Mastering Problem-Solving: Insights from St. Jude's CEO

Dr. James Downing 00:00
As I became CEO, it just was essential to hire people who complimented my skills and weren't afraid to challenge I need them to challenge me, I need them to say no, they think this is more important than that. And I need to listen and react, you know, what are those most important challenges, sometimes you see a problem, and you want to go for a solution that accomplishes what you think the answer should be. And that's not the way to approach it,

Jacob Morgan 00:24
here are the five problems that we're faced with, we need to solve all five of them, go figure out a plan,

Dr. James Downing 00:29
it's up to you to figure out what to work on. That's not really an important problem that that you should be working on, you can do it, you could accomplish something, you could publish some if it's not on mission, we're not going to do it.

Jacob Morgan 00:41
My guest today that I actually just finished interviewing is Dr. James Downing, he participated in my last book called The future leader. And he's also going to be featured in my new book leading with vulnerability which will be coming out in the coming months. For those of you not familiar with Dr. James Downing, he is the president and chief executive officer at St. Jude Children's Research Hospital. And he's responsible for leading almost a $13 billion commitment to accelerate progress in the research and treatment of pediatric cancer and other catastrophic diseases. So we had a very, very interesting conversation today, which was framed around this idea of how do you solve the world's toughest problems? How do you approach them? And how do you tackle them, not just in the realm of science, but also in the realm of business as well. So some of the things that we talked about are specifically how do you break down a problem and approach it? What does it mean to have a blue sky approach to solving problems, we also looked at how to test ideas, measures of success, what to do when ideas don't succeed. And also if there is such thing as a good or a bad idea. So it's a really, really interesting conversation specifically around how to approach a problem. If you are a subscriber to leading the future of work plus, and you can subscribe, by the way on either Apple podcasts or on Spotify. If you are a subscriber on either of those places, then you will get access to a bonus episode with Dr. James Downing, where we talk about how to connect people to the work that they do, and some specific strategies that you can use and and go with for that. So again, that's only available to subscribers of the show, which is available on either Apple podcasts or on Spotify. When you subscribe on Apple, you'll get ad free listening early access to new content, and a bonus episode every week. If you subscribe on Spotify, you will get ad free listening. And you will also get access to the bonus

episode that you can watch as a video not just listened to. So I hope you decide to subscribe to the show on either Apple podcasts or on Spotify. And really, what I wanted to frame our conversation around today was this idea of how to solve some of the world's toughest problems and challenges and how you approach it and how you think through them. And so with that context, maybe we could begin with just a little bit of background information for people who are not familiar with Wii U. And with St. Jude Children's Research Hospital, maybe you can give a little bit of context around around what you guys do.

Dr. James Downing 03:14
Yeah, so St. Jude Children's Research Hospital is a unique institution. We have our main facilities in Memphis, Tennessee, and we're a mission based organization. And the mission is to advance cures and means of prevention for pediatric catastrophic disease through research and treatment. And so different than any other children's hospital different than any academic medical center really focused on that mission. And I've been at the institution for 37 years, as sort of a young man to develop myself as a scientist, and so sort of wandered my way through the organization and just completing my ninth year as CEO. So it's been a long and interesting journey. And I've seen the institution from a small place when I first came have somewhere around maybe less than 1000 employees that today over 6000 employees, a massively different campus and strategic plan that calls for an additional 2300 employees and almost 2.3 billion in new construction over the next five years. So a growing institution that's focused on that mission. And that mission is to address the toughest problems in pediatric catastrophic disease. What are those and what can we do that other places can't do?

