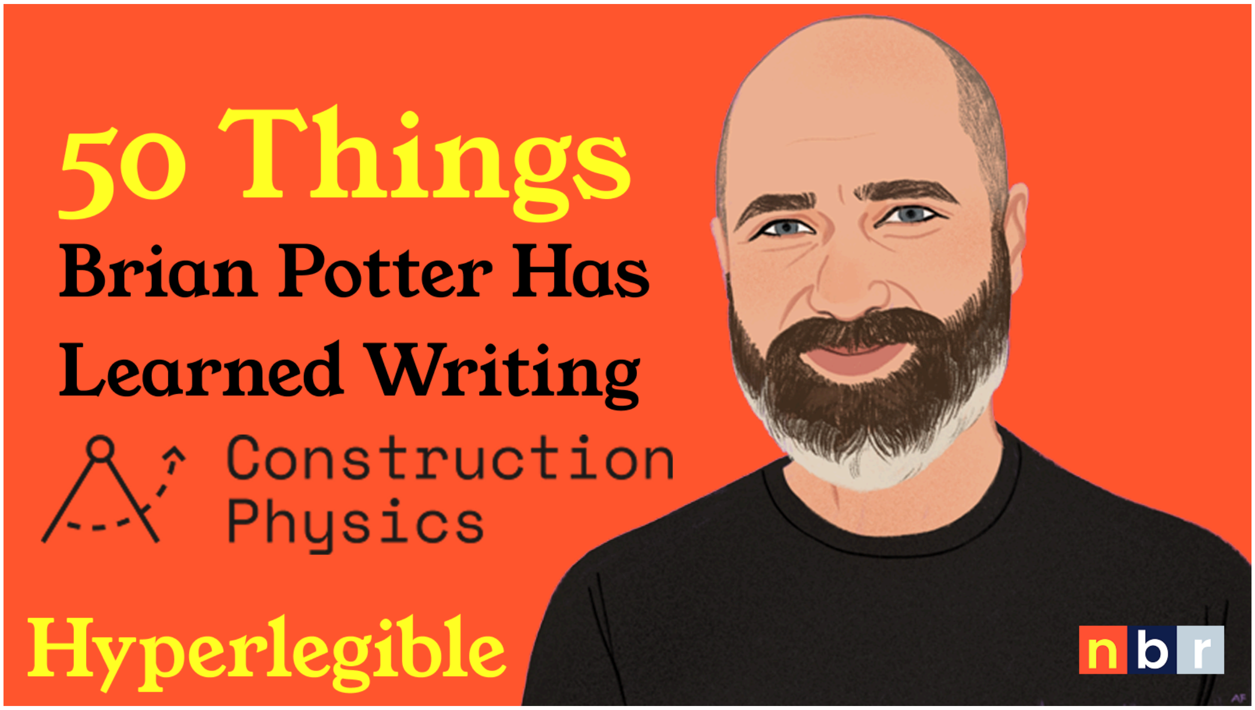
Hyperlegible 007: Brian Potter

50 Things I've Learned Writing Construction Physics



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Transcript

(**Thanks Claude for cleaning this up**)

Packy: Brian, thanks for coming on Hyperlegible. Longtime fan. Normally, to kick these off I like to ask people to summarize their essays in about 2 minutes. Obviously you've done this as a list - the essay "50 Things I've Learned Writing Construction Physics" - so instead I'll ask like what's the one overarching theme you've learned in the 5 years that you've been writing Construction Physics?

Brian: The overarching theme is that things are almost always more complicated than they seem. Simple explanations, you know, very rarely exist. There's almost... even if the simple idea that you think is - turns out to be correct - there's always relevant detail and texture to the problem that will give you a much richer understanding of why things work the way they do. So it's almost always worth it and valuable to sort of dive in as deep as you can when trying to understand a topic or a problem.

Packy: So ironically now you've tried to kind of simplify all the different things that you've written about, but then you link out to like these very very deep pieces that you've written over the years. Why did you decide to write this one as a list as opposed to some sort of essay?

Brian: The real answer is that I was on working on a compressed schedule. I was going on a work retreat that was going to eat into a bunch of my writing time, and I have a very like fairly specific time frame that it takes me to get something done typically. And it involves a big period of time spent reading background sources and thinking through stuff and things like that, and I just didn't have that time. So I was like, I need something that I can turn around much more quickly to get something out when I wanted to get it out. And so I was like, okay, well if I base it - if I base something that's based on stuff that's already written, that will cut out most of the research part because it's just stuff that I already know how to do.

And then, you know, I previously had a sort of like a table of contents, a guide to sort of the stuff that I had - various essays that I had written - when my topics were like much more narrowly focused around construction. And I sort of got rid of that a while ago when it didn't really serve as a very good guide anymore. So I wanted - I thought it would be useful to have something like that, but the list of topics is now so broad that there's not like a coherent synthesis I think of just every single thing that I've written. It's just become a much more broad scattering of topics. And so I did it in a list format. And the last reason is just people love lists, man! As Buzzfeed really got that right - people love the list format. It's very compelling to people. So it was shaped by the constraints that I was working under at the time.

Packy: I think you said you wrote 600,000 words so far, and I think I've read most of them. So actually having it all in a list form was a nice kind of walk down memory lane and encapsulation of a lot of the things that you've covered. What is your normal research process for a piece? You mentioned reading a bunch of stuff and trying to synthesize, but you now send out kind of your reading list and even that is like, "I read these seven books and then here's a million different articles that I read." So like what does that process look like for you? How long does it take? What do you read?

Brian: So typically it's kind of varied over time. For a very long time I tried to pretty religiously get something out once a week, and if something was taking longer I would just split the essay into two or three or four parts and just get it out one at a time. The grid...

Packy: One of the all-time best, the grid four-parter on the grid was one of my favorite.

Brian: The grid four-part, and then I had this very long multi-part piece that was comparing the World Trade Center and the Empire State Building construction processes - was like four or five parts or something like that. I've kind of switched away from that and I now basically just try to one-shot it and get it out all in one piece, which I think makes for better essays but it sometimes requires a longer research process.

