Dear Partners,

A challenging year ended on a positive note. In 2020, Askeladden Capital Partners LP returned a little over 4% gross, or 3% net of management fees, compared to a 13% gain for the S&P 1000 Total Return Index.

So far in 2021, we're up roughly ~16% gross, a few points ahead of our benchmark.

Although we view the absolute result in 2020 (and certainly so far in 2021) as reasonably acceptable for a year in which much of life (and, consequently, economic activity) ground to a halt worldwide, we of course view the *relative* result – which is what matters – as unacceptable. Our job is both to generate positive absolute returns (nobody wants to lose money), and to beat the benchmark (which you can, of course, invest in for free.)

Below is the performance table. We will spend the balance of the letter discussing what we think went well in 2020, what didn't, and – as we reflect on 5 years of existence as an investment firm – what we can do better to take advantage of market conditions. Uncharacteristically, we'll also spend a little time discussing the market environment that we find ourselves in, and make the case for why now is a great time for not only active investing, but active *value* investing.

	2016 - 2020 Annualized	2016 - 2020 Cumulative	2016	2017	2018	2019	2020
ACP Gross	~ + 25.2%	~ + 208.3%	+ 69.5%	– 1.8%	+ 21.7%	+ 45.5%	~ +4.6%
ACP Seminet (Gross Less Management Fee)	**~ + 23.2%**	**~ + 184.4%**	**+ 67.0%**	**– 3.5%**	**+ 19.9%**	**+ 42.9%**	**~ + 3.0%**
S&P 1000 Total Return	**~ + 13.9%**	**~ + 92.0%**	**+ 31.2%**	**+ 15.3%**	**– 10.3%**	**+ 25.2 %**	**+ 13.0%**
ACP Seminet +/– S&P 1000 TR	~ + 9.3%	~ + 92.4%	+ 35.8%	– 18.8%	+ 30.2%	+ 17.7%	~ – 10.0%
Outperformance Allocation = 30% of performance > the higher of {benchmark, zero}	~ – 2.6%	~ – 28.4%	– 10.7%	0.0%	– 6%	– 5.3%	0.00%
ACP, Net of All Fees	**~ + 20.7%**	**~ + 156.0%**	**+ 56.3%**	**-3.5%**	**+ 13.9%**	**+ 37.6%**	**~ + 3.0%**
ACP Net +/- To S&P 1000 TR	**~ + 6.7%**	**~ + 64.0%**	**+ 25.1%**	**-18.8%**	**+ 24.2%**	**+ 12.4%**	**~ – 10.0%**

DISCLAIMER: 2020 returns are unaudited. Previously-reported figures for 2019 were based on unaudited figures from Fund Associates; the subsequent audit by Spicer Jeffries LLP reported slightly lower returns, due to exclusion of the GP account from their calculations, and we have restated 2019 results with the lower of the two figures. 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. Cumulative/annualized multi-year results may differ from the product of individual year results given carry-forward provisions in our three-year performance fee structure.

Please consult your monthly statements from Fund Associates LLC or audited annual financials from Spicer Jeffries LLP for actual returns. Cumulative net returns are calculated assuming a hypothetical investor joined on the date of inception (2016-01-08) and paid the standard fee structure. Individual-year net returns are calculated assuming a hypothetical investor joined at the beginning of the single year, and redeemed at the end of the same year. Data is presented only for Askeladden Capital Partners LP and not for any of the separately managed accounts which Askeladden Capital Management LLC also oversees. Askeladden Capital Management LLC is no longer accepting new clients at this time, and performance figures are thus provided for general interest only, and not for any marketing purpose.

The Fine Line Between Principled and Stubborn

Words have a funny way of applying context, or bias, to the same set of objective facts. Take someone who, under pressure from outside forces, resists those forces and remains on their current path.

If those outside forces are perceived as negative, and the current path is perceived as positive, then the person is "principled" – maybe even "steadfast." Think about a hero cop in a TV show, refusing to stick his hand in the pot of drug money even as his department goes to shambles.

But if the outside forces are perceived as positive, and the current path as negative? Then the person is "stubborn" – maybe even "bull-headed." Think about the classic movie old-timer who refuses to get with the times and adapt to modern technology.

We think the key to a good investing process is to remain *principled*, without being *stubborn*. I'll concretize this a bit.

Our fundamental principle is the basic tenet of Finance 101: a business is worth the sum of its future cash flows, discounted back to the present. Reasonable people can disagree on the inputs: the likelihood of revenue growth or margin expansion, the correct discount rate, etc. But *in the absence of a market in which to sell your ownership stake*, most rational people would only buy a business for the sake of the future cash flow it produces.[i]

In this principle, we are unwavering; we will not buy an asset – e.g., GameStop – at a much higher price than we believe it to be worth, simply because we believe that some "greater fool" will purchase it at *an even higher price* in the future. We don't believe this approach can underlie a rational investment process, because it is speculative rather than reliable: at the end of the day, you're making an investment on the basis of hope, not math.

When we invest on the basis of expected cash flows, there is of course the chance that we are *totally wrong* – but if our judgment is directionally right and the company performs reasonably well, then over a long enough time horizon, those cash flows will predictably accrue to us one way or another. This could be through dividends/buybacks, improved market price, or a sale of the company to a buyer who values the cash flows – one benefit of a developed financial market, particularly in a low-return world, is that undervalued streams of cash flows generally don't remain undervalued forever.