Jacob Morgan 04:41
Yeah, no, it's it's fascinating. And I remember I had the opportunity to speak there. How many years ago was this? Oh, man, supposed to be more than half a decade ago, where I had the opportunity to see the facility. I think that's even before some of the new construction that you have going on. And it was just a very, very impressive place to see and some of the work work in seeing some of the rooms and the equipment and the machinery. It's, it's, it's really, really wild. So you mentioned this idea that you guys focus on solving these really, really complex challenges and problems. And what I thought would be really interesting is to take the approach that you use and your team's use on solving problems, because maybe a lot of business leaders out there can take that similar approach to apply it to their problems, even though they might not be around fighting cancer, or, you know, helping children with the illnesses that they're faced with. But it could be around their business problems, their challenges, their innovation. So let's start with, let's say, you are presented with a problem. So what what kind of a problem would you be presented with? And then how do you start to think about it as a way to even begin to try to tackle it? Yeah, I

Dr. James Downing 05:52
mean, I think I always start at a slightly earlier step, you know, it's really what is the problem that we're focusing on? What is that challenge that we're faced with? Is it the most important challenge? Is it significant enough for us to focus on it and address it? You know, if it's an obstacle, well, what's the one obstacle that if I, if I address that, many of the others I won't need to address? So, you know, for me, it's always being as crystal clear as possible on what the problem is. And so, you know, for a place like St. Jude where, you know, when we were developing the most recent strategic plan, you know, I spent two

days by myself, what are the problems we're focusing on? across our entire spectrum of activities? What are the most important problems, the most important challenges? And how can I be crystal clear on what those are? And how can I then select out of those because you have to prioritize? I like to say those that are tractable, there are many problems where, you know, in research or in medicine, the technology may not be ready, our fundamental understanding may not be ready. And so even though it's a challenge, we can put all the money we want in the world on that problem, and we're not going to make progress. We have to go earlier in the process. And so, you know, what are those most important challenges? Is it tractable? Can I actually make progress on it? And then, you know, what will it take to make that progress? And I think, as we lay out that, then we go back through that process at greater and greater depth, you know, clarifying the problem, what is it we're trying to answer? How are we trying to answer that? What is it that we exactly need in background information in peripheral research? And then what's our approach going to be to meeting that challenge? You know, and, you know, as an example, you know, we look at success of pediatric cancer is been incredible, over the last 60 years from, you know, less than 20% survival to over 80% survival today. So the problem though, is when you look at that, that still means one in five children in the US or in developed countries are going to die. And so how do we cure those incurable cancers? And so then you start thinking, Well, okay, I know sort of what they are, and I can name what they are. So let's just focus on that cancer, this cancer, that cancer and see if we can make progress that's like, No, we can't do that. We have to go further back. And we have to invest in fundamental science and basic biology to understand how a cell works and what the alterations may be that lead to that cancer, you know, so, you know, sometimes you see a problem, and you want to go for a solution that accomplishes what you think the answer should be. And that's not the way to approach if it really is is attractable. What aspect of it is attractable? Can I address that? And then is it worth the effort to address it in the bigger picture of what you do, and what your organization is? You know, and so we look at the problems we're addressing as an organization, but then we also have problems that interfere with what we're doing. And we approach those much the same way. You know, is it a problem that I can really focus on and address? And so that's part of the way we think about it.

Jacob Morgan 09:46
With hybrid work is the new normal, how are you collaborating securely and making sure your data is safe in the cloud? AvePoint has your back. AvePoint provides the most advanced platform to optimize SAS operations and secure Collaboration, no matter how big your business is, or what it does. AvePoint can help you modernize your digital workplace, over 17,000. Companies trust AvePoint to rapidly reduce costs, improve productivity, and protect your data with the renowned confidence platform. Visit AvePoint. today to get started and learn more, that's AvePoint AVPO i n t.com. So walk me through this idea of is it even worth doing? Because I don't think that's even necessarily something that a lot of leaders think about right? Like, is that problem worth solving? I think the traditional approach a lot of people have is, here are the five problems that we're faced with, we need to solve all five of them, go figure out a plan. And it sounds a year approaches. Well, wait a minute, let's take a step back and figure out can this problem still exist? Do we need to tackle it? So how do you think through that? Because what happens if you don't tackle that problem? Does it just continue to exist and just keeps floating around out there in the world,