So ideally the process will be like: I pick some sort of topic or something I want to know about, something I feel like I don't understand or a question I'm trying to answer. And I will narrow it down to the point that I can read basically everything that is written about the topic. And if you do a good job picking your topic, that's often a lot more possible than it seems like it would be. If you pick something that's sufficiently narrow but broad enough to still be interesting.

And so I'll try to read everything that I can get my hands on about some given topic. And I say read, but for most of the stuff that I'm - I'm skimming. I have an idea of the thing that I want to answer, and even something written about, you know, a book or a paper or something, most of it will kind of be not especially relevant to what I'm trying to understand. So I can kind of skim through, I'm very good at skimming, and I can just highlight all the portions that I think are relevant to me and just kind of get it all stewing in my subconscious.

And then once I've done that process and I have a bunch of ideas, you know, things that I think are interesting, I'll kind of put them on down a page and then try to kind of structure it into something like an outline. You know, what can I - how do I condense all these things that I've read, all this content, into some small tightly bound structure that I can hang all this detail off of basically? And then so I put together an outline and then I flesh out the outline into a draft, and then I go through and edit the draft.

And for a typical one in the realm of like 3,000 words or so, the whole process takes you know like a week. And so when I'm finishing up one essay I'm starting the next one. But if it's a longer one, you know, one of these like seven, eight, 9,000-word behemoths, it takes you know longer than that - two weeks maybe - a lot more reading up front and then more time to get the words down on the page.

Packy: Yeah, it does seem like you have a lot kind of already in your head about how the world works or how building things works, and then you look for specifics. How much is writing Construction Physics changed the way that you think about the world, and how much of it is just kind of you getting that all down on paper and finding how it applies to different areas?

Brian: So very little I would say. Almost none of what I write about I knew beforehand. So almost everything that I write is the output of a research process for something that I didn't understand going into it. So anything that I've written about is usually something that I didn't really understand at all going into it. And I would say the newsletter, it's not so much an output of how I thought about the world, it's more a result of the kind of research process that I had. I didn't really even realize that I had this research process.

And I think where I kind of came from was that before what I do now, I used to work in construction and I worked as a structural engineer basically designing buildings like parking garages and apartment buildings and water treatment plants and stuff like that. And what often would happen to me is I would - we would get some job, some project, and it would be some type of building that I had no familiarity with. We never designed like a building like that before. When you're a structural engineer, you can't just like wing it or, you know, the consequences for getting that wrong are like very, very high, right? People could get hurt if you misdesign a building. People get hurt, building could collapse, people could die. And even if that doesn't happen, if you screw up sufficiently you could get your license revoked. So you know, avoiding those outcomes was always very, very important to me.

And so my strategy, what evolved to sort of design these unfamiliar building projects, was to just okay, I'm just going to go look up every reference for how to design this building and, you know, go and search for all these things. Because there's design guides, you know, published by these various industry associations for any given type of building, right? For any given thing that you're doing, somebody has already designed that type of building before. And you can go figure out - there's probably an industry association that has published a guide or a set of advice on how to do that building. So go through and try to find every design guide or thing that existed for a given building, and then I could anchor my decisions in like what the accepted wisdom was in how to do this thing.

So I sort of - when I started writing this newsletter, I think I probably subconsciously transferred that process where I was like, okay, if I want to not be full of shit about this thing that I'm writing about, the easiest way to do that is just go and read everything about this given topic. And then I will be sure that I'm not missing something critical. So that's I think the newsletter has kind of evolved out of that process more than anything else.

Packy: It's funny too, I mean when you mention being a structural engineer and kind of the stakes at hand, I think that comes across in the writing itself too. Not just the research process, but like where there are things where I'll do a bunch of research but there are some things where I'll be like, "Wouldn't it be cool if the world worked this way?" Where you were just going like nine levels deep and being like, "Actually it works in this very specific way." And it makes sense when you're thinking about coming from a background of like this thing cannot fall apart.

All right, so you conclude the piece by writing kind of something similar to what you summarize. I'm just going to read it here: "My main takeaway from this list of takeaways and from writing the newsletter more generally is that there's almost always more to the story. Things that seem like recent developments often have key predecessors going back decades or even centuries. What seem like historical inevitabilities are often highly contingent products of chance and circumstance. Intuitive explanations for phenomena are often wrong, and even when they're right, the full story is often much deeper and more interesting. Causes are often complex and what seem like simple problems often tenaciously resist solutions."

So I'd love to take each one of them in turn. What are some things that seem like recent developments but have key predecessors going back centuries or decades?

Brian: Yeah, lots of interesting examples. Wind power I think, you know, wind turbines being a significant fraction of electricity generation is kind of an interesting example that feels like a very modern thing - like a post-90s thing. Maybe if you're into the energy weeds a little bit more, you know like, "Oh, in the '80s California built all these wind turbines and that's what that was the start of wind turbine construction in the US." But if actually you look back even further, wind turbine - wind generated electricity - was actually pretty popular on like farms and rural areas in the US in like the early 20th century. Because this was before there were like transmission lines out to all these rural areas providing like central station electric power. And so you would have these farms with like an electric powered windmill to, you know, power the radio and stuff like that.

And then what happened was a government administration called the Rural Electrification Administration - I believe it was a New Deal program but don't quote me on that - they just came and built out all this transmission infrastructure for all these rural areas. And that basically killed the wind turbine industry in the US because it was much cheaper and more convenient to get your power that way than it was from these sort of kind of rickety wind turbines. But you know, people have been using windmills for electricity almost as soon as electricity became widely used at all.

Another example more sort of relevant to my background is I previously to writing this newsletter I worked at a construction startup called Katerra. And Katerra's goal was to sort of build factory-built housing cheaper and less expensive than other builders could build and kind of be like the Henry Ford of housing. And this was like - sounds like a good idea. And it sounds like such a good idea, in fact, that many, many people have tried to do that before. You can go back again decades, depending on how you sort of classify it, maybe even hundred years or more, and find people trying to do pretty similar ideas unsuccessfully. And yeah, there's just lots of things that look like that, have this basic shape.