It's Just A Matter of Time

I said that we wanted to be principled, but not stubborn. And I think a good definition of stubbornness is the apocryphal Einstein quote about insanity. Given a broad enough sample, when things work, you should generally do more of them; when things don't work, you should generally do less of them. If you keep getting frustrated because your Jello won't stay nailed to a tree, maybe you should *stop trying to nail Jello to a tree.*

One of the things that didn't work well for me early in my investing career – mostly pre-Askeladden – was buying crappy companies instead of good ones (i.e., "classic" asset rather than cash flow focused value investing). So I stopped doing that, and started buying better businesses that generated consistent cash flows and grew nicely, and – lo and behold – my ability to forecast companies' future results suddenly improved dramatically, and with it my portfolio performance.

Reductionistically, I think the two factors that most determine *short-term* results in the market are investment judgment and timing. As for the first, I actually think we've generally done remarkably well, in both 2020 and over the previous four years. Of course, there have been some instances where we were totally wrong about the business, and lost money as a result – most recently and notably Lydall (LDL).

I don't want to sound as if I don't think we can and should improve our investment judgment – of course we should – but mistakes happen to every investor, including Warren Buffett. Even Brady throws interceptions sometimes. We're not Brady or Buffett, but by and large, I don't think that poor investment judgment (i.e. fundamental business analysis and valuation) has been our biggest problem, whether in 2020 or otherwise. Of course, going into the year, we had no idea that COVID would happen, and therefore many beginning-of-year estimates had to be redone to factor in a new and previously unfathomable macro environment.

Since then, however, companies in our portfolio have generally met or *outperformed* the fundamental estimates we made during the year. We discuss company fundamentals in more detail in the private portfolio commentary, but here are a few brief examples:

- Franklin Covey (FC)'s SaaS business has delivered really great results, with new contracts being signed even by highly-affected companies like hotel chains and airlines, and meaningful double-digit growth in both total U.S. Enterprise subscription revenues and invoices, with more-affected ancillary parts of the business (like training, education, and international) already rapidly exceeding or approaching pre-COVID revenue levels.

- MiX Telematics (MIXT)'s SaaS business similarly held up very well, far better than management initially expected, even though many of their customers (car rental fleets, oil and gas firms, bus and coach) have been operating through a truly horrible macro environment.

- Omnichannel brand Duluth Trading (DLTH)'s products turned out to be exactly what the consumer ordered (via e-commerce) during COVID. The company is actually exiting COVID in a stronger position, with more customers, faster growth, and better margins (which you can't say for many apparel companies.)

- AerCap (AER), which Mr. Market at one point thought might not be able to access the capital markets and run into serious liquidity issues, recently issued 5-year unsecured debt below 2%. Conversely, some leasing peers, as well as customer airlines, don't have access to affordable financing. So AerCap's market positioning seems likely to be far stronger post-COVID than it was pre-COVID, with fewer competitors and needier customers. And while more book value impairments might perhaps arrive in the future, so far, cash collection and impairments have vastly outperformed our internal expectations.

- Despite the dreadful headlines about small businesses, small business lender Marlin Business Services (MRLN), which we began to purchase in late November at a substantial discount to tangible book, reported a massive fourth-quarter CECL reserve release that we predicted would happen at some point (albeit not that soon). Credit trends are now approaching pre-COVID levels. (The struggling small businesses you hear about in the news only represent a small slice of Marlin's customer base – many are doing quite well.) Mr. Market's view that the business was permanently impaired was – and is – still vastly too pessimistic.

Over a longer time period, our forecasting track record generally seems to mirror 2020. The fundamental judgments we made about companies' competitive and market positioning, and growth potential, have generally ended up being realized, over some long-enough period of time.

Even Liquidity Services (LQDT) – one of our early ideas that we ended up abandoning because expected results were taking too long to materialize – has, actually, played out spectacularly this year, unfortunately at no profit to us, given that we didn't own it. COVID seems to have accelerated the shift from offline to online transactions in their end-markets, and their unified platform is operating more efficiently, allowing them to generate solid EBITDA margins and free cash flow while rapidly growing revenues.

Again, have we made mistakes in our fundamental judgments in 2020? Yes – I discussed some of these in our previous letters (i.e., being too early to buy some recovery plays, and not being imaginative enough about the depth and length of the crisis.) These undoubtedly detracted from our results. But broadly, we bought good businesses at great prices, and these businesses generally performed in line with or higher than the expectations we underwrote when buying them.

Small Winners

So what haven't we done well, either in 2020 or before? Timing. We have generally, *given enough time*, been right on positions and earned a strong IRR.

But in many cases, we've been wrong on timing in the interim. Often very wrong. In 2020, we've watched as many of our high-conviction positions have recovered less than some of their peers, notwithstanding the strength of their fundamentals. This happened to us before in previous years with some of our large positions; for example, we built a large position in Franklin Covey that dragged us down over several periods, but ended up being monumentally profitable over different ones. Franklin Covey has actually generally *outperformed* our internal expectations, but that hasn't always translated cleanly (period by period) to outperformance of the stock.

I've long stated a goal of having 10-15 portfolio positions, but – particularly if you exclude immaterial "bookmark" positions – we've generally run with far fewer positions than that. And that's a shame, because we've frequently found small positions turn into big winners. For example:

- We entered 2020 with a modest, mid-single-digit position in Israeli-based Magic Software (MGIC) – a provider of software and consulting solutions in the digital transformation arena – and this ended up being one of the best-performing stocks in our portfolio over the first half of the year. (It will not surprise you that we sold too early.)

- In 2020, we bought a low-single-digit position in Liberated Syndication (LSYN) – a growing podcast hosting platform with robust free cash flow – that nobody seemed to care for the better part of the year, until it suddenly became a sexy hidden-gem growth story that is apparently the darling of FinTwit. (I guess that makes us hipsters; we liked them when they were obscure?)

