 25%
ACP Net +/- To SP1000TR	~ + 1%	~ + 0%	~ + 33%	**~ + 8%**	~ + 15%

DISCLAIMER: Data is estimated, unaudited, and provided for directional color only. Past performance is not a predictor of future results. We do not expect our future returns to approximate our historical returns. Amounts may differ due to rounding. Please consult your monthly statements from Fund Associates LLC or audited annual financials from Spicer Jeffries LLP for actual returns. Net returns are calculated assuming a hypothetical investor paid the standard fee structure of a 1.5% annual management fee and 30% of the outperformance, if any, vs. the S&P 1000 Total Return index Data is presented only for Askeladden Capital Partners LP and not for any of the separately managed accounts which Askeladden Capital Management LLC (the investment advisor to Askeladden Capital Partners LP) also oversees. Please see additional important disclaimers in the appendix.

Although I'm not doing my job right if the portfolio isn't undervalued, I do believe that it was *particularly* undervalued as of 9/30; moreover, our watchlist has several candidates that could absorb a meaningful amount of capital were we to harvest gains from existing portfolio positions.

Notwithstanding general market valuations, our concentrated small/micro-cap approach continues to yield attractive opportunities that I believe offer substantially above-average return with substantially below-average risk; I'm as optimistic about our prospects today as I have been since launch.

As such, on October 1st, I invested more money from my own personal cash reserves to increase my stake in Askeladden Capital Partners. The investment is modest relative to my net worth and existing stake in Askeladden, but material relative to my annual after-tax cash flow.

Month to date in October, our performance has declined meaningfully along with the broad market selloff - in our experience, very little analysis is being done during such market corrections, and all securities are generally sold indiscriminately regardless of their fundamental characteristics.

We've taken the opportunity to deploy additional capital at what we believe are extremely attractive rates of return, and are very close to fully invested. We've been through this sort of volatility about a half-dozen times since launching Askeladden, and feel like we've managed it better and better each time.

Starting this quarter, I'm separating portfolio commentary from the public investor letter to protect the valuable proprietary research IP we spend our time and resources generating. Clients have separately received a 16-page overview of all material portfolio positions; I kindly ask that you not share that commentary.

Five Years of Professional Investing, Five Counterintuitive Lessons

So much value investing content is *stale*: a cloned rehash of something someone else said. I always try to not do that, so that anyone who takes the time to read this letter comes away with something fresh and new.

I don't claim that any of these five lessons are wholly original, but they're not simply the n+1th debate over the delineation of "growth vs. value," or tired platitudes about being fearful when others are greedy and vice versa. They're all drawn from my experience and integrated with the relevant mental models; if I'd understood them as well five years ago as I did today, I'd have a lot more dollars, and a lot fewer white hairs. Each lesson is self-contained, so feel free to pick and choose the lessons that sound most interesting to you.

Like all of the content on the *Poor Ash's Almanack*, the best free mental models resource available - for which praise has poured in from all corners of the world - hopefully these lessons will be as entertaining as they are educational.

Moreover, all of the lessons here are very *practical*. As someone whose clients are entrusting me to make the best decisions possible with their capital, I don't have time for stuff that sounds fancy and intellectual but doesn't actually make me better at my job.

We'll start, as promised, with toilet seats.

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Investing Lesson 1: Sandwiches, Toilet Seats, and Salmonella.

(Pages 3 - 6)

Mental models: cognition vs. intuition, Bayesian reasoning / priors, base rates, probabilistic thinking

Hey: you ever make sandwiches, or know anyone who does?

Well, if you do, here's a tip for ya: if you want your next PB&J *without* a side of salmonella, science says it's probably safer to make it on the toilet seat than on your cutting board.[1] [2]

That's a piece of advice most people wouldn't just find unconscionable: they'd find it downright *revolting*. It goes against all common sense.

Science and our stomach aside, there's the social-perception angle. Can you imagine chiding your mother-in-law for engaging in the statistically dangerous food-safety practice of making your kid's sandwich on the counter?

> *"Granny, come on now. You know you're supposed to be doing that on the commode. Be a good role model for little Johnny."*

Just imagine what the health inspector would say if she discovered the local deli employees carefully crafting footlongs in the men's room.

When it comes to germs, we sometimes wildly obsess about non-issues and blithely ignore really big ones. Why? Our intuitions are untrained. Most of us without a degree in microbiology have, at best, only the vaguest of understandings of how germs survive and multiply.

So actions that would strike most of us as risky can actually turn out to be surprisingly harmless. My favorite example: you'd think that an easy way to get sick would be vigorously making out, for several minutes, with someone of your preferred gender who has a severe case of the common cold. Turns out, it's actually really hard to catch the common cold this way.[3]

Conversely, actions that might seem totally innocuous to a casual observer - for example, dropping a few cloves of raw garlic into a bottle of olive oil, or leaving a foil-wrapped baked potato out on the counter - can turn out to be *exceptionally* dangerous, and even fatal. [4]

Hooks, Riptides, and Due Diligence

Let's bring this discussion of intuition back to investing. A common aphorism is that the great ideas jump out at you - Warren Buffett does his modeling on the back of a napkin, any good investment idea can be written on a sticky note, yada yada yada.

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[1] 2016 "Today" article citing University of Arizona microbiology professor Dr. Charles Gerba. https://www.today.com/health/your-kitchen-dirtier-toilet-seat-869333 Admittedly, I'm taking some artistic license here; his discussion assumes that people *cut raw meat* on cutting boards and then just casually rinse them before reusing for other purposes. I'm not sure anyone actually does this; personally, I sanitize anything raw meat has come into contact with by pouring boiling water all over it (sometimes multiple times). Still, *directionally*, the point holds, and he goes on to discuss how germ-laden most people's sponges and dishcloths are and how that can contaminate many countertops - but it's harder to make a metaphor about dishcloths than it is about toilet seats.