Packy: We're going to dig in a lot more on that idea because it's something that I leaned on your work pretty heavily for when I've been researching it. But more generally on kind of lessons from the past, do you think the people working on the new versions of old things more often just kind of ignore the lessons of the past, or overindex on what they've learned from the past and apply it to a situation that might not be the same anymore?

Brian: So that's a good question. Because you know, when I was working at Katerra and it became clear to me that this idea had been done a bunch of times before and it hadn't worked any of those times, my question became, well, what's different this time? What would make it succeed where it hadn't succeeded before? And I never really got a good answer to that. And then very ignominiously went bankrupt.

But yeah, it's, you know, there's lots of ideas that like it doesn't work, it doesn't work, it doesn't work, and then conditions are right and then it does finally work. Gwern, online essayist, has a very good essay about this where it's a book review about the MIT Media Lab, and there's all these people that had all these correct ideas about what the future of computing and technology would look like, but they were unable to capitalize on that and become rich because it just - you just require the exact right set of circumstances just at the exact right time to sort of be correct about these things. And if you're too early, you make no money at all, you just fail. And if you're too late, somebody else has beaten you to the punch.

So kind of yeah, for a lot of these ideas you have to have the exact right timing, and it's very, very hard to sort of get that timing. You have to understand exactly when the conditions are right to kind of do it. And then just, you know, kind of more generally, it's - there's a very sort of increasingly widely known notion that a lot of these startups and just economic growth more generally is very reliant on these essentially bubbles or like irrational exuberance where people pour these - this money and investment into ideas where it maybe can't quite justify those huge amounts of investment anymore. And then often blows up into like a huge bubble and people are ruined. But in the result of that bubble, you have built up all this infrastructure and developed all this interesting technology, and it eventually becomes quite valuable.

And so you know, we're probably seeing something like that with AI where there's all this huge amounts of money getting invested in like these AI companies and building all these data centers. A lot of these people are probably going to lose a lot of their money, but we're going to be left with like this very useful technology and infrastructure to deploy it. But if you look back again, you know, things have predecessors. You saw something similar with like, you know, fiber optic cable was this huge buildout that was done before the economics could really justify it. In the late 19th century, you saw this with railroads where people build this like huge amount of railroad infrastructure and then, you know, massive bankruptcies and lost all their money because they overbuilt it. But then we had all this railroad infrastructure after the dust settled.

So you just, you know, it's - you're kind of dependent on these like - for your economic progress and technological progress, you're dependent on these people that are individually taking like these pretty bad bets, but at a society-wide level it ultimately proves quite fruitful. So I'm not sure, you know, it's very hard to sort of be rational when you're sort of making these decisions because in what steps you should take next or what technology is sort of ready to kind of be widely deployed and what vital next no longer exist, because in some sense the system relies on some degree of irrationality built into it.

Packy: And now we're here talking on Zoom thanks to them laying all that fiber optic cable. So thank you to the people who lost your money in the past. You mentioned in there also the highly contingent nature of some of this stuff. Can you give an example of something that seemed like an inevitability but was really a highly contingent product of chance and circumstance?

Brian: One of my favorite examples of this is Morris Chang, the guy that founded Taiwan Semiconductor, TSMC. When he was just starting out of his career - like right after he graduated from his master's at MIT - he almost took a job at Ford and wouldn't have worked in semiconductor manufacturing at all. And he only took a job at a semiconductor manufacturer, this company called Sylvania, basically by chance because he got into this sort of dispute with the HR person at Ford because they wouldn't raise his salary by $1 a month. And he got very upset with them and took this other job at a semiconductor manufacturer out of spite. And then, you know, 30 years later or whatever, 25 years later, he goes and founds TSMC.

And that's an interesting example because the model - like this idea of TSMC where it's just like, "We're just going to be a pure play foundry. We're not going to do our own chip designs. We're just going to manufacture chips for other people." That probably would have come along anyway because just the basic idea that Morris Chang had, which is just these fabs are getting so expensive, it's getting increasingly impossible for individual companies to all have their own fabs - that probably would have the way that the industry would have evolved anyway. But it's very likely it wouldn't have ended up in Taiwan, right? It would have ended up in Japan or South Korea or something like that if Morris Chang hadn't ended up doing it. And of course, if it doesn't end up in Taiwan, you know, the current like global geopolitical system or, you know, concern looks quite different. So that's I think an interesting example of how it could have ended up quite differently.

The Apollo program I think is another example where it's sort of embedded so deeply in American consciousness that this is, you know, we went to the moon - that's what one of the things that makes the US awesome. And it feels like just like an inevitable outcome of the competition with the Soviet Union and there's all these geopolitical forces that are pushing us in that direction. But reading about it seems like at the time, you know, very early on it was - it was costing all this money, people were getting quite tired of it. And it does seem like if

Kennedy hadn't been assassinated and that hadn't like galvanized people into wanting to fulfill this sort of declaration that he made, that maybe we don't go to the moon. Maybe we get tired of spending all this money and the program gets canceled. And then, you know, again things might end up looking quite different.

Packy: That one's fascinating. Do you think there's a history of the world - maybe it just doesn't make any sense at all, but would love to hear why it doesn't make sense because I've never thought about this idea - where Morris Chang stays at Ford and Ford becomes a car foundry? Like do - is it great man versus great idea theory I guess?

Brian: Oh, that's well - see, Morris Chang's ideas, they were very baked into his knowledge of like semiconductor industry. And it was just like, "I know that these fabs are getting really expensive to build. I know that" - and it was also baked into the idea that he knew that like Southeast Asia was very, very good at making semiconductor chips. They had really, really high yields, like way better than anywhere else. So fundamentally baked into the proposition was that I need to go and set this operation up somewhere in Southeast Asia where they know how to make chips. So I don't know if Ford ever ends up fulfilling all those requirements, but you know, again, who knows how things would have turned out.