- Similarly, in 2020, we took a modest position in Korn Ferry (KFY), which we've followed for a long time and know well. The stock has since doubled from the prices we were buying it at in May. Of course – we sold it too soon, to buy other things that have remained stuck in the mud, so didn't really benefit too much from our good call on the buy side.

- In December, we initiated a small position in specialty equipment rental / service company Concrete Pumping Holdings (BBCP – also known by its trade name Brundage-Bone) – which proceeded to rise by over 50%, as investor sentiment around construction prospects in 2021 was boosted by more confidence in the end of COVID, and the infrastructure-bill ramifications of the unexpected blue sweep of the Senate in the Georgia run-off.

The Road Less Painful

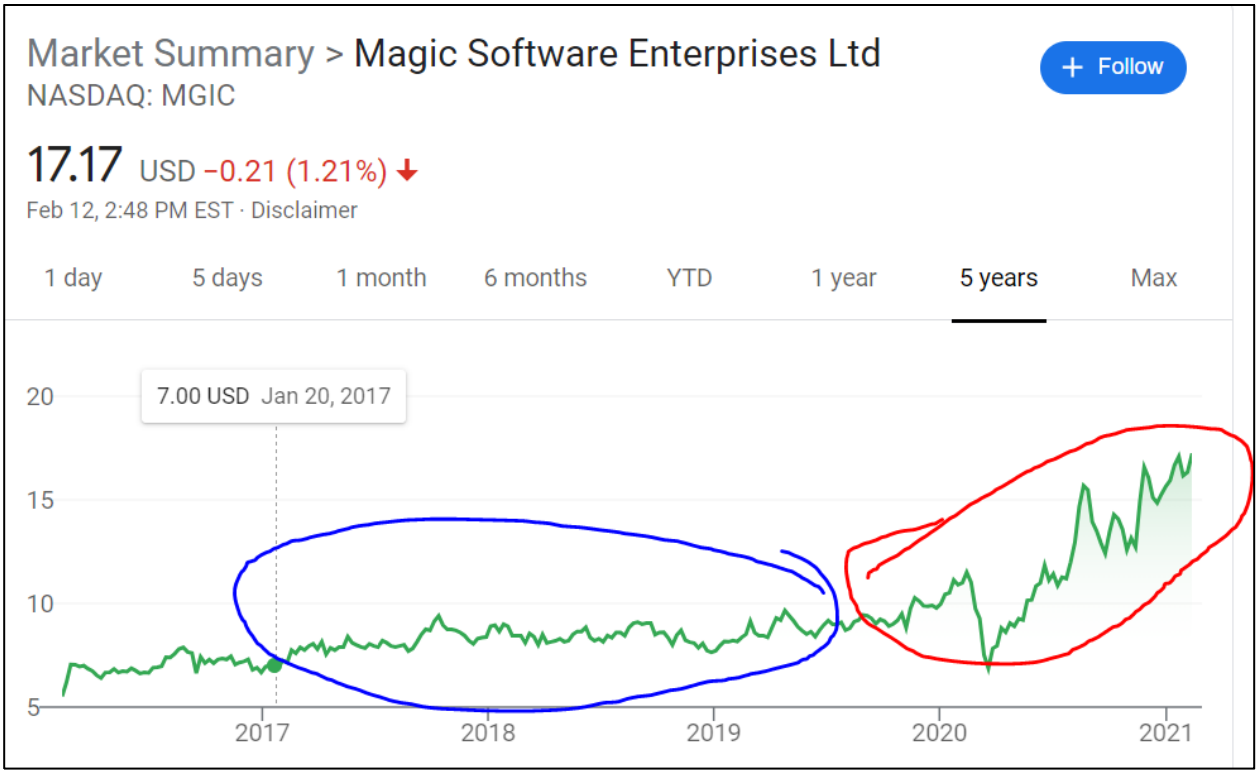
Some value investors take the tack of acting like private-equity investors, or permanent capital – *we have a long holding period, so we don't care about year to year performance.* I've always viewed things a little differently: one of the realities (for better or worse) about the public markets vs. the private markets is nearly-instant liquidity. I have the opportunity to take advantage of the market on any given day of the week – so why shouldn't I, if I can do so profitably? I recognize that I'm not *going to* outperform in every single year, let alone quarter, and that one good year (or bad one) doesn't necessarily matter.

But why in the world would I *want* to go through multiple years of underperformance if I think I could do better?

We have, fairly consistently, had small positions turn into big winners in a reasonably short period of time, while also having large positions (that should be the best performers) languish, which suggests that our overly-concentrated approach has flaws. What I'm saying here isn't new; you'll see it's relatively consistent over multiple years of letter-writing.

It's not ex-ante predictable when or why stocks will "catch the magic" – in fact, Magic Software (MGIC) is a great example of this. The company reported strong results for years that nobody really seemed to care about, leaving it languishing at a low double digit earnings multiple despite very strong growth (which is where we bought it).

And then all of a sudden, it got really hot towards the end of 2019 and 2020, finally – after years – rewarding shareholders who had patiently waited for the company's attractive market positioning and strong organic growth to translate into a reasonable, fundamentally-defensible multiple. (We don't have a view on its valuation today; but we do believe that the prices it traded at from 2017 – 2019 were far too low relative to its intrinsic value.)



So was MGIC a good investment, or not? It worked out very nicely for us, even though we sold too early and didn't manage to profit from much of the recent run. But MGIC actually didn't outperform much in 2019, and it *certainly* would have been painful to own for much of 2017 and 2018 despite very strong fundamental results.