[2] If you're ever invited over for dinner, please be aware that notwithstanding my newfound knowledge of this **base rate**, I still stick to the conventional approach of preparing food on the kitchen counter rather than the toilet seat. Diner beware…

[3] This lovely tidbit is courtesy of Jennifer Ackerman's "Ah-Choo." The best part: some review board actually *sanctioned* a study that paid volunteers to make out with rhinovirus-y strangers. Somewhere, Shawn Achor is wondering what he has to do to get a charades study approved…

[4] https://www.fsis.usda.gov/wps/wcm/connect/a70a5447-9490-4855-af0d-e617ea6b5e46/Clostridium_botulinum.pdf?MOD=AJPERES

This is often surprisingly true: despite the fact that I typically create research documents that often range from 10 to 50+ pages in length on an initial pass, most of that work is due diligence - a series of checks, whether quantitative or qualitative, on the validity of an initial "hook" into an idea.

It's certainly not uncommon for a "hook" to turn into nothing - some ideas that seem intriguing turn out to have flaws once you dig deeper, which is why it's important to do the work. But I find the converse to be true far less often: rarely do I start profoundly uninterested in something, and after days or weeks of painstaking work, find some magic detail that all of a suddenly seals the deal.

This "know it when you see it" hook recognition is a common pattern I've heard from talking to managers with far more experience than me. And it's a bit perplexing, because it contradicts my early experiences as an individual investor: I'd get excited about ideas on the basis of very little information… and, predictably, they'd turn out to not work out nearly as well as I thought they might.

What's changed? Well, to return to the section heading, Kip Tindell had it right when he made The Container Store's fifth Foundation Principle about the mental model of **cognition vs. intuition:** *"Intuition does not come to an unprepared mind. You have to train before it happens."* (More discussion is available in Tindell's *Uncontainable* - UCT review + notes).

It is now generally well-known and accepted that *"we think much less than we think we think"* - or, in other words, much more of our decision-making is driven by automatic heuristics than by careful, procedural reasoning.

But most of the discussion about data vs. judgment (or, framed differently, cognition vs. intuition) misses the critical insight that judgment *with* data works better than either without the other. Nate Silver discusses this point through the lens of meteorology and other fields in *"The Signal and the Noise"* (SigN review + notes); Dr. Jerome Groopman does the same thing for medical diagnoses in *"How Doctors Think"* (HDT review + notes).

Our brains are capable of performing amazingly complex distillations incredibly quickly when they're trained to do so: think of fighter pilots. Or, in a more pedestrian everyday sense, for Americans, the automatic ease with which we translate degrees Fahrenheit and wind/rain forecast into a weather-appropriate outfit. Practice crystallizes cognition into intuition, freeing up cognition for higher-level tasks. In fact, overthinking in critical, time-sensitive situations can often lead to disaster. Cognition is simply too slow.

However, importantly: trained intuition only applies in the circumstances in which it was trained for. When removed from those circumstances, it's useless or worse. Laurence Gonzales drives this home in *"Deep Survival"* (DpSv review + notes), discussing how unprepared we are to emotionally understand the magnitude of forces involved in riptides or avalanches.

Yet we have a tendency to *overgeneralize* the applicability of our trained intuition - we like to think, in other words, that being good at one thing makes us good at everything, as if being a talented investor / lawyer / business manager somehow makes us better-equipped to calm down a screaming three-year old, change the oil in our car, or make mayonnaise from scratch. (Nobody actually believes those examples, but we nonetheless act as if it's true in many important domains of our life).

So, someone who's spent a lot of time analyzing niche industrials and professional services companies (like me) will probably have pretty accurate and useful intuitions about the investment prospects of the n + 1th industrials/professional services company based on a quick review of the situation.

But here's the kicker: all of that time spent studying business fundamentals has given me absolutely *no* useful training in interpreting recent stock-price movements and predicting their future directions. In fact, based on my understanding of neuroscience and economics, there's a strong case to be made that our intuition *cannot* be trained for such tasks, as there are simply too many variables and too much reflexivity to be able to identify and recognize consistent patterns.

Nonetheless, I spent many years implicitly trying to do so, following the classic value investor sensibility of not buying stocks that have run up, and eagerly buying stocks that have pulled back. Over time, I've realized this sort of reflexive reliance on intuition - *"it's more likely to be a bargain if it's pulled back,"* or *"it's not likely to be a bargain if it's run up"* - has little to no basis in reality.

To put it somewhat more formally: five years of professional experience has provided a lot of training in accurately determining intrinsic value of certain kinds of stocks; I spend much of my life thinking about and doing this, and I have gotten better at it over time, and have a high rate of success in doing so.

Conversely, all that time spent on fundamental investment work has not provided any useful training in assessing a stock's recent price action - the **base rate** of me predicting a stock's near-term movements, or its responses to "catalysts" like earnings reports, is woeful. I would, in fact, argue that it is probably impossible for most people to assess such things profitably most of the time (a topic I will discuss more in an upcoming memo… bombshell: Howard Marks, who I've long been a great fan of, gets it critically wrong in his new book.)

Again looking at **base rates**, or statistical tendencies: although I haven't run an analysis on every purchase I've made, I can say that some of my most profitable purchases have been stocks that have run up, sometimes meaningfully - and some of my worst decisions have been when deep short to medium term price declines have activated my bargain-hunting sensibilities (see **product vs. packaging**, or Thaler's excellent discussion of transaction utility in *"Misbehaving"* (M review + notes)).

This isn't to say I've become a momentum chaser (far from it). I've just recognized that I have no rational basis on which to assess the relationship between recent and future price action; as such, it's no longer an input in my investing process.