Packy: Knowing that so much relies on circumstance and chance and contingency, what should people do differently? How do you live? How do you prepare for that world?

Brian: You know, I guess my sort of takeaway from that, or one takeaway from that, is that just a very motivated person or like a small group of like very, very motivated people can actually have a - maybe ignoring reason or logic to some extent - can have like a very outsized impact on how the world works. Both because, you know, yeah, because stuff is dependent on chance and circumstance to some extent, and just because so much of these big outcomes, big events are ultimately predicated on a small number of people being extremely dedicated and just willing to do the stuff that the other people won't do.

Packy: So this is maybe one of my favorite parts about reading Construction Physics is that you kind of highlight the gap between what's intuitive and what's actually real. In everything that you've come across, what do you think has the widest gap between the intuitive explanation and the real story?

Brian: Widest is hard to know, but one I think - one big one I think, and I think about quite a bit, is with ship building. And I think the model that most people have - well, most people don't think about ship building at all, but the people that think about it I think the model that they have is something to the effect of: we were really great at ship building during World War II, and then after that the Jones Act made us - you know, even though the Jones Act came before (maybe they don't think about the timeline amazingly tightly) - but after that ultimately the Jones Act made us bad at ship building, and now we are crummy at ship building.

And the reality is even before World War II, and arguably sort of during World War II, the US was fairly crummy at ship building in terms of like productivity. And we were basically crummy at it since the Civil War. And during World War II we were very good at building a lot of ships quite quickly, but in very high volumes, which is like, you know, sort of a unique unsustainable situation. But in normal commercial operations, the US was still pretty not great at it. And it was not great basically since the transition from wood to iron and steel ships in the late 19th century. And that's - so that's like, you know, 80-year difference in when US ship building went off the rails.

Packy: And do you think that that's a mainly just a volume play that we were so good then? Is it like wartime we drop regulations, we fund it with federal money? Like are there things that we could do now for kind of smaller scale ship building that we can take from that period to get going?

Brian: Well, so World War II was interesting because we were able to build a very, very large amount of ships and we're able to build a very large amount of ships quite quickly. But it basically, you know, it came at a cost. So typically you're not building this huge number of ships of the same model over and over and over again, you know, hundreds and thousands all at once. And so when you're in that situation, you can kind of do things with your manufacturing process that you can't normally do. You can have something like series production and stuff like that.

But really, but even sort of at the peak of US ship building during World War II, when the US is building like, you know, a Liberty ship every 10 days or something like that, we were still less productive in terms of like the cost and like labor hours per ship than the British were. So even in that situation we weren't like, you know, we were only so good at it. We didn't totally overcome the sort of natural US ship building capabilities.

Packy: Why are we bad at it? Like the British you wouldn't think are much better. The British you wouldn't naturally think are, you know, much better. It's not like the Japanese or something where now you think like there's just a totally different model. Why are we so bad?

Brian: Yeah, this is a good question. I don't understand it amazingly well. The US was good at building ships when ships were primarily made of wood, and they were like reasonably competitive with other countries' processes. And then when it transitioned to sort of iron and steel, US ship builders were kind of reluctant to make that switch because they felt like Britain is so good at making iron, they're so good at making steam engines, they just have this insurmountable advantage in sort of the factors of production at building these ships. Our wood ships are still getting better, we already kind of know how to build these things.

And then the US had very strict - that's one reason why the Jones Act is not a major facing factor is why US ship building is so crummy is because even before the Jones Act, the US had very strict Jones Act-like laws which restricted what sort of countries' ships could go between US ports. And so they were already operating in a fairly protected market. And the US sort of historically traded a lot more with itself than with other countries. And so US ships could go up and down the coast and up rivers, and that was a pretty big market for ships. And so the US ship builders just didn't feel especially compelled to compete very hard I think is it, which is not really an answer, you know, but it's that kind of gives you the basic shape of the problem.

And then because those capitish laws, those Jones Act-like laws were in place and have always been in place since, you know, the early 19th or late 18th centuries, that situation has just never really resolved itself. And now there's all these other factors of scale and cheaper labor elsewhere and cheaper inputs and stuff like that that make it very difficult, perhaps impossible, to sort of overcome.

Packy: Makes sense. Last one from this very rich paragraph: What's a problem a lot of people think is simple that is actually really complex?

Brian: Yeah, so this is a problem - one example I'll go with is something that I spent a lot of time in the early years of the newsletter writing, which is the problem of construction productivity, which is just, you know, we don't really get any better at building buildings in terms of like hours it takes to produce them, in terms of the cost per square foot of building or whatever. None of that gets cheaper. Most stuff that we build over time, we sort of figure out other ways to build it, you know, cheaper and faster and better. And that doesn't really happen with construction.

And I think again, this sort of intuitive explanation people have for this, which is why companies like Katerra got started, was just, you know, we need to figure out how to build this stuff in factories because building in factories is what makes things cheap. And once we can figure out how to do that, we can sort of make these buildings cheaper. But really it's a little bit more complicated than that. And really it's not just, you know, the act of doing something in a factory - just doing something inside a big enclosed space doesn't magically make it cheaper. It's just there's specific things that make that are possible inside a factory. And if you can do those things, you know, have economies of scale or do, you know, automate and have very high volume production and stuff like that, that's what makes it cheaper.

And it turns out that with construction, even if you do your work in a factory, doing the specific things that a factory makes possible to make stuff cheaper is quite difficult. And so it's very difficult to make your construction more productive and cheaper just by doing it in a factory.

Packy: Yeah, we're going to dig in a lot here. Like I said, I wrote about a company called QB. I referenced your pieces that I'd read in the past, and reading again you, I think, spend 21 of your 50 kind of slots in this piece on construction. And so it's a rich vein. It makes sense that you know you can't magically just go to a factory and then things just because you're under one roof get cheaper. But I thought you had an interesting kind of comparison: why is prefab construction not cheaper than conventional on-site construction, but manufactured and mobile homes are?