Dr. James Downing 11:07

you know, on the vast majority of problems, you're going to address they're not emergencies are not at a code, there's not somebody arrested on the table, and we've got to get the heart going and get get, you know, circulation going and get respiration going. You have time to stop and think and evaluate, before you move. And before you jump. And, you know, so I remember when I was a young man, you know, as a researcher working on research problems. And as an as a scientist, you're fit, you know, it's up to you to figure out what to work on. And I remember to two episodes, when I was very young, I was probably one I was in my late 20s. And, you know, what I might have been in my early 30s. And the first one was a Nobel Laureate, and I was telling him what I was doing. And he stopped me. And he said, so why are you working on that? And I think pretty clearly what he was saying was, this is not really an important problem that you should be working on, you can do it, you could accomplish them, you could publish something, but why are you working on it. And so, you know, I answered and went through the day, but that night, it hit home, what he was saying, you know, probably about 10 years later, I was meeting with a senior investigator I was visiting somewhere giving a talk, and he just made it crystal clear. He says I only work on really important problems, because I don't have much time left, and I'm not going to waste my time, I'm only going to work on what I think is important. And so, you know, I think you have to ask that question all the time. And then you have to ask, and these five problems that are sitting in front of me, is there one, that if I address that, I won't have to address some of these other problems, they'll become irrelevant. And if they are, then you've got to go after that. And that's that idea of the one thing I think there's a book written on it that what's that one thing that if you do that, most of the other things won't be required. And so, you know, I think that pause, you know, being utterly clear on what the problem is, you know, writing it so that it has sharp clarity, and you've thought it through and you know, often you say this the problem? It's like, no, no, that's not the problem. Think a little deeper. Okay, this is the problem. Now, think even deeper, lay those out and then assess, you know, is there one that won't make the others irrelevant, and is the one I'm going to choose important enough for me to spend my time on, you know, or my company's time on to address.

Jacob Morgan 13:48
So let's say you pick that problem. So it seems like first step is you kind of get all the problems together, so to speak. And then you need to define, which is the one that you should be tackling first. And that's, it sounds like you do that through a series of questions that you ask yourself around. Which one if I don't tackle it would make me irrelevant, which ones, you know, kind of like are not going to be life threatening, or that I have time to tackle versus which ones I don't. So it seems like the constraint there is mainly around time, resources and kind of even ability to go after that problem to begin with. So once you identify that, what's next, so I'm a business leader at a company, I brought my team together, we found five problems and I pick one, maybe two, maybe it's around growth, maybe it's around creating a new product or service or getting into a new market. I spotted the problem. What do I what do I go after next?

Dr. James Downing 14:46
You know, there are again different ways I think about it, you know, first even in establishing the problem. You know, I spend time by myself. I spend a lot of my time thinking in Getting my thoughts together, but then immediately it's to my senior leadership team, because I might be completely wrong. Right? And and I need them to weigh in, I need them to challenge me, I need them to say no, they think this is more important than that. And I need to listen and react. You know, I like to say a lot of times I

throw out good ideas, but they always come back much better. By that process of them weighing in, you know, then if we've decided this is an area we've got to go, the next step is do we have the expertise to move forward on this? You know, and so we have to critically assess, you know, do we have the expertise? Do we have to hire consultants to get us more information? Do we need to establish collaborations, you know, outside of our organization to move forward? You know, or, you know, do we need to spend time acquiring knowledge to then convince ourself, we have enough to go forward? So I think, again, that's a critical element, you know, often it's just hire consultants and let them come and tell us what to do. Yeah, that's a complete mistake, right? Because that's being lazy. And so it's better for the senior team to dig in, and to say, Well, what do we know about this? How might we approach it? Do we have the expertise? Do we know who might be logical collaborators? Or is this an area we're completely ignorant on? And we do need to hire consultants. And if we have to go that route, we need to ask ourselves, is this something we should be taking up? You know, and so we always ask ourselves, you know, once we have one way we structure on new initiatives, what does the world need? Are we do we have the expertise to address that? Or not? Is it on mission? Or not? And then can we raise the funds to make it sustainable, as long as it needs to run to solve the problem or address the problem? And so we look at the intersection of those four things, is really that next step, you know, if we know it's something the world needs? Next up is, you know, do we have the expertise to do it? Or can we acquire the expertise to do it? But if we can't, then we shouldn't be doing it? And, you know, if it's not our mission, we're not going to do it, you know, in the end, and then we do have to worry about the economics of it.

