Why primary research is overrated.

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Heuristics heuristics heuristics: we use them because we have to. We can't reasonably start from first premises every time we express a thought or ask a question; we'd never get anywhere. Most of the time, we don't have to worry about how exactly the internal combustion engine underneath the hood functions or how Google's search algorithm gets us closer to what we want to know; we can skip all those mental steps that (usually) don't matter, just assume they're true and logical and our car will start when we turn the key, leaving us free to focus more on the mental processes that (usually) do make a difference.

Except when we go too long without asking those questions, we often end up with nonsensical results that everyone agrees to be true. For example: in the age of social and mobile and cloud, why the heck are we still working at cramped desks in expensive co-located urban-core office space that it's societally inefficient and expensive to maintain?

One of the core assumptions underlying modern investing is the notion that *you have to have an analytical edge*. Indisputable, of course, which is exactly why I'm about to dispute it: even when I probably have an analytical edge, I'm just gonna assume that I don't, and live with that.

Surely you've heard one or more versions of the following:

If it's in the news, it's in the price.

What do you see that the market doesn't?

Do you have a view on next quarter's comps?

These are all reasonable questions to ask, if you accept the premise that an analytical edge is what leads to outperformance, except: that premise doesn't look very true.

Primary research is the flavor du jour of the investing world: it started with extensive write-ups on VIC and SumZero and Seeking Alpha, cascading to its inevitable conclusion when Bill Ackman spent countless hours and who knows how many million dollars finding out literally everything one could possibly know about the business operations of pyramid scheme (I mean, sorry, multi-level-marketing endeavor) Herbalife. And it's funny, we all knew that Herbalife was a scummy organization whose fundamental business model was "*let's see how many people can we rip off before lunch go go go*," but there's a big difference between being an unethical business and being a legal pyramid scheme. And all of the primary research in the world, most of which just confirmed what all of us could guess with 95% confidence just from having eyes and ears and functioning gray matter, couldn't save Ackman from whiffing on the core question underlying the investment thesis: *will the FTC put Herbalife out of business.*

This isn't about Herbalife, though, nor is it about Valeant (where Ackman's primary research also missed the core question about whether PBMs and payors could be suckered into the sunset), or Micron and Fannie Mae (hi Einhorn!). Nor is it about an utterly fantastic write-up I read by a moderately well-known New York fund, in which they interviewed dealers in Brazil and scoured the ends of the earth to

determine that one of the two duopolistic providers of center-pivot irrigation was an absolute gem of a company through and through. And they were right, on all of that, but still got blown up on their thesis, because they whiffed on the one question that was really important: the right starting point to use for valuation in an incredibly cyclical industry (ag), and the right long-term growth rate. I spent all of a few hours reviewing annual reports from the 1990s, comparing them to today, and determining that aforementioned fund's projections were wildly, wildly optimistic. Notwithstanding that they "knew" the company in absurdly more depth than I did, and I had no analytical edge whatsoever – I was literally just reading the publicly-available information that is everyone's starting point – I quickly determined that not only was that fund overly optimistic, but so was the market.

And then over here on the other side of the primary research equation, everyone's favorite value investor – Warren Buffett – does his due diligence on the back of an envelope, or perhaps on a Dairy Queen napkin if he's feeling really fancy, and we all know what he and Munger have done over the past however many decades. Meanwhile, his intellectual protégé Allan Mecham sits in an office above a taco shop in Salt Lake City, reading 10-Ks and apparently never even bothering to call management, and he's blown the market to bits for, oh, a decade and a half.

The plural of "anecdote" is not "data," of course, but I do find it tremendously interesting that *everyone is trying to gain an analytical edge and yet apparently in the collective never doing so*, judging by the poor performance of most long/short funds. You'll find plenty of value investors willing to go to bat halfway on this – i.e., advocating a non-quantitative approach to valuation, where they prefer conservative guesstimates to three-statement DCFs with decimal-point precision – but you'll find very few people willing to stand up and say, "primary research is overrated." Because saying that sounds a lot like, "I'm lazy and I don't want to do much work," and when you're in an industry that by and large optimizes for AUM rather than for returns, you're usually pretty concerned about the optics of what you said.