Brian: So this is a good question, and I will say I don't feel like I have an amazingly great answer for it. Like I feel like I don't understand it amazingly well. I know I understand a lot of the pieces, but I don't quite know how they fit together. So one answer is just that manufactured homes are targeting sort of a lower-end market, and they use sort of lower-end, often use like lower-end less nice materials and stuff like that. And so that's part of it.

And another part of it is that a manufactured home is very good and very inexpensive if it - the smaller it can be and the more it can be in like a single unit. So manufactured homes come in like single-wides, which is just basically like one single unit, and then double-wides, which is two units put together, and then you can even go to like triple-wides, which are three units. And then sort of more complex things. And as you go up in size and complexity, the difference between conventional construction and manufactured homes starts to shrink. And so once you're getting into sort of a triple-wide with maybe a garage and a porch and all this other stuff, that difference has really, really been whittled away.

So part of it is just there's certain form factors that manufactured homes are able to build in that you can build that stuff quite inexpensively. But as you sort of get more complex and you move away from those specific form factors, the advantages really disappear. But more generally, the difficulty of prefab construction is just you often can't really save all that much money by - or, you know, make your process all that much more efficient by moving it into a factory. And there's a lot of other costs that you incur in the process of doing that.

So in terms of savings, you know, you can't really - even if you move your stuff into a factory, it's hard to sort of have your factory build things in really high volumes, partly just because the market for homes is not amazingly big. And you're shipping into sort of a relatively small catchment area because it's too expensive to ship this stuff too terribly far because homes are just so big. You can't ship them all that far while still being sort of cost competitive.

And then there's other sort of costs that you incur too. You have to break your house into a lot of different pieces, and then you tend to add costs and things like connectors to sort of stitch those pieces together. And you have extra, you know, extra probability that something go wrong. And it's - when you're building these factory-built modules to sort of stitch them together, you have to have, you know, more quality control. And you have to fix a problem - it's much more expensive to sort of fix the problem when you have these factory-built components and stuff like that.

So there's all these additional costs that you incur. I have some - I know some guys who worked at a company that tried to get into the sort of prefab modular construction and they sort of abandoned it after it didn't go well. And they came up with the idea of the "prefab tax" or the "modular tax" where there's all this other additional stuff you have to do with modular construction that makes it more difficult and more expensive that you don't have to do with conventional construction. And in general, it's just hard for the advantages that you get from doing stuff in a factory have historically not really been enough to offset that in a lot of cases.

Packy: And all of that said, reading your kind of reflections on your experience at Katerra, it didn't seem inevitable that it was going to fail, right? Like there was a bunch of stuff in your piece. It was like decisions got really slow and we became a big organization, or it seemed like the CLT bet just didn't work out the way that you thought - the CLT, or they thought the CLT bet was going to work out. Like it seems like there were avoidable things that in retrospect maybe Katerra would have worked had they done stuff differently. Taking all those lessons, like if you had to solve this problem, if your job - the president appointed you, you have to figure out how to fix this by starting a company - what would your approach be?

Brian: This is - so this is not the most interesting answer in the world. My approach would be, you know, somebody writes me a check for, you know, $500 million or whatever. I would take that check and I would sit on it for 10 years, 15 years, 20 years until automation and robotics are good enough that you can basically completely automate the on-site construction process. And then that would basically solve the problem. That's, you know, for if nothing else worked, if you can replace, you know, a carpenter making $50,000 a year with a robot that cost $5,000 or something like that, that essentially solves your problem, right? And so I would, you know, just wait until the automation gets good enough. If the check is really big, you know, if I have a five billion dollar check, maybe you can actually do the work to push that automation forward.

Packy: I think Masa wants another shot.

Brian: My answer - that is the most straightforward way I know that would basically solve the problem of construction productivity.

Packy: That makes sense to me. Why does Chicago build skyscrapers twice as fast as New York City? I did not know this before I read this.

Brian: Yeah, I didn't know it either. Yeah, it's a good - that's a good question. I don't have a - I'm not super confident in my answer. My general sense is that New York has a lot of very burdensome building rules that make it really expensive to build there. There's all these rules about unions...

Packy: Yeah.

Brian: You know, lots of major cities have unions. I don't know if Chicago does or not. But I think the building unions in New York are particularly - are particularly burdensome in a lot of ways. There's all these rules about safety that end up being burdensome - all the, you know, scaffolding. I think they have very stringent scaffolding rules and stuff like that that can often make it difficult. As I understand it, you can't really build with like a tower crane in New York, so you have to use these other smaller, less good cranes to kind of build - get your buildings built.

And so there's all these difficult rules that make it just especially difficult to build in New York compared to a lot of other US cities. And Chicago is the only other city other than New York that really builds appreciable numbers of skyscrapers, so it's the - you know, it's not so much New York versus Chicago I think it's New York versus most other places in the US.

Packy: And then you blow my mind in the opposite direction by saying that in terms of building high-profile skyscrapers, the US is actually as good as China - as fast as China, as good as China. What's going on there?

Brian: Yeah, this is also very surprising to me. This is another thing I feel like I'm adding this caveat a lot - like I don't really know the answer. And the reason I gave that sort of stipulation that it's high-profile skyscrapers is because this is based on data from this website called the Council on Tall Buildings and Urban Habitat, and they - it's a big database of essentially every, you know, most major skyscrapers that are built in the world. But I don't think that that database really tracks all these like the huge numbers of like residential apartments that get - you know, China puts up, you know, hundreds of like identical buildings, you know, that are all this like residential concrete high-rise. And they're not track- as far as I know, they're not really tracking those ones that are in there. There's not like super great information for...

So it wouldn't surprise me if like those types of buildings they can get built quicker or whatever. But so for like high-profile, which means like the big stuff that gets like, you know, tallest in the world or more than 80 stories or something like that, yeah, they're fairly comparable. And I don't have an amazingly good answer. I sort of - one possible part of the answer is that in general, construction developers often seem to value speed of construction a lot less than you might think that they would.