Mental Models Applied To Portfolio Management

Most investors are familiar with the concept of "circle of competence," but don't understand the underlying mental models thoroughly enough to apply it consistently - resulting in implicit or explicit utilization of non-trained intuition, which, as we have discussed, is inferior to cognition.

The most well-known example: managers with strong bottom-up stock-picking capabilities becoming overly influenced by their own (untrained, usually untrainable, and thus unhelpful) intuition on macroeconomic forecasting.

I'm careful not to go down that road. My approach to staying within the limits of trained intuition - while still finding opportunities to train intuition - manifests itself in Askeladden's process in several ways:

A. It starts with keeping my eyes open and being receptive to the lessons the world offers to teach me. Intuition can be trained; it just takes time and effort. (*Five years of experience vs. one year of experience five times.)*

B. A concept I call "familiarity risk" (an interaction between cognition/intuition and **trait adaptivity**.) When writing research documents and updates, I take the approach of always not only assessing the merits of an idea, but *my own ability to handle it.* What's my historical experience with this company - and broader categories which this situation would fit into, whether that's dimensionalized by sector, business model, or type of investment setup?

That's an input into how aggressively I can underwrite a valuation or size a position, or how long I choose to watch and learn before becoming involved (if ever). The lower the familiarity, the higher the risk, and the more cautious I need to be. The truth is that there are many great ideas other people have that I would never be able to handle - and there are many great ideas I have that other people would never be able to handle.

I have a reputation for being a highly concentrated investor, which is something I'm willing to do, but there's a difference between being *concentrated* and being *stupid*. I'll take big positions when there's an overlap between a great idea and a great ability to handle it on my part, but I routinely take 100 - 300 bps starter positions, too - and, not uncommonly, they never get bigger than that for a variety of reasons. It's all a function of the risk factors on both sides of the table.

C. Constant self-reflection on research techniques - setting aside *companies*, what's working in my process and what's not? What are things I do that consistently fail to add value? What are the things that add value consistently and how can I do more of them? What are things I *could've* done that *would've* added value, but didn't?

(I've previously discussed this last one in the context of "management premiums" for really good management teams - I used to never assign them because I wasn't confident in my ability to assess management teams, but as I've been able to train my intuition in this area, I'm more confident in letting this influence decisions modestly.)

Moreover: look around. What are the things other people do that add value and how can I emulate those? I've enjoyed discussing and collaborating - at various levels of depth - with investors with profoundly different approaches from mine.

This includes a deep-value manager (we're very focused on business quality), a manager who uses extensive primary research (we rarely do so), and, of course, the research documentation process we shamelessly stole from our friend and mentor Zeke Ashton at Centaur Capital.

All of this may sound elementary. It's easier said than done. This isn't about a simplistic, binary, black-and-white "I only invest in XYZ sector or ABC kind of company." It's about assessing - with regards to everything from *research techniques* to *trading decisions* - whether I have any reason to believe that my intuition will be helpful. If it is, I'll make active decisions based on that intuition. If not, I'll try my best to stick to whatever **base rate** is applicable, maximizing my statistical chance of success.

Takeaway: trained intuition is superior to cognition, which is superior to untrained intuition. It's important to differentiate which is which - and to actively work on training intuition.

Investing Lesson 2: Begin With The End In Mind

(Pages 7 – 9)

Mental models: utility, nonlinearity, path-dependency, opportunity costs, n-order impacts, humans vs. econs, local vs. global optimization, bottlenecks

One of the most amusing assumptions of classical economics is that we're capable of "optimizing" our decisions: that is to say, that we evaluate the benefits and costs of any given decision, and compare them against our (of course infinite) knowledge of opportunity costs of *other* possible decisions which this one precludes, and then we further have the (again of course infinite) willpower to make and stick to the right decision all the time.

Sound unrealistic? Yeah - the founders of behavioral economics thought so too. As Richard Thaler relays in *"Misbehaving"* (M review + notes), Amos Tversky once asked an economist:

> *"You seem to think that virtually everyone you know is incapable of correctly making even the simplest of economic decisions, but then you assume that all the agents in your models are geniuses. What gives?"*

Suffice to say that we're not naturally prone to evaluating the universal consequences of our decisions. There are a number of counterintuitive mental models - **n-order impacts**, **feedback**, **complexity**, and so on - that make doing so difficult.

At the simplest level, however, people often fail to utilize a **local vs. global optimization** paradigm when making mental models, thus unwittingly trading off long-term opportunities for what seems like a good idea in the short term. One of the best metaphors for how to avoid this kind of thinking is the Stephen Covey maxim to "begin with the end in mind."

It's one of the core habits in *"The 7 Habits of Highly Effective People"* (7H review + notes), and Covey illustrates it vividly by asking readers to think about what they'd want said at their eulogy. It has a way of putting current frustrations and desires into perspective.

Let's translate this theory into practical investment ramifications: one of the most important lessons I've internalized as a professional investor is that *my goal is not to maximize theoretically achievable returns* - that is to say, returns that could ideally be achieved in some fantasy land.

Instead, *my goal is to maximize practically achievable returns* - what I can *actually* manage in the *real world.*

What's the difference? Allow me to explain.

Every decision made as a professional investor - ranging from firm structure to investment approach - has far-ranging impacts that often aren't considered initially. Here are some examples of the sorts of tradeoffs I've observed in the industry.

- Large firms have more resources than small ones - but also need to spend far more time on compliance, operations, and client management. PMs at larger shops routinely report having far less time for research than at smaller shops. Additionally, the larger the shop, the fewer opportunities are sizable enough to be worthwhile.
- For small shops, adding a few resources can provide a meaningful increase in man-hours to manage research and operations. However, there are also added inefficiencies related to training, communication, and required HR work (tax forms, health benefits, and so on). Moreover, many instances of process failure are related to people issues. Investors who don't anticipate these issues, and aren't adequately prepared to manage through them, often end up with the worst of both worlds.