We think there's something to learn here. We firmly believe that the market will recognize good fundamentals given enough time. Someone who invested in MGIC in mid- 2017 would have been right on the fundamentals – driven by increased demand in the digital transformation market, the company consistently grew revenues with strong EBITDA / cash flow margins. And yet they would've been flat until early 2019. It wasn't until 2020 that the correct fundamental analysis would have been reasonably / properly rewarded.

What should one do when they know that A) fundamental analysis will pay off, but B) over an uncertain timeline, if you make the assumption that C) timing the market's enthusiasm or consistently identifying stock-specific "catalysts" is difficult or impossible to do?

We think the correct answer is: have more shots on goal. We know that every company that fits our process will, if we are correct, pay off *at some point*; we just don't know when – so having more companies means we have a higher likelihood of some of them paying off *soon*. This is beneficial, because it allows us to reinvest in the others that still trade at attractive valuations, rather than simply sitting on your hands and waiting for the market to care.

We made the decision during the depths of COVID (spring – early summer 2020) to concentrate our capital deployment in our highest-conviction positions. We had reasons for this, including that we had a lot more confidence in how these companies would perform given our intimate knowledge of them.

At least sitting here in early 2021 with fairly clear line-of-sight, in both the market and the world, to a post-COVID era – that doesn't seem to have been the correct decision. Our portfolio probably would've done better had we had a few additional shots on goal – this would have, likely, improved our short-term performance. To be clear, we do still expect to make up the underperformance (and then some) over time – there are no guarantees, but we feel very good about what we own. Nonetheless, a less concentrated portfolio with a few more small winners would have allowed us to subsequently redeploy capital into those same high-conviction names at a later point, when the forward IRR is likely better (due to having skipped all that underperformance).

The Sweet Spot of Concentration

No more bashing our heads against the wall. I think that the worst part of my process is that I've been overly focused on a single number – my expected IRR calculation – to drive portfolio decisions, which has led to us consistently concentrating in whichever names are at the top of our watchlist, at the expense of holding small positions in other names. Returns over periods of time *shorter* than three years – as well as longer – would probably would be improved, over most periods, by a little more portfolio diversity.

Think of this as an incremental rather than revolutionary change to our approach. If anything, we're not changing our strategy per se; we're just trying to do a better job of *sticking to the strategy we originally espoused* (but for various reasons, haven't managed to keep to, exactly.) We've always *wanted* to run a 10-15 stock portfolio, in theory – we just haven't done it.

We're not abandoning concentrated investing and running a 50-stock portfolio – setting aside our philosophical and theoretical preference for concentrated investing, we don't have the resources to keep up with a 50-stock portfolio, at least not with our intensive research process.

Rather, we're simply trying to step away from the *uber-concentrated* Munger-esque approach – we hope, over time, to not have the portfolio dominated by 5-6 very large positions ranging from 10-20%+, but to rather have more representation among the 300-600 bps type positions, in addition to some positions in the high single to low double digits (with the largest positions perhaps being in the low to mid-teens).

If these smaller positions work out well fundamentally, but are dead money in the stock market, it's less problematic, because *we can always buy more as the chance of a really great outcome increases over time.* In other words – **we think this approach is the best intersection of a principled, fundamental-value approach, with what gives us the best chance to benefit from the unpredictable pessimism and euphoria of the market** (which we will discuss in the next section, for anyone interested)**.**

This gives us two ways to win: heads, we have a position (even if small) that generates great returns over a short time period, which we can subsequently reinvest. Tails, we have the opportunity to buy more of a position with now-higher expected returns, without having given up a lot on the opportunity cost side.

Overly-large positions tie our hands; if the stock languishes, we really can't do anything – we can't buy more, and we certainly shouldn't sell it at the point of maximum pessimism. Huge positions work really well when they work (and they will again for us), but they seem to cause a lot of unnecessary heartache in the interim. A little less concentration eases this heartburn, while still allowing us to win big if we're right.

Kathmandu (KMD) is a nice example of what we're aiming for. It started as a small position in the summer of 2018, and actually went backwards for a while. But the position was small enough that this didn't really matter – if a small position goes down 10-20%, it's not really going to move the needle on your overall returns (like it would for a 10 – 15% position).

We steadily accumulated shares over the next year as they languished, having a conviction-sized position by the summer of 2019 near the lows, and ended up having a great outcome on this over the next six months into early 2020, monetizing most of the investment for a big profit. COVID makes things difficult to evaluate after that. [ii]

Of course, as a principled value investor, I'm not going to go out and buy more stocks overnight for the sake of buying more stocks – at the extreme, I'm not going to buy something at what I view as fair value (let alone above fair value) just for the sake of having more positions.

Similarly, I of course won't ignore fundamental value and just liquidate our "lagging" holdings overnight just as they're most likely to outperform over future periods. Think of this as a more gradual, incremental journey over the next 24 months, as we monetize existing positions and build new ones.

Lead Measures – The Watchlist

Thinking about fundamental drivers, the obvious corollary is that if I want a somewhat more diversified portfolio, *I need more investment candidates, and our watchlist needs to be bigger.* This will sound obvious when written, but: our portfolio isn't comprised of the best risk-rewards in the market. Rather, it's comprised of *the best risk-rewards in the market that we actually see, and are able to analyze.*

Some types of companies will always be outside our analytical capabilities or our ethics, which is fine. No biotechs or breastaurants for us. But there are thousands of publicly-traded small cap stocks globally, and only about 150 on our watchlist – it's laughable to think that we have the market cornered on all the best ideas. There are tons more out there; we just have to find them.

The process I initially imagined for the watchlist needed some updating over the last several years. It became clear over time that the combination of our high "candidate acquisition cost" (detailed, lengthy research process) and our high "involuntary churn" (lots of M&A, etc in the small-cap space) meant that for a while, we were really just treading water to keep our watchlist stable, after rapid growth in the initial years.