And I don't under- this is another thing that I don't understand like amazingly well. But you would think that it's like for most things, if you're building something, you maybe have like a lot of like fixed cost or overhead bound up in the building of that thing. And so if you're not producing something, you're still losing money because you're not spreading your fixed costs out or you have - you're incurring overheads without getting money back in from selling the widget or whatever that you're making.

And in construction, that's a little bit less true. Your costs are a large and part labor and materials. And if you're not building something, you're not incurring those costs. That's not like, you know, that's not totally true because there's overhead cost, there's general conditions and construction loans and stuff like that. But yeah, historically it's - I've observed that just developers just often don't seem to be amazingly concerned about getting stuff built super quickly, at least, you know, modern day ones. If you go back historically, that becomes different.

Packy: Makes sense. There's been an empty lot across the corner from my house for I think the four years since I've moved in, and now it makes sense. They're just incurring whatever costs - you know, they don't have to pay for labor and materials, and so it's just going to be a rat-infested empty lot on the block for a while. But speaking of high overnight costs, let's move on to energy because this is one of the areas again that I've leaned on you a lot for. Like I mentioned, loved the grid piece. But talking about nuclear and a bunch of other stuff, and I think in your energy category, the most interesting question for a lot of them ends up just being kind of why. I think there's probably a lot behind those. So to start, why did the US start adopting gas turbines after the 1965 Northeast blackouts, and did that adoption accelerate? They're becoming economical?

Brian: Yeah, so with gas turbines, it's interesting because it's an idea that people have had for like a really long time. You can find like people sketching out like gas turbine ideas since like the 1700s or something. But to get a gas turbine to like produce a useful amount of energy, you need to have a pretty efficient compressor and other sort of components like that. And it just took a long time for those compressors to get efficient enough and good enough that you could like extract a useful amount of energy from a gas turbine. And it wasn't really till like World War II that that kind of started to be true.

But with the 1965 blackout, what happened - you know, there was this huge blackout all through the Northeast in the US. And I think a gas turbine was actually used - I should have looked this up before this interview, but so I'm, you know, 70% sure that a gas turbine was used to like sort of kickstart the grid back into operation after the blackout. And it turns out that gas turbines are like very good at like starting a grid from what's called a black start, when you just don't have any power at all.

And so a lot of utilities bought gas turbines as basically like an emergency measure in case of a major blackout. And that ended up being the big initial industry use of these like industrial gas turbines. But it was really - what really accelerated gas turbine adoption was this law in the 1970s passed by Carter called PURPA, which basically was the law that forced utilities to buy small amounts of electric power from like private producers. And sort of there was the first major thing that like kind of broke the utility monopoly on electricity.

And so many - and the law was structured in such a way that if you - it only had - they only required to buy power in certain conditions, and one of those conditions was if you provided both heat and electric power combined - you know, combined heat power plant. And a combined cycle gas turbine, which generated electricity and also heat and then recaptured that heat to generate more electricity, qualified under PURPA. And so you started having all these, you know, small operators, non-utility operators start building these combined cycle gas turbines. And that's what kicked off, you know, combined cycle gas turbine adoption in the US.

As far as improvement, it was really kind of just, you know, a long many decades of just incremental improvements. They slowly got better, they slowly got cheaper, and then eventually you had this sort of instigating events that made people suddenly start to adopt them for specific uses in large numbers.

Packy: Chance and circumstance. Why does Oregon have the second most planned electricity generation after California?

Brian: This is a very good question. I don't really know the - this is one I really don't know the answer to, and I do not have a good theory. I know they have like a lot of like hydroelectric power and stuff in sort of the up in that part of the country. So I don't know if they're like buying - if, you know, buying this - these power generation going to like plan on selling it to other states more broadly. I don't know. I really don't know this one.

Packy: All right. You write that the US has about 10,000 times as much energy storage capacity for hydrocarbons as it does for grid-scale electricity. What do you expect to happen to that ratio over time, and how?

Brian: Yeah, so that's - it's an interesting question, and it kind of depends on like how different technologies evolve and like their comparative rates of improvements. You know, if batteries continue to get cheaper, I expect them to get coupled with new solar PV deployments more and more, and probably wind turbine deployments as well. And so I expect battery capacity to really continue to increase quite a bit. The amount that it will increase, I don't - you know, is it going to get, you know, 10 times more, 100 times more, a thousand times more? I don't have a super good sense of that. You know, for that you need to talk to like a real energy expert that understands all this modeling and stuff like that really well.

But so I expect batteries to, you know, get more deployed. But then depending on how, you know, certain technologies work out, you could see a decline in, you know, hydrocarbon use. You could see a, you know, steady state or increase in hydrocarbon use. If, you know, some of these scenarios where people think, well, solar PV is going to get just incredibly, incredibly cheap, and the best way to take advantage of that is to use this incredibly cheap solar PV electricity and just synthesize synthetic fuels, right? And then just take advantage of all the hydrocarbon infrastructure that we already have. And so if that ends up happening, you could see maybe we need even more hydrocarbon storage, you know, if we need to double or triple the amount of energy that the US uses. So I don't - yeah, I don't know. It kind of depends on sort of how some of these other technologies continue to evolve.

Packy: Both batteries and specifically solar are like the poster children for learning curves. You write that kind of learning curves can stop and you can't take them for granted. What stops learning curves?

Brian: The big thing is just embedded in a learning curve is just the idea that like there's possibilities to sort of improve this process or improve this technology. And if those possibilities just kind of aren't inherent in the technology or whatever it is that you're doing, then your learning curve can only kind of really go so far.

And the example that I use to sort of point to this is like titanium, which has improved on like a learning curve-like fashion, but it hasn't fallen down the learning curve very far. And a big part of that is just the process that's used to produce it is this chemical process called the Kroll process. And it's, you know, it's really not - it's not the process you would ideally choose if you had your druthers as to how you could make a given material or given chemical. I'm not a chemical engineer, but I get the sense that it's like it's kind of involved. It's like a batch process.

It's hard to - it's kind of difficult to do. It's kind of hard to keep the titanium from getting contaminated and stuff like that.