- More diversified portfolios can reduce risks related to being wrong on any individual idea. At the same time, they suck up far more time in terms of monitoring and trading existing positions, often leaving the investor with less time to conduct *de novo* research to identify new prospective investment candidates.

- Similarly, investment approaches focused on extensive primary research, "channel checks," and so on can conceivably provide incremental insight, but also dramatically restrict the number of names that can be followed and diligenced at any level – it's a question of the **marginal utility** of information.

- The same applies to approaches which attempt to predict near-term events (whether corporate actions or earnings reports). While potential incremental returns might be gained in the short-term, the tradeoff is that all time spent becomes completely worthless once the event or report materializes, vis-a-vis more long-term approaches where research today can have a much longer-term payoff spanning days or years.

- Any form of added "complexity" beyond plain vanilla, unlevered, long-only stockpicking - that is to say, short-selling, the use of options, the use of margin, or the use of complex derivatives - adds meaningful operational burden. Although the use of such instruments can add return, it also - as with the above three items - reduces the time and mental energy an investor has to hunt down new ideas.

None of these are particularly brilliant insights; they are all relatively mundane. Yet despite the fact that they're fairly obvious, I've seen much of the industry operate as if this was not the case - as if these tradeoffs can be hand-waved away.

Indeed, thinking about the longer-term consequence of actions, and how they'll affect other parts of investing, allows investors to make more appropriate decisions. For example:

- Many investors often make the mistake of relying too heavily on other investors' judgment - at the extreme, utilizing "cloning" or "guru" approaches where the investment decisions of "great" investors is used as a starting point. While this could make sense from a **base rates** perspective, there are a number of execution/practical disadvantages. Primarily: when you don't fully understand an investment thesis *yourself*, it is extremely difficult to evaluate and manage the position over time, as you won't know how to interpret or react to incremental data points.

- Many investors fall down the rabbit hole of being seduced by "big ideas" that have substantially more variables/inputs - and thus are substantially less predictable - than bottom-up analysis of individual companies. In trying to incorporate these new worldviews, investors vastly overestimate the payoff and vastly underestimate the risk of interference with existing process.

For someone who is extremely interested in learning and improving, I'm also *extremely picky* about areas where I want to learn and improve. It has to fit into my existing structure and process, which is highly informed by "starting with the end in mind." For example, from a business structure perspective:

- I'm closing to new clients at $50MM in FPAUM to preserve a long runway for continuing to compound client capital without exhausting our small and micro-cap opportunity set. I've heard from numerous investors the challenge of rapidly scaling to, say, $200 - $300MM, then realizing that an investment strategy that excelled with a mid-8-figure asset base doesn't work quite so well with a mid-9-figure asset base.

While determining the exact capacity of any strategy is more art than science, closing early and compounding our way toward whatever that capacity ends up being provides us with much more ability to identify - and adjust to - any variety of problems that might crop up at scale.

- I'm always planning to remain a one-man shop, to avoid the "worst of both worlds" issue of being a small team - there's as much management / communication / HR work as would be required for a larger team, but there's insufficient resources to outsource or delegate those issues to someone else. People management is a very different skillset from investing - not one I've developed, nor one I want to develop while trying to be a great investor.

- I shun investing approaches that would add operational complexity and the consequent burden of invested time and emotional energy.

It also shows up from an investing perspective:

- The "watchlist" approach we utilize enables us to develop a differentiated longitudinal view of companies, allowing us to be more nuanced and objective in our underwriting, grounded in a historical perspective rather than overly focused on recent data points and trends.

- Our research documentation process not only allows us to immediately retrieve due diligence on any individual company, but also allows us to identify cross-situational insights that apply far afield from where we found them.

- Our general avoidance of businesses with risk factors we wouldn't know how to manage or evaluate, such as excessive leverage or commodity exposure, prevents us from ending up trapped in situations we can't handle appropriately.

- Our intentional avoidance of any sort of non-obvious macro view (i.e., something not based on very long-term, very obvious **base rates**) prevents our trained-intuition bottom-up underwriting of individual stocks from being confused by irrelevant exogenous noise.

- Our strong preference for relying solely on our own analysis - i.e., using others' work to provide context and perspective, but never make the decision for us - ensures that we're not caught in limbo if things don't go the way we expect, because we know what we're looking for and how we'll respond if it doesn't materialize.

Takeaway: think about long-term consequences and the tradeoffs engendered in making a decision.

Investing Lesson 3: When's A WIN A Loss?

(Pages 10 – 13)

Mental models: memory, process vs. outcome

> *"Writing is a powerful technology. Why not use it?"*
> - Don Norman in "The Design of Everyday Things" (DOET review + notes)

Of all the mundane things that can wreck a process, it turns out that plain old *memory* is one of them. Richard Thaler has a fun anecdote about hindsight bias in his wonderful book *"Misbehaving"* (M review + notes), translating the classic "principal-agent" problem into a real world counterpart: the "dumb principal" problem.

Essentially, once an outcome is known, it is a natural cognitive phenomenon to assume that the outcome was always destined - this is called "creeping determinism." But the truth is that we don't have a crystal ball, and we have to make the best decisions we can based on information available at the time. What's more, in future, we have to *evaluate* those decisions on the same playing field, rather than simply pointing to a good or bad outcome as proof of a good or bad decision. Oftentimes, it's not that simple.