After identifying this problem in 2019 (I've talked about it before), COVID got in the way through much of 2020 – but I think we've now got it figured out. In the private portfolio commentary, we discuss three new (smaller, 200 – 600 bps type) positions that we've taken since November 2020, all as a result of new watchlist additions – companies we'd never seriously worked on before November 2020. (We've mentioned Marlin and Brundage-Bone in this letter; the third I'll keep confidential for now, as we're still building the position and would prefer people not discovering it over the next several quarters.)

I believe that a number of process improvements – such as a streamlined research note-taking documentation process (to reduce time invested in each new name), outsourcing of less value-added research and business functions, and subscription to various services (including an expert network, which 2016 me would've scoffed at) – will help us cover more ground in less time. Again, we're not radically reimagining things – just trying to have a Danaher Business System type culture of continuous improvement.

Three new investments in 90 days isn't repeatable, and I also still believe in the value of mostly investing in companies that we've followed for a while as opposed to just found yesterday. Nonetheless, I am optimistic about 2021 seeing significantly higher research productivity than 2019 or 2020. Of course, this probably won't have a material impact on 2021 performance, but it will hopefully prepare us well for 2022 and beyond.

Mr. Market

You know that I don't spend much time talking about the market, because I tend to focus internally on what *we* can do better, not what the market is or is not doing. I generally think it's a waste of time to worry too much about "the market" – I'm not in the business of buying or selling the stock market; I'm in the business of individual security selection. And I frequently explain this to friends or acquaintances who ask me what I think about whatever the Dow did this week. I generally don't know, and I generally don't care.

But given what transpired in 2020, I'll spend a little time discussing what we've seen during a year that capped off a fairly long period of historical stock-market performance and multiple expansion. Unlike some, we don't really have any top-down views on the market – but the market is comprised of individual stocks, many of which we follow, and we do have a lot of bottom-up views on those, so that is the perspective we are speaking from.

This is not forecasting: I don't have any particular views on what the market will or won't do over the next month, year, or three years – but I do want to make some observations that hopefully help contextualize our approach.

Feel free to skip this section, and please yell at me as rudely as possible if I slip into the bad cranky-old-value-investor habit of waxing poetic about the market in every letter hereafter.

From The Lowest Lows to the Highest Highs

In the classic Mr. Market analogy, a rather excitable fellow stands on a street corner, shouting out prices at which he wishes to buy or sell various securities. Some days, Mr. Market is very pessimistic – in March 2020, he was willing to sell you strongly-capitalized companies with robust cash flows for dimes on the dollar, even if COVID stood to minimally impact – or perhaps even benefit – their business. (Even most of the FAANG stocks sold off in March 2020, despite COVID being perhaps the greatest macro windfall for their business in history.)

Other days, Mr. Market is positively euphoric. As we've discussed in previous letters, a few months later in the summer of 2020, some companies – even some in industries heavily affected by COVID – were trading at all-time highs, as if their business prospects had somehow dramatically improved as a result of macroeconomic circumstances that were putting some of their customers out of business.

And then there's the… well, we'll call it "excitement" about anything that looks vaguely like Cathie Wood might buy it. As of today, the largest company in the Russell 2000 index is Plug Power (PLUG) – a company involved in fuel-cell technologies. Plug sports a market cap of ~$32 billion dollars, up about 33x over the sub-$1 billion market cap that it had at the beginning of 2020.

Revenue, meanwhile, apparently grew a relatively pedestrian 42 *percent* in 2020, with roughly similar growth expected for 2021, which is actually not that different than the 32% apparently recorded in 2019, and below the 74% recorded in 2018 (just working off my screen here – I don't know PLUG from Adam, or LM Acquisition Opportunities). In other words, maybe 6% of Plug Power's 2020-2021 gains in the stock market can be attributed to revenue growth, and the remaining 94% can be attributed to a rise in the price-to-sales multiple (technically they raised some capital at elevated valuations, but just work with me here).

Perhaps this price tag is the steal of the century, and the company will go on to dominate a new era of clean energy – but as it stands, the company has a history of reporting marginal or even *negative* gross profits, and substantial negative operating and free cash flow. For roughly the same market capitalization, an investor could purchase established leading companies that consistently generate billions of dollars in free cash flow, such as Hilton, AutoZone, Discover, or Yum! Brands (the owner of ubiquitous restaurants like KFC, Pizza Hut, and Taco Bell).

For Plug Power to merely justify its current valuation – let alone outperform from here – a whole lot of things have to go right, and then some. Unlike, say, Tesla, Plug Power is not exactly head and shoulders above the rest of the competition from a branding and revenue perspective. This isn't Amazon or Facebook. There are many other players in the fuel-cell market (or, at least, claiming to be in that market) who are also valued at billions of dollars – this Barron's article has a nice overview of several, and there are more out there (FuelCell Energy, up *only* 16x since the start of 2020 to *only* a market cap of $8 billion, is also one of the top components of the Russell 2000.)

Even if the world goes fuel-cell, Plug is, of course, going to have to compete against probably dozens of others well-capitalized entrants, to secure its share of the goldrush (if the gold actually exists). Keep in mind that while wind and solar energy have actually been relatively successful in growing their share of the energy pie over time, many of the companies in that sector have turned out to be mediocre investments at best.