And so it's just there was hope, you know, for a long time they would find some sort of better process to make this material, and they really just haven't found one. And so you're stuck with this sort of kind of crummy process to make something. There's only so much that you can kind of squeeze out of that.

And then another big thing is just the - it's kind of the structure of the product that you're offering. And so there's a very good paper - I don't remember the name of the authors - but it basically breaks down energy technologies into sort of different categories based on what sort of kind of learning curves that you see. And the stuff with the best learning curves is stuff that can be manufactured in really, really large volumes and has sort of a uniform structure and doesn't really need much custom adaptation to the specific use or the specific site or something like that.

So something like solar PV, where you can just make, you know, a hundred billion identical modules in factories is very amenable to sort of learning curve stuff because you can just scale up your factories and get huge economies that way. And making the same thing over and over, you can improve your process and reap the benefits of that over a large amount of output. If your thing requires a large amount of like customization and adaptation to like specific circumstances, it's much harder to reap the benefits of learning curves and have really large learning curve style improvements. So any sort of like major construction project type thing kind of falls into that category.

Packy: The promise of SMRs and kind of new nuclear is we're going to take it from a construction process to a manufacturing process, and we're going to come down the learning curves. You talk about the fact though that the US Navy follows many best practices for nuclear reactor construction such as design reuse, but this hasn't been sufficient to make nuclear power cheaper than other forms of ship propulsion. Why do you think nuclear is fundamentally just resistant to learning curves, or is this possible to get them on that learning curve?

Brian: Yeah, this is a good question. And so a lot of - I've said that like four times, "that's a good question." Wow, every question is a good question. Part of that is just that construction project thing. And then I think with nuclear especially, it has some unfortunate negative economies of scale where stuff gets like - you know, typically with like something like a power plant or like a chemical facility or something, you get pretty good economies of scale due to geometric effects. As you make your equipment physically bigger, mostly due to like area-volume relationships - you know, you make a tank that's like twice as big and it does not take twice as much material to make that tank.

And nuclear plants are sort of major, you know, big chemical handling facilities in some sense. And so a lot of power plants like, you know, coal plants or whatever, they will show these like nice economies of scale making them bigger and bigger to a certain point. But with nuclear, there's some sort of unfortunate negative economies where as you build your plants bigger and bigger, the risks become bigger and bigger. And so you require - you're required more burdensome safety systems to sort of prevent those risks from coming to pass.

So a big - the risk of a meltdown from like a big nuclear plant is much, much higher than - much larger than a much, much smaller reactor where if the reactor is small enough, as I understand it, you really can't have a meltdown at all because there's just not enough heat generated. So that's I think a big part of it is that you kind of have these sort of like negative economies of scale in nuclear plants.

And then just in general, there's, you know, with most things it's like, you know, we built this thing and we had some problems, and we then fix the problems and it works better. But with nuclear, everyone is so worried about the risks of radiation that, you know, we built this thing and we figured out there might be some problems, and then we had to like change everything how we do about this construction to make sure that we never have anything resembling those problems again. So every new, you know, improvement comes with this like new ratchet of regulations and overhead that makes everything more difficult.

So yeah, it's just, you know, again, people are fundamentally very worried about nuclear safety, and it just, you know, bubbles up into all sorts of difficulties with building this stuff. And US naval reactors are very similar in that sense. People are very worried about radiation leaks, and so they're built to a very high standard of safety, and that just makes it hard to sort of build this stuff cheaply.

Packy: Yeah, it's funny how many similarities there seem to be with your kind of ship building and Jones Act story where you blame the Jones Act but all the seeds were planted before. Same with kind of Three Mile Island or even the NRC and kind of nuclear. There was, you know, the over-ordering of nuclear plants and it was a utility killer, and Jane Fonda was like - all these things kind of happened. And then you get Three Mile Island, and then you get kind of the NRC. And so now you're in a spot where people are scared, but then you put regulation in place and now it just gets so unbelievably hard to build even if you wanted to get back on the learning curve that it seems like a tough situation. But a lot of people are taking different approaches, and so, you know, fingers crossed and optimistic.

Brian: Yeah, it's on my list of - it's perpetually on my list of things to like look into small modular reactors because I really haven't spent much time learning about it, and I don't understand it amazingly well. But yeah, historically with power plants, geometric scaling - you're just making them bigger and bigger and bigger - has been a pretty important way that electricity has gotten cheaper. And so I'm not - you know, with a small modular reactor, there's like a trade-off there. You have the benefits of like series production where we're going to make, you know, 50 of these and the 10th one is going to be cheaper than the second one or something like that, but you're trading off some of those geometric benefits. But again, as I pointed out, you're also trading off maybe some of these safety requirements as well. So it's not amazingly obvious to me what that looks like on the balance. So that's kind of one of the reasons that I eventually want to look into it when I have the time.

Packy: I'm going to put my vote in if there's a voting process for the SMRs. I cannot wait to read your take on it. I know we're coming close to time here. I have a bajillion other questions, but I'm just going to give you a couple more that are like that really kind of surprised me. One is that the number of US bridges in critical condition actually declined by 70% between 1992 and 2023. And you said that US interstates are roughly as good as other countries. Why do people think we're so bad at these, and what have we actually gotten right?

Brian: I'm not sure. I think part of the reason why people think we're so bad is that there are a lot of things that the US is like legitimately quite bad at. In general, the construction stuff that the US is best at is stuff that is a single family home or like resembles like a single family home. And then there's certain other things - industrial facilities we're probably pretty good at. But a lot of stuff, you know, the more that something requires like a lot of government involvement and oversight, the worse that the US tends to be at. So there's a lot of things that just we are legitimately quite bad at. And yeah, like the things that we're better at - homes that we're good at building, stuff like that.

Packy: Two more just rapid fire ones before I go to my three closing questions. What did Cambridge and Bell Labs get right in terms of Nobels?

Brian: Cambridge I don't have a good sense of what went right there. You know, the US is - the US today is sort of in a position that Britain was at the late 19th and early 20th century where maybe a falling industrial power but very, very, very strong in science. And the US in this - and UK was just very, very good at science in the late 19th and early 20th centuries. And Cambridge has just been very good for very long.