But the interesting thing about lying to other people is you eventually start to believe it yourself: and clearly the majority of investors have internalized the idea that a) they need an analytical edge to outperform, and b) they somehow have one, despite the fact that c) literally everyone else is trying to do the same thing and, in the collective, not outperforming. So perhaps there are some people who truly do have an analytical edge, and can utilize that to outperform, but in the aggregate it's not a bet that seems worth making.

What's much more interesting, to me, is turning that premise on its head: *let's start by assuming that I never have an analytical edge.* What would that mean? That would mean that while I often do "deep research" (such as facility tours, talking to people in the industry, and so on), I view those as data points rather than determinants. Okay, this is one person's opinion, or one experience dining at this restaurant – but everyone has their own agenda, their own schema, their own unique set of circumstances that clouds their objectivity. One angle does not an investment thesis make, and the aggregate of self-conducted channel checks can often be summarized in terms of Yelp or Glassdoor reviews, or competitors' publicly-released financial statements, and so on.

That is to say, granularity is not always valuable: when chatting with a prospective LP, I brought up the example of restaurant chain **Fogo de Chao** (FOGO), of which I am a modest shareholder. The thesis, reductionistically, is that the company is trading at an attractive steady-state free-cash-flow yield and has a long runway for accretively reinvesting that cash flow into new restaurants. New unit economics are fantastic, the concept already has the best of both worlds – national brand recognition and validation

with a modest footprint and plenty of room for expansion – and over to the side you have some macro noise around labor costs and soft restaurant comps, but that's all plus-or-minus stuff that really doesn't detract from the long-term thesis. The prospective LP was seeking to understand why I would spend time working on new names rather than drilling down deeper into Fogo. The answer? What would you have me do? Spend time and money visiting every new restaurant when I could just watch Yelp reviews instead? There is no new information to garner here: the make/break question on the thesis is simply *can management competently put new boxes on the ground.* If the answer is yes, the stock works over my investment horizon; if the answer is no, then it doesn't.

And again, this isn't about Fogo, but rather the bigger point of forest/trees: particularly when you're a kid running a fund out of your childhood bedroom (like me!) there is always someone out there who has more resources than you. And that argument diminishes a bit when you're at $100 million in AUM, or $500, but it still applies – assuming you're the axe on a stock is incredibly dangerous. But because it makes a good pitch to prospective investors (look at all this incredible primary research we do!), you fool yourself into believing that's how to outperform – even though people who do that don't outperform.

So if analytical edge is a mistaken assumption, what's the right one? Behavioral edge. Making good decisions. Being epistemologically humble. Take the no-modeling, ignore-the-macro approach on step further: don't just look for really wide disparities to reasonably conservative fair values, but look for really wide disparities to level-of-analysis-required. As Buffett and Munger say, "there are no points for difficulty." Just as it's probably not a good investment thesis if you have to model it down to the decimal point, it's probably also not a good investment thesis if it hangs on the verdict of one equipment dealer in super-rural Brazil.

If you study the great value investors in depth – in particular, Charlie Munger – what emerges is the concept of mental models. Most of the world isn't rocket science, and being an effective investor (or more broadly, an effective human being) isn't necessarily about being smarter or having some complex secret "edge." It's about thinking about things the right way and making the right decisions over and over and over again, in a room (/world) full of people who don't even try. People who are too busy worrying about what other people think of them, who are too busy running away with premises they didn't create and never bothered to stop to examine. Those people probably do a very good job of squeezing out whatever analytical edge there is, but they're not even trying to make good decisions – it's just not on their radar screen.

Saying "I think about things a lot and try to make good decisions" isn't an investable pitch to prospective LPs, so it's not very popular – but I don't care about being investable; I care about earning returns. So it's what I do.

Don't take me the wrong way: I think primary research can be valuable, and often engage in it. Facility visits in particular are super helpful. But I think it's not an end in and of itself. It's valuable when you use it as an opportunity to ask the right questions, on the right stories, at the right times. It's valuable when you can get it for free (i.e., free-riding on data sources like Yelp or Glassdoor or I suppose even Twitter and Google Trends, and certainly on investing-idea sites like those I mentioned.) But the fundamental problem of all organizations is resource allocation, whether time or money: and my time and money is better spent putting myself in the best positions to make good decisions, rather than searching for a mythical analytical edge I'm never going to find.