To illustrate this point, let's turn to an anecdote. A friend and I were sitting in his car after a charity lunch earlier this year, talking about stocks - as we like to do. (He's not an investor by profession, but very interested in mental models and the like.) *"I see Dave and Buster's (PLAY) popped after earnings,"* said my friend. "*Great call - you were just talking about how that one seemed undervalued!"*

I shrugged. *"I have no clue why the stock's up so much. It was one quarter. The numbers weren't even that good - comps were still down 5%. Guidance is unchanged. I have no idea what people are so excited about. It's not enough data to ascertain whether or not their business is back on solid footing. As far as I'm concerned, literally nothing fundamentally has changed."*

I didn't actually own PLAY for that pop, but there have been many similar situations I could point to where I did. For example, in late February 2018, we purchased a small position in Dallas-based enterprise software company Zix (ZIXI) at a cost basis around $3.95. We had followed the company for about eighteen months, and had always liked the business and the management team.

Over that time period, the valuation had never aligned with a price we were willing to pay.[5] However, Zix began experiencing some challenges that depressed its near-term outlook. We viewed these challenges as transitory, while the market appeared to be pricing them as structural - so based on our previous diligence and our interpretation of the company's then-current results, we took a low-single digit position.

After an in-person meeting with the company's CEO to discuss the challenges - and the company's planned response - we were optimistic about the company's prospects, and wanted to build our position further to, say, mid-single digits. Alas, it seems that others saw the same opportunity that we did, and by the time we were ready to buy more, the stock had already rallied meaningfully.

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[5] One of the reasons for this: Zix held a large balance of "deferred tax assets," which, for those not an expert in obscure balance-sheet items, essentially means they were able to use long-ago losses to offset future profits for tax purposes. As such, they've been paying low to no cash taxes for a while, and will continue to do so for a while.

A while, however, is not "forever" – they have a finite amount of past losses available to offset future taxes. Unfortunately, a number of investors do very sloppy valuation work, and many who followed / purchased ZIXI took the conceptually indefensible approach of simply valuing ZIXI at a multiple to current free cash flow. The problem is that the company's current free cash flow is benefited by the finite deferred tax asset.

I'm well known for barely modeling, but even I have standards that exceed this sort of gross incompetence in financial analysis: you have to separate the (finite) value of the deferred tax asset from the (ongoing, underlying) free cash flow the company would earn on a fully-taxed basis. Under previous tax rates, ZIXI would've been a quite high tax payer without the DTA, as its business is substantially all domestic U.S. income.

The tax reform bill helped us here, as it lowered the benefit from the DTA and thus made it a less analytically relevant issue.

The rally never let up, and merely a month and a half later, we found ourselves trimming the position on price strength - and by early May, merely two months and a week or so after our initial purchase, we had completely sold out of ZIXI at a weighted average price of ~$4.89, which represented a slight premium to our fair value estimate on the stock at the time. That's good for a ~23% return on capital deployed, and a fantastic IRR (which I'm too lazy to calculate.)

But was our investment in ZIXI a success? In our minds, that's still an open question. Consider this: between our initial investment and our last sale, the company *did not release a single data point*. The company's first earnings report subsequent to our initial purchase actually occurred *after* we made our last sale. The stock's rally was not due to any "new news" - it was simply due to the market reinterpreting available information.

Admittedly, short-term financial results don't offer a ton of value (see the PLAY example above), but they at least offer *something* that short term stock prices don't: if I expect a company to have sales that are more or less flat, and they're up or down a few percent, that probably doesn't mean much. But if the company's sales are down double digits, well, that maybe means something.

Since our sale, ZIXI has reported three times, and all three reports corroborate our initial thesis - which is good. Nonetheless, it's still not proven, in our minds, that our assessment of Zix was valid. It certainly appears that there is a quite high probability that the company's challenges were merely transitory and they are on strong footing going forward… but we don't know yet.

Why am I making so much out of what amounts to a small win in the portfolio? The answer is because sometimes, a WIN's a loss.

In a recent conversation with a large endowment that was vetting Askeladden as a prospective investment, I stressed how important I think it is to focus on evaluating my results by *process*, not outcome. Very reductionistically, here is how any value investing process works at a high level:

1. The investor comes up with some view on the future of the business.
2. The investor comes up with some valuation that they deem appropriate, relative to that view on the future of the business.
3. Given enough time, if the assessment in (1) is correct, the market will come to reflect the valuation in (2), and the investor will earn a profit.

There are essentially three potential points of failure here, of which I view only one as analytically relevant for my purposes. In reverse order, starting with (3): the business could do really well but the market could perpetually fail to recognize its appropriate value. This is usually not a meaningful risk because the company can take actions (such as selling itself to a financial or strategic buyer, repurchasing stock, paying out dividends, etc) that will return value created to shareholders.

Similarly, I don't view item (2) as a risk - a failure here would essentially mean that I was too aggressive in my valuation, *given a certain set of fundamentals*. If anything, you could probably fault me for being *too conservative* in my valuation approach. There may be occasional exceptions, but by and large, I think that I tend to underwrite things much more conservatively than the average investor, and as such this isn't really a realistic avenue for process failure.

That leaves only one point where we can realistically have a bad outcome: if future results differ materially from those that we underwrite. To put it another way, as long as we - on average and in the aggregate - correctly underwrite future results, we will earn our targeted investment hurdle rate (typically 20% annually).

So it logically follows that most of our focus should be on calibrating our process of underwriting future results. If we do this correctly, we will earn superior returns over time. If we fail to do this correctly, we may be bailed out by luck once in a while - but, on average and in the aggregate, our performance will fail to live up to the standards we view as acceptable.

Going back to ZIXI, then, we got a good outcome on the stock price. I'm not complaining. But there was, at the time we sold, no rational basis on which to conclude that our underwriting was successful or not. There was no incremental data to corroborate the notion that ZIXI was on solid footing.