As far as Bell Labs, Bell Labs was able to hit this very difficult spot to hit where it was able to take a very, very extremely long-term view of what was like valuable research and stuff like that, and fund this highly speculative stuff that might not pay off for years or decades or whatever because they were sheltered under the wing of a government monopoly. But they also were pretty careful to make sure that the stuff wasn't just totally blue sky, totally disconnected from anything that could ever make a profit for the company.

So they had this whole army of like, you know, special engineers and managers that would like subtly redirect the efforts of all their researchers to working on stuff that was likely to be promising, even if that, you know, promising payout was in a long term in the future. They kept them on track to doing the stuff that would likely have some sort of commercial benefit.

Guy named Eric Gilliam - he's written a lot about research organizations and what makes a good research organization, and he spent a lot of time researching Bell Labs. And he describes it - I'm gonna sort of screw up this phrasing which is better - but it's like they have a long yard but a narrow fence, where they had a lot of freedom to explore different research avenues, but that was directed in a pretty narrow way to the stuff that was likely to have a good payoff. And so they were able to sort of hit this like relatively narrow target and existed in a realm where they could afford to just spend money on this stuff for a very long time because they were just this huge enormous, you know, company monopoly. So they had a lot of - it was a sort of special confluence of circumstances that are quite difficult to repeat.

Packy: Last one that I'm going to go right at your list on, and then just three easy ones. So thank you for putting up with this just like onslaught of... Oh yeah, you're fine. Why have jet engines and commercial airliners always been so expensive to develop?

Brian: This is another thing that I don't really understand amazingly well. You know, the nice thing about being able to write about kind of whatever I want to is I can just dip in and out of the various topics as they catch my interest. But the downside is, you know, a lot of these things I'm not an expert on, and even after reading - even after reading quite a few books on a topic, that still doesn't make you an expert.

You know, with jet engines, it's just - they've, you know, it's this combination of needing like really high levels of performance. They have to, you know, these jet engines are operating at really, really high temperatures. Everything has to be made to a pretty high level of precision. It has to be like really, really reliable to kind of be safe. And that's just a diff- it's always just been those three things have been kind of difficult to achieve simultaneously.

And with air- with commercial aircraft, you know, commercial jet aircraft in particular, I think it's somewhat similar concerns where it's just hard to make these things reliable and safe and fuel efficient all at the same time. And it just takes a lot of investment to kind of make that happen.

Packy: All right, so now the easy stuff. What's one other essay of yours - there's a bunch of great ones linked here, I think people should read all of them - but if you had to choose one other essay of yours that people should read, what is it?

Brian: It's funny. The most - you know, there's a contrast. The stuff that I like the most is not necessarily the stuff that is the most popular, and the most popular stuff is not the stuff that's necessarily in my wheelhouse. I was looking at it the other day, and I think the most popular essays are all stuff that is written about like airplanes and aircraft. So like, you know, building airplanes in World War II, titanium, airports, you know, how to build a jet engine - all that stuff is very near the top, but it's not necessarily my background.

And then the stuff that I like the most is often stuff that is - other people do not necessarily seem to be amazingly enthusiastic about it. So I recently wrote a piece about diamond drill bits and how the technology has sort of evolved over time and what it tells us kind of about like learning curve improvements that I was really happy with and I learned quite a bit, but it, you know, it didn't make a huge amount of waves.

Another one that I personally really like is exploring the early anti-growth movement in California, which is another, you know, example of something that feels like a concern of today's times but is really something that has very deep roots. And sort of you can see a lot of the same, you know, anti-building, NIMBY-type concerns that we're constantly dealing with today in, you know, take place and take shape in California in like the late '50s and 1960s. I think exploring those I think really gave me a better understanding of why we have these sort of anti-building movements and why they're so sort of difficult to do.

If I had to pick one, I'd say "Building How to Build 300,000 Airplanes" - building airplanes in World War II - which is quite popular and a lot of people like it.

Packy: Awesome. Yeah, again, read the whole catalog. I think my favorite has to be the grid series. I just thought it was amazing, but and I've linked to it in a few different pieces, so if you haven't read it already and you're listening to this, go read that and go read "How to Make 300,000 Airplanes."

What's one essay, or you can choose book because you've written - you've read so much as part of your research process - like what's one that you just loved that you think other people should read? An essay or a book?

Brian: There's this essay - it's written, I don't remember the guy's name, John something - but it's called "Reality Has a Surprising Amount of Detail." And it's just about like for any given thing that you are trying to do, actually accomplishing it requires you addressing and coming face to face with a lot of specific nitpicky details. If you don't resolve these specific nitpicky details right, you'll be able to - not be able to accomplish the thing that you are trying to accomplish. This applies to basically no matter what you are doing. And reality is rarely as simple as would be convenient for us, and there's always sort of, you know, complex nuance of things that are important to take into account. So I think that one is very quite good, and it rhymes with a lot of the things that I conclude in this sort of 50 things that I've learned piece as well.

Packy: Yeah, normally I would ask you for the one sentence takeaway, but you kind of gave it there and we've talked about it throughout - that the reality has so much complexity. So I guess the question is similar to one I've asked about something different earlier, but knowing all of that, knowing that reality is so complex, what should people do? Like how do we go fix the world? How do we go build new things understanding that there's so much complexity and detail out there?

Brian: You know, I think the sort of most straightforward answer is just remember that - remember that stuff is detailed. Remember that simple answers usually aren't so simple. Remember that the world is complex and don't get caught up in the sort of simple, easy, comforting explanations for things, and be willing to sort of put in the work to understand how stuff is - how complex stuff really is. And have the humbleness to recognize how much that you don't know, and how much for any given topic you still probably need to learn about it before sort of drawing conclusions from it.

Packy: Well, Brian, thank you so much for teaching me some of those details through your writing over the years and letting me appreciate that lesson even more. And of course, thank you for doing this with me today. This was fun.

Brian: Yeah, thanks for having me.