For example, Wikipedia states that the installed solar power capacity in the U.S. went up 13x since 2010, or ~30% per year – quite strong growth that would make most industries other than SaaS turn green with envy. And yet, even *with* the clean-energy craze of the past 12 months, the Invesco Solar ETF (TAN) really doesn't look like something you would've wanted in your portfolio over the past decade, despite the massive bull market in equities over the same time period. Even after the huge run over the past however many months, someone who bought this ETF in early 2010 would barely have kept up with inflation, and certainly not the S&P 500:

Today, a lot of market activity seems reminiscent of previous market crazes such as the internet bubble of the late 90s – any company simply announcing it got into a hot space could IPO at billions of dollars with no actual existing product. Take Nikola, which was briefly worth over $25 billion – and is still worth $8 billion! – despite, to the best of my knowledge, *never having built a single electric truck or generated a single dollar of revenue.*

Undoubtedly there are real companies with real technologies in the electric vehicle and clean energy market, that will generate real profits over time, but it seems like investors are making the flawed assumption that *every* company in that market will turn into a mega-star.

So these are the extremes of Mr. Market – on one day, believing that solid, profitable businesses are too risky at single-digit multiples of cash flow; on another day, believing that every single vaguely hydrogen fuel-cell associated company, or electric vehicle company, or whatever recently SPACked, is worth billions (or tens).

For fun, consider that I bought Microsoft years ago, at a price starting with $2, as a newbie value investor. In the early 2010s, Microsoft traded at a very low, single digit multiple to free cash flow. Why? Mr. Market thought it was a passe business with no opportunity beyond legacy Windows + Office licenses – despite its strong position in servers / the cloud, areas which seemed likely to grow sufficiently to offset any weakness in legacy desktop systems.

At the time, the shareholders of Microsoft were all stodgy value funds, not high-growth investors. Fast forward to today, and on the back of the company's evolving subscription business and Azure cloud business, the stock price still starts with $2… but has an extra digit. (Don't worry, like a good value investor, I exercised my paper hands and sold the stock for a small profit, missing out on pretty much all of the run.)

The Death of Value Investing

The benefit of active investing over passive investing, as we see it, is that active investors have the opportunity to take advantage of Mr. Market's vacillating emotions. You can buy from him when he is overly pessimistic, and sell to him when he is overly euphoric. If you're brave enough, you can even borrow from him when he is overly euphoric, and sell back what you've borrowed when he turns more pessimistic. This is the fundamental premise of value investing – trying to identify a gap between what an asset is worth, and the price for which you can buy (or sell) it from its current owner.

Much (digital) ink has been spilled over the death of value investing, but in our view, all rational investing should, in some senses, be value investing. Even much of "growth" investing is, in a certain frame, value investing: notwithstanding that a SaaS company may be trading at 10x sales, perhaps an investor thinks consensus analysis actually *underestimates* the company's future margin and growth potential, and thus should instead trade at 15x.

Whether an investor's judgment is correct or not is, of course, the big question here – but philosophically, the only scenario in which value investing can "die" is the scenario in which human judgment becomes perfect, and humans (as collectively represented by the market) no longer suffer periods of over-optimism or over-pessimism, leaving no room for anyone to have a more informed analysis than anyone else.

Some criticisms of value investors are, I think, valid – certainly, it seems like some in previous eras may have been overly reliant on quantitative shortcuts (such as price-to-book or price-to-earnings ratios) that have been arbitraged away by supercomputers able to calculate these things much faster than a human ever could.

But others seem overblown – far from value investors being stuck in the mud still trying to buy Graham net-nets, most of the "value investors" I know are actually quite familiar with the value of growth, the power of recurring revenue, and so on. In fact, looking at some of my friends' portfolios, large holdings tend to be related to themes like SaaS / subscription businesses, online gambling, video games, and so on – generally, capital-light, high-margin, high-growth businesses. These are hardly your grandfather's low-return, capital-intensive "value portfolios" of heavy manufacturing concerns trading below asset value.

It doesn't take much modeling to realize that a business that grows at merely mid-single digits is probably worth double that of a business that doesn't grow at all. Our own dyed-in-the-wool value portfolio has a 40% allocation to SaaS names, and a further ~30% allocation to companies which we expect to grow revenues at a high single to double digit rate over a 3-5 year period. We've recently been building a position in a company with a subsidiary that can legitimately be classified as a 20%+ growth digital disruptor, that nonetheless trades at a valuation that can easily be tied back to reasonable medium-term cash flow projections.

Nonetheless, our portfolio certainly wasn't your best option over the past year. So, should investors just buy indexes and/or "hot" companies with "great narratives" at any price? This is the strategy that largely would have worked best in the recent past.

To be clear, I'm not talking about real, underlying value creation – in some cases, the companies at the top of the leaderboards have indeed grown rapidly, with attractive unit economics, so they are undoubtedly worth more today than they were at this time last year, and three years ago, and so on. Value investors were clearly wrong to continually harp on the then-minimal reported profits of businesses like Amazon and Facebook, as that was not the right analytical framework.

But it is also clear that a substantial portion of recent stock market performance has been due to investors being willing to pay a much higher multiple on the same underlying unit of revenue, cash flow, or so on, and ignore very real costs such as stock-based compensation.

Clearly, in the case of PLUG, it's a bit difficult to quantitatively argue – with evidence – that the future for the company's fuel-cell technology is 33x better today than it was at this time last year. PLUG could certainly be worth its current price tag, but if it is, then Mr. Market was *horribly* wrong in January 2020, and PLUG (and its peers) were then one of the greatest investment opportunities of all time.

I don't mean to harp on PLUG; it is simply the extreme end of a trend we've seen across our watchlist. (PLUG isn't on our watchlist; we were just curious about it because it was the largest R2K component.) Many companies have seen their multiples rise endlessly, despite business results reasonably consistent with previous periods.