And, interestingly, the biggest 'mistake" I think I've made in the eleven quarters since Askeladden's inception is not one that will show up as a loss on the P&L. In fact, it shows up as a big WIN. Trade records show that Askeladden Capital Partners purchased shares of mostly-rural hardline telecom company Windstream (WIN) at a weighted average cost basis of $28.75 per share in January 2016. We sold ~60% of the position in April 2016 at ~$38.89 - a 35% gain relative to cost basis - and sold the remainder of the position in September 2016 for $41.30 per share - a ~43% gain relative to cost basis.[6]

Clearly, on an annualized basis, that's a pretty good return on capital… so what's the problem? Well, the problem is that:

A. I had no clue what I was doing,

B. My thesis turned out to be entirely wrong,

C. The profits we earned on the stock were pure luck and entirely undeserved.

If you pull up a stock chart of Windstream today, you'll see that it trades for around $4.90 - a decline of nearly 90% since we sold our last shares, and still a decline of over 80% vis-a-vis our weighted average cost basis. (Hey look, it's a real-life example of the old saw: a stock that's down 90% is a stock that was once down 80%, then fell by 50% more!)

I am not close to the Windstream situation, but it doesn't take much work to gather that all the core tenets of my thesis were wrong. Essentially, the bull thesis on Windstream - the fundamental results we underwrote - was that the company was trading very cheaply relative to its operating cash flow; much of that cash flow was being reinvested into growth projects to improve the company's network and competitive positioning, but what was left still amounted to a huge dividend, and management planned to use future cash flow to accretively repurchase shares and retire debt. Although the company's business was challenged, it wasn't imploding (on the whole - there were some good segments, some bad segments).

Summarily, it all went downhill since then: the good segments turned out to be not-very-good, the bad segments turned out to be awful, and the "growth" capex turned out to be maintenance capex. Now, of course, a lot of other things have happened - a long period of worsening financials, an acquisition, a lawsuit regarding the propriety of the CSAL/Uniti transaction, etc - and I'm not suggesting that investors should "pro forma" our results as if we'd bought Windstream and held it until the current date. Windstream could be a great buy here - or it could go bankrupt. I don't know. That's not the point.

The point is that even though Windstream shows up on our audited financials as a win, it sticks out in my mind as a major process failure. We deserved to lose money on Windstream. The only reason we didn't was

[6] Note that Windstream subsequently conducted a 1-for-5 reverse stock split, so we have multiplied the actual cost basis per share in trade records by 5 to maintain comparability.

luck. If I invested in a hundred Windstreams over the next 10 years, luck would only save us on a few of them. The rest of them would result in permanent loss of capital.

Admittedly it was never a large position, and we managed to luck out from a moment of market optimism, but: looking back, problems abounded. I knew very little about the space (see "familiarity risk" earlier), and if I'd had a wider/deeper understanding of telecom, I would likely have been more focused on the **base rate** of hardline telecom companies being businesses which dig holes in the ground, throw all their operating cash flow into them, and pray that the cash comes back out someday.[7]

Indeed, Windstream was, in some ways, the last dying gasp of my old, pre-Askeladden deep-value-ish approach: today, a business like Windstream (that was optically *extraordinarily* cheap) would never make it past even our initial quality hurdles. Even setting that aside, it had a very high degree of leverage, which as I have discussed elsewhere, can do brutal things to even what seems like very large/wide margins of safety. It was an industry we were not at all familiar with, suggesting the position should have been smaller and accumulated slowly over time as we saw more corroborating data points (see discussion of familiarity risk).

Meanwhile, other process failures don't show up on the scorecard at all. I referenced our valuation conservatism earlier; as a result of this, it's often the case that stocks we think aren't cheap enough go on to do really well - or stocks that we sell because they meet our fair value go on to have further upside.

I'm often asked about some of these, and, again, my response is very focused on *process* rather than *outcome*. For example, Korn Ferry (KFY) has posted very strong results since we sold it for a nice gain, and at one point, the stock price had more than doubled from our highest sale price thereof. That said, in reviewing the results since then, I don't really believe that they were *ex ante* predictable - put more simply, the company has vastly outperformed my expectations, along with those of everyone else in the market, but I think things could have worked out another way. Perhaps I was a little too conservative on KFY, but it's not one I lose a lot of sleep over.

Conversely, I've previously talked about my extreme regrets regarding our lack of participation in the upside at DMC Global (BOOM) - formerly known as Dynamic Materials. These were situations that *were* predictable, and my failure to successfully invest in them was simply a result of **status quo bias** - I anchored too much on my initial valuation, failing to re-underwrite the situation as business results inflected positively and data began to arrive demonstrating that their line of intrinsically-safe, factory-assembled DynaSelect and DynaStage products were gaining massive market share in the oilfield.

I avoided this mistake again, purchasing DMC Global in early 2018 at a price that was in the neighborhood of >2x what we'd sold it at previously, and ~4x what we'd paid for it initially. Was that a bitter pill to swallow? Yes - but with the correct **framing**, I was able to avoid **sunk-cost** thinking. It wasn't important what I'd bought or sold it at before. What *was* important is what I thought it was worth, and what it was trading for now. Our investment in DMC Global, albeit small in absolute size, proved very profitable in 2018.

In any event, returning to Richard Thaler's fun story about dumb principals: one of the things we've learned is that to accurately calibrate decision-making, we need an accurate record of decisions made and the information they were based on. We've previously discussed the massive benefit of our thorough research documentation approach on productivity in terms of not losing previous work and being able to quickly make appropriate investment decisions (i.e. much of the legwork is completed long *before* we choose to invest), but an additional, equally important benefit is the vast trove of data we can use to calibrate our process and train our intuition.

[7] Forgive the oversimplification. I'm sure there are other telecom companies out there that are better. But, as best I can tell from the very limited follow-up work I've done, it's not the sort of sector Askeladden is well-equipped to analyze or invest in.