Jacob Morgan 17:25
Yeah, I like the the step in there that you included about the challenge, you know, the challenge process where a good idea becomes better after you throw that out there. And I don't know, I wonder how many leaders actually do that. I feel like a lot of leaders inside of companies say, Hey, here's the idea. It's my idea. It's a good idea. Go with it. Because letting other people challenge you requires a little bit of humility and vulnerability. And I remember when we talked a little while ago, you told me that you used to not be the kind of leader who was so open to that, right? You used to be more? What's the nice way to put it more commanding control more kind of like, you know, so what, can you talk a little bit about that transformation that you went through? And why this idea of being challenged and being Okay, with that is so important, and how did you even get to be okay with that without letting ego get in the way and getting upset?

Dr. James Downing 18:22
Yeah, I mean, I, for me, it was more of a leadership journey, you know, as as you know, I was a scientist running my laboratory. And so you're in charge of your lab, but you're training younger people to do things, and you're, you know, getting input. But, you know, rarely is there anybody in the group that knows as much as you and so you're, you know, almost a teacher and a mentor to everybody in the group. Then I set up a molecular diagnostics lab at St. Jude, this is almost 30 years ago. Again, I knew I knew what I was doing, right. And so I was able to assemble a bigger team, and able to set up that lab. But then I took over as chair of the department of pathology. I didn't know what I was doing. Right? There were aspects of the job. I had no experience. And I realized immediately, you know, I needed somebody to help me who could be a confidant and I needed to open up to learn on a constant basis from everybody. And, you know, it wasn't that hard because I knew, I didn't know everything, and

I knew they knew I didn't know everything, and so I couldn't fool them. And so, you know, who would I be fooling pretending I knew everything. So, you know, open it up and take the ideas. You know, and then as I became CEO, it just was essential to hire people who complimented my skills, and weren't afraid to challenge me and would challenge me, you know, all the time. And, you know, my job became more, raising the questions that would get them to jail. ONJ me, you know, and then I will say, you know, I sort of say, you know, be careful in hiring consultants and think through your own but an example where we hired a consultant that actually opened our eyes even more was this part of our strategic planning process, we we set out the problems, and then we establish workgroups across different streams of workforce and operation. So 150 plus people are involved, and they develop many ideas. And one of the ideas that floated through that, and actually came up through a blue sky process that we have, was to develop sort of a town square setup for our patients and families where they could come to a town square and get what they need coffee, you know, church services, school, bank, post office, you know, that kind of stuff. And, you know, and so we thought, well, we know our patients better than anybody. We've been dealing with them for 60 years, we know what a town square is, we've got lots of them down here in the south. And so let's just build it. And I decided, no, we're gonna hire a design thinking firm, IDEO, and we're gonna get them to come in. And let's just see what their process yields. And it was eye opening. And what it was, was that, you know, when we go into interactions with our patients, or their families, or their siblings, or our staff, we have all kinds of biases from our position, and our history. And so we filter what they say, we listen to what we want to listen to, we don't listen to what they might actually be saying. And so IDEO came in with a team, and met with different groups of families that were in different services, you know, might have been brain tumor, or leukemia or solid tumor met with, you know, parts of the support services for our patients. And they came forward with ideas that never even occurred to us, I mean, just completely novel, you know, saying that this space, yes, it can be a town square, but it can actually address problems that the families have, that you're not even aware of. And we listened to him, they developed it, we just opened it in January, and it is unbelievable. And, you know, it provides an environment for patients and families that meet so many little obstacles they had throughout the day. And we thought, you know, we knew everything, but we didn't because we weren't, you know, we couldn't listen to them, because we're their doctors were their caregivers. Were