The Breadth of Multiple Expansion

Take Kadant (KAI), which we've followed since 2014 or 2015. It's not exactly in a sexy space – it makes bulky capital equipment, and higher-margin consumables, for industries such as recycling, aggregates, and paper/cardboard production. We're not aware of the company announcing any new ventures getting into marijuana, space exploration, or anything else like that. It's just a boring industrial company.

Per analyst consensus on our screen, Kadant's revenues and EBITDA in 2021 are expected to be a mere 6-7% above 2018 levels, and *below* 2019 levels. Despite this flattish top and bottom line, and no dramatic structural changes to the company's business model or industry that we can identify, the company's valuation has expanded from a defensible 9/11x EBITDA at the end of 2018/2019 to over 15.5x today.

A similar story plays out at micro-cap industrial calibration company TransCat (TRNS). We've always liked the company's strategy of moving more to recurring revenue, but the business model hasn't changed in some revolutionary fashion – nor has growth been spectacular. FY2022 EBITDA is expected to be $22 million or so, up 8% annualized from 2018, although some of this growth has been contributed by acquisitions, and some of it has been driven by a more capital-intensive rental business that they've been growing (they spend a lot more on capex now than they used to). The company's results are fine, but they don't seem sufficient to justify the company's LTM EBITDA multiple jumping from 8-10x in March 2018-2019, to an NTM EBITDA multiple of close to 16x today.

And then there's the "if-it-squeaks" panacea WD-40 (WDFC), a company I'm genetically predisposed to love (both my parents are engineers, and my dad's always been a big DIY guy.) I had the privilege of meeting CEO Garry Ridge long ago, and love his empowering management style. The company's results in the past few years have been solid, but it's not as if this is an emerging-technology growth story that will take over the world: it's WD-40, for heaven's sake. There's already a can of it in pretty much every garage or attic in America.

Recent results benefited strongly from a boom in renovation and fix-it-up behavior as people were locked at home – but conceivably, this bump in revenues, like that seen at grocery stores, will dissipate over the coming years as people return to normal lives. Nonetheless, WD-40's EV/EBITDA has skyrocketed from already extremely-rich levels of ~30x in the previous three years, to ~40x and ~37x expected EBITDA for FY2021-2022, or – if my data source is right – a 1.4 and 1.8% free cash flow yield. However good a brand and company it may be, it is difficult to see how WD-40 could justify such valuations on a cash flow basis.

These are merely a few examples; I could go on and on (and on). The particularly puzzling element, as an analyst / portfolio manager, is that there is no one common factor to which companies have seen this sort of massive rerating in 2020 – it's not just a matter of being in technology or a "disruptive" industry. It's not the matter of being a big, liquid large-cap, or a small, illiquid small-cap. It's not eve, for that matter, related to having done well during COVID. Kadant is expecting annual revenue to be down 11-14% in 2020, and yet its stock is up over 40% since 1/1/2020 – go figure.

Obviously, if we thought we could take advantage of this market behavior in a fundamentally-tethered way, we would. But so far, it's eluded us. Bear in mind that we *do* own plenty of growthy businesses with margin expansion; we *do* own SaaS; we *do* own beloved and growing brands – ours just haven't garnered the multiples they deserve intrinsically, let alone the stratospheric multiples that peers trade at.

For each of the very real, very solid companies on our watchlist trading at an all-time high, we could point to a company in our portfolio with a similar profile, whose multiples have not seen this sort of stark rerating. In many cases, multiples are meaningfully below where they were at the start of 2020.

In other words, if you take the sort of discount-rate or macroeconomic assumptions required to justify the multiples of the high-flyers that seem quite common on our watchlist – and apply them to our portfolio – then our portfolio should probably be trading 50% - 200% higher, depending on the specific company. Try as we might, we can't identify company-specific factors that even come close to justifying the difference.

So does fundamental value not matter anymore? Is it all about narratives, or investor constituencies? (ARK aside, we've noticed that even in the small-cap space, having one or two "influencer" shareholders tends to lead to rerating totally disproportionate to business performance."

One could point to the past – in the end, fundamentals perhaps matter most when nobody believes they matter, whether it comes to the creditworthiness of mortgage borrowers or the business models of internet companies. But theoretically, I think it's also demonstrable that while past multiple expansion has begotten more future multiple expansion, this is not a trend that can continue forever.

Trees Don't Grow To The Sky

PLUG, as an extreme *reductio ad absurdum* case, is helpful. It seems extremely unlikely that PLUG can go up by another factor of 33x in 2021. That would put it at a market cap of ~$1 trillion, rarefied territory occupied by only a few of the world's largest, most leading companies – PLUG isn't Amazon or Facebook (at least, not yet). Certainly, if that were to happen, PLUG couldn't go up by 33x *again* - US GDP is $21 trillion, and I'm not sure there's a precedent for any company trading at or above the level of GDP.

In a less extreme sense, while you can point to valid, tangible reasons that multiples have increased for many companies over the past year – COVID separating the world into haves and have-nots, lower interest rates – these fundamental drivers must also end at some point. We are not going to have *another* pandemic that further accelerates the adoption of technology in 2021-2022, and although we could perhaps have negative interest rates, we could of course also see much higher interest rates.

Trees do not grow to the sky; at some point, physics – or math – impose fundamental constraints on growth. Anyone who's run a DCF model knows that modeling indefinite above-GDP growth means that your specific company (or project) eventually grows larger than world economic output; similarly, multiples can only double so many times before they reach a territory that is unjustifiable with any sort of rational economic analysis, howsoever bright-eyed. There is also simply not enough incremental money, at some point, that will fuel further multiple expansion of a stock – it gets more expensive as the stock gets bigger.