Investing Lesson 4: The Width Of A Hair

(Page 14)

Mental models: nonlinearity, hyperbolic discounting

> *"When you're starting a company, it never goes at the pace you want or expect… you start, you build it, and you think everyone's going to care.*
>
> *But no one cares. Not even your friends."* - Nate Chesky, cofounder of AirBnB

That quote, from Brad Stone's "The Upstarts" (TUS review + notes), epitomizes the analytical challenges discussed by Geoffrey West in "Scale" (SCALE review + notes). We're linear approximators living in a profoundly nonlinear world. Just like the world looks flat up close but is actually round, many of the phenomena we observe are linear enough over our (very short, ADD) attention span, but are profoundly *nonlinear* over any large enough scale.

I know, I know - "compounding" is the eighth wonder of the world, there's even an apocryphal Einstein quote about it or whatever, and it's hardly new news to investors. But one of the interesting things about nonlinearity is that no matter how much you *think* you think nonlinearly, nonlinearity can always be profoundly surprising when it pops up. Part of this is because of **hyperbolic discounting** - our tendency to vastly overemphasize the importance of the present. Statistical realities about long-term consequences (atherosclerosis is bad, mmkay?) fall flat in the face of short-term desires (steak is tasty.)

Here are a few examples, in no particular order.

- I've often heard - from other investors, and even from the long-tenured IR representative of a billion-dollar company - that "bad sentiment" on an industry is here to stay, for a really long time. I've often looked up merely months later to find that sentiment gone - or completely reversed. Cue the classic saw about making traders broke by giving them tomorrow's headlines.

- "Time arbitrage" is often a function of merely thinking nonlinearly. My former PM had a term - "data point extrapolation" - that he used to describe the behavior of many analysts, buy-side and sell-side alike: take the most recent two data points, draw a line through 'em, and that's your forecast. While this works well enough much of the time, occasionally, there are situations where it is fairly obvious that there will be a big nonlinear change in results at some point over the medium term - yet the market often fails to price this, overly focusing on near-term trends.

- Sometimes, stuff doesn't matter until it's all that matters - **critical thresholds.** There's a fun story in Richard Rhodes' *"The Making of the Atomic Bomb"* (TMAB review + notes) about how merely moving your finger an unnoticeable fraction of an inch can be enough to turn an unreactive pile of uranium into a nuclear chain reaction that could level a city.

  In a ZIRP environment with easy terms, it's insane how many investors started to classify LBO-style balance sheets - i.e., 4-5x EBITDA - as "reasonable" or even "strong," suggesting that shares of these companies deserved to trade at premium P/E multiples as if the debt didn't exist. Being aware of these sorts of potential critical thresholds, when the market is blase, can help steer us away from disaster.

Many lessons are in the "still learning" rather than "fully learned" camp, and this is one of them: try as I might, I still find myself needing to think less linearly than I do.

Investing Lesson 5: All In Ain't Alright

Mental models: mindfulness, sleep, schema, dose-dependency

> *"I'm not afraid of being punched in the face… just the seconds before, 'cuz I hate that I can't see the future."*
> - Handguns, "Queens"

Continuing with the idea of nonlinearity, here's a profoundly important concept that many intelligent people - including me - take far too long to grasp. If one is good and two is better, that doesn't mean the relationship continues like that forever.

A practical example: one extra-strength Tylenol (500 mg acetaminophen / paracetamol) will help your headache, two (1000 mg) will probably make you forget you ever had one. But, under certain circumstances, as few as ten (5,000 mg) could cause irreversible liver damage and even death.[8]

This sort of "dose-dependency" relationship applies to many aspects of life. Nobody likes a granny driver -- if you're planning to go under the speed limit on the highway, don't even bother getting in the right lane. Just straight up stop driving and call an Uber. On the other hand, if you routinely go 15 - 20+ miles over the speed limit, I don't want to be on the same highway as you… let alone in the passenger seat of your car.

Yet especially in the business and finance world, there's a perverse fascination with the idea of "more is better," culminating in "grit," which is really dumb. Pointing to football players and Navy SEALs as role models is a little childish and ignores **selection bias**: yes, being totally devoted to some narrow task and sacrificing everything to achieve it works for those ends, for people who are cut out for that. But it's not how the rest of us should live our lives in different circumstances.

I've railed against the grit nonsense before and elsewhere and don't need to do it again here. It's important to try hard, of course – but it's vastly more important to utilize **structural problem solving** to put yourself in the right position to maximize your output per unit of input. Summarily, one man with a bulldozer can get more done, with poor work ethic, than one man with a shovel and the best work ethic in the world. (I've previously and elsewhere discussed the importance of **sleep**, so I won't do so here.) Moreover, one man with the *right blueprint* and the wrong attitude is going to make more progress than another man with the *wrong blueprint* and right attitude. (See Covey on the right map of Chicago.)

More interesting is the business and emotional angle: while there's often a fascination with "total alignment" - i.e. managers who invest their entire net worth in their strategy, and whose fees are solely performance based - it's a profoundly idiotic ideal, because it completely ignores dose-dependency and **n-order impacts**.

Yes. Managers should be aligned with their clients. They should care about results. But investment CAGR shouldn't define their identity. In fact, I believe it's important that it doesn't: strong emotions (both positive and negative) affect decisionmaking. Overconfidence and arrogance during good times are as dangerous as desperation (or tail-tucking) during bad times.

To avoid emotions - and, consequently, portfolio decisions - being inappropriately driven by market whims, it's important for an investor to have enough detachment to actively dampen the emotional vol rather than getting sucked into a **feedback**-driven vortex of bad decisions compounding bad ones.

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[8] https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2659888/

Single doses of more than 150 mg/kg or 7.5 g in adults have been considered potentially toxic, although the minimal dose associated with liver injury can range anywhere from 4 to 10 g.

This involves both **margin of safety** from a financial perspective (i.e., not having to worry about paying your bills if the market implodes), and from an emotional perspective. Taking these in turn:

First, performance-only fee structures are bad for client and manager alike. While they sound nice in theory, the practical reality is that correlations go to 1 in any sufficiently panicky selloff, and it really doesn't matter what you own - any market exposure, no matter how resilient and undervalued, will get clobbered on a mark-to-market basis. It's difficult to focus on making good investment decisions when you're worried about where your next mortgage payment is coming from; as such, it's not optimal from a behavioral standpoint for managers' personal cash flow to be tied to the vicissitudes of market performance.

Similarly, second, overinvestment by managers in their own fund is likely suboptimal. Clients don't, and shouldn't, have the majority of their net worth invested in any one fund - so their interests are not necessarily aligned with that of a manager who has all, or a substantial majority, of their net worth invested in the fund. This may lead to inappropriately risk-averse decisionmaking at exactly the times when the manager should be the most aggressive - for example, during deep selloffs.

Third, in sports and investing alike, "love of the game' is often lauded to the extent that it's encouraged, or expected, for this love to consume your entire life. Players are criticized for having (or having the appearance of, or merely wanting) a life more diverse than just their chosen profession 24/7.

It's quite possible, however, to love someone or something and yet not want to spend *all* of your time with it - you can probably name a couple friends and family to whom this applies. More pertinently to investing, one of the most critical attributes an investor can have is objectivity: the ability to step back and accurately assess mistakes.

And so the problem with conflating your entire identity or self-worth with your investment track record, AUM level, process, reputation, or what have you, is that to the extent some part of that investment approach or structure is broken, you need to be able to step back and identify and fix the mistake. It is very difficult - sometimes impossible - to do this if your identity is exclusively tied up in it, because then *you're* the mistake.

The phrase "can't see the forest for the trees" is literal - on a recent backpacking trip to the remote Northwoods of Minnesota, I very literally couldn't, at times, see the forest, because I was in the middle of so many trees. Trees are actually kind of boring up close: it's only when you get a ridge, a lake, a meadow, or some other kind of break/opening that you have the space to see the beauty.

To this end, at least three investment managers in their 40s and 50s have discussed with me the challenges they've faced with issues related to being "too close" to their business, ranging from burnout / reduced productivity to being too slow to catch mistakes. Being "all in" sounds good - but it's not, because it leads to **commitment bias** and **sunk-cost** thinking.

Something - anything - to avoid this is helpful. One of those managers I referenced took up drumming. One of them devotes a lot of time to exercise, and liked to take his dogs for a walk when he got home from work - *"they don't care if you had a bad day at the office. They don't care if your clients are leaving, if the market's down. They don't remember the last walk and they aren't worried about the next walk. This walk, right now, is the best thing that's ever happened to them."*

This is, it seems, one of the lessons typically learned later rather than earlier - which is why I like to surround myself with people a lot older (err, more experienced) than myself, so I can learn by proxy.

I try as best I can to craft an identity that allows me to have a little separation from the vicissitudes of the market. I am a nice person. I have 4-5 close family and friends relationships that I invest a lot in. Puppies and children think I'm cool. I'm a writer, and over the past year, I've become a legitimately good cook. And I'm (moderately) adventurous: I like going on road trips that involve backpacking and other fun outdoor activities.

Of course, one of the challenges here is the **local vs. global optimization** problem engendered by optics: what's optimal isn't always optically pleasant for clients to see. This goes back to Lesson 2 (Begin With The End In Mind) - cultivating the right sort of client base is as important as cultivating the right sort of investment approach and process.

Conclusions

The nearly 20 pages' worth of lessons presented here are far from all I've learned in 5 years of professional investing. There's much, much more. But what I've discussed represents some of the most critical - and counterintuitive - takeaways. Cumulatively, they've helped me dramatically boost the quantity and quality of my research output, as well as my ability to appropriately manage portfolio positions.

Where to next? These lessons have some logical conclusions on the evolution of Askeladden as a business and investment process. More specifics to follow in the year-end letter, which will reflect on Askeladden's first three years of existence - and our plans for the next three.

Westward on,

Samir

Appendix

DISCLAIMER: Data is estimated, unaudited, and provided for directional color only. Past performance is not a predictor of future results. We do not expect our future returns to approximate our historical returns. Amounts may differ due to rounding. Please consult your monthly statements from Fund Associates LLC or audited annual financials from Spicer Jeffries LLP for actual returns. Decimal points have been excluded so as not to convey a level of precision that these estimates are not intended to convey.

Net returns are calculated assuming a hypothetical investor paid the standard fee structure of a 1.5% annual management fee and 30% of the outperformance, if any, vs. the S&P 1000 Total Return index, which was chosen because it had, at the time of inception, historically outperformed the Russell 2000 and most accurately represents our typical investment universe of small and mid-capitalization U.S. equities (i.e., those with a market cap of $10 billion or less). We may invest outside this universe (for example, in U.S. large caps or international small caps.)

Individual investors' returns may differ from those presented here due to their date of entry into the fund or their specific fee structure (for example, accredited but non-qualified clients may not, by law, be charged a performance allocation, so they are typically charged a higher, flat management fee).

Results are presented only for Askeladden Capital Partners LP and not for any of the separately managed accounts which Askeladden Capital Management LLC (the investment advisor to Askeladden Capital Partners LP) also oversees. While separately managed accouts are generally allocated very similarly to the fund, SMA clients' performance may differ based on factors such as: timing of account opening, tax considerations, specific client instructions, and manager discretion; therefore, SMA clients should consult their Interactive Brokers statements for specific performance information for their account.

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