As we recently saw with GameStop (GME), gravity eventually takes hold. Although that specific situation played out over months and then days, and involved a gamma squeeze and "diamond hands" and all sorts of other things that I don't really understand, it's not really any fundamentally different than what we were just talking about: a group of investors purchased shares at higher and higher prices, mostly because the stock had been going to higher and higher prices, and they expected to be able to sell it to someone else at higher and higher prices. At some point GME ran out of incremental buyers willing to pay an even higher price, and the trend reversed.

Of course, as anyone knows, "the market can stay irrational longer than you can remain solvent." GameStop's stock reached objectively absurd valuations, then doubled again, and again, and again, before finally starting to fall down to earth. There's no reason that hyped companies buoying market valuations could not continue on their current path of multiple expansion for months – or years.

Nonetheless, from our perspective, the longer it goes on, the less likely it is to continue, and the easier it is for investors to take advantage of Mr. Market – earning higher returns over the long term. Just as the best time to be greedy is when others are fearful, we think that the best time to be a fundamentally-focused investor is when everyone else doesn't seem to care about fundamentals.

We look at much of the recent market behavior with amusement and skepticism, and – since we don't hate money – we would certainly like to position ourselves to best take advantage of it. We think the more diversified portfolio approach might help (we've certainly had our share of big winners this year… unfortunately, they just weren't our larger positions).

But we're not going to start slapping 15x revenue multiples on random businesses just because that's what all the cool kids on FinTwit are doing, or thinking that we deserve to earn stratospheric returns simply for buying a stock – we believe cash flows matter, and we'll sink with that ship. We'll be value investors to the grave.

Conclusions

I would hope that over the course of 2021 and into 2022, we benefit from a reversal of what I view as temporary timing-related differences that have affected us recently. We have a number of portfolio holdings that should be very strong fundamental performers as the world goes back to normal, and we think the market will eventually recognize that.

Nonetheless, hope is not an investment strategy. While our process has worked over a multi-year period, and continued to work in 2020 (at least judging by company fundamentals rather than market performance), we are doing our best to improve upon it in an attempt to more consistently achieve strong results in any given period.

Related to what I've discussed here, I also wanted to announce my intention to wind down Askeladden Capital Partners LP at some point in 2021, and continue on solely with our SMA business (the latter of which comprises ~97% of firm AUM). This will most likely occur in Q3, although I may kick the can down the road depending on performance – I'd like to end on a high note. This was originally planned for some point in 2020, but had to be delayed, because I don't view it as acceptable to unilaterally return clients' capital after a period of significant underperformance – that's just not the experience I want to leave people with.

Why am I doing this? Return on investment. ACP LP represents only ~3% of AUM, but takes up a disproportionate amount of time, thanks to all the work (statements, audit/K-1 process, additional compliance/regulatory burden) involved with having a private partnership.

When I started Askeladden, I thought that a private partnership was the only way to go, perhaps because I was getting advice from people who'd launched in previous eras when trade-allocation tools didn't exist and SMAs involved a lot of administrative burden, or who did long/short or invested in PIPEs or etc – I literally didn't even know SMAs existed until I was six months in. (Nobody told me, and I never thought to ask!)

When I figured it out, I quickly realized that the SMA structure was far superior for our plain-vanilla business model, as it involves less administrative work, and less risk (clients retain custody and have real-time transparency into the portfolio).

The endpoint of that realization is that it is not in the best interest of either me or our clients to maintain a structure for a very small part of our business that is, in my view, net negative – i.e., it is worse than an SMA for Partners, and it takes away time that I could be spending on generating more and better ideas for all clients.

I know that some other value managers have gone in the opposite direction as they've grown (migrating from SMAs to a private pool), and certainly there are valid reasons some may choose to do that. For us, however, the Partnership is simply an inferior experience for both me, and for clients. All Partners should be able to set up an SMA if they wish to continue investing with Askeladden, a process which I will of course assist with. When the time is right, I will reach out to individual partners regarding the specifics.

I look forward to a 2021 that is hopefully better for our investment portfolio (it's already looking that way so far) – but, far more importantly, is better for our collective health and well-being than 2020 was. COVID was always a crisis with an end-date, but one that deeply affected all of us in the meanwhile, whether we lost loved ones, our work or education, or treasured relationships and pastimes.

Soon, everyone in America can, if they choose, return to normal pre-COVID life without any worries about risk to themselves or their loved ones. This winter, everyone in my immediate family, including myself and my elderly parents, have contracted COVID, fortunately recovered reasonably uneventfully, and – in one case – subsequently received a vaccine.

Although my life has been back to normal since last May, since that seemed like the optimal decision for my own health and well-being, my parents obviously took some precautions, such as not eating at restaurants. Now that they're immune, they're eagerly looking forward to catching up on some missed birthday meals, and I expect this movie will be playing very soon in your local theater, if it isn't already.

Brighter days are to come, and I thank you for your overwhelming support during days that were sometimes quite dark. The feedback that some of you provided was generally very supporting and constructive – and we had vastly more capital additions than withdrawals over the course of 2020. It is my unwavering mission to make those decisions pay off, and I continue to look for ways to reinvest revenue to drive returns.

Westward on,

Samir

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[i] There are of course exceptions, such as "prestige purchases" of sports teams, etc, where significant non-financial status and experiential value is associated with the ownership stake. But nobody buys a laundromat, or some boring industrial company, because it's cool. They buy it because they think it will make money.

[ii] The story doesn't have a happy ending: then COVID happened, and the New Zealand government shut down e-commerce, and Kathmandu did a big equity raise, so we had substantial losses on the remaining portion, although it still worked out well overall. But those unique idiosyncratic circumstances aside, Kathmandu represents the ideal situation where we weren't sitting on a big position that was languishing or going backwards for a long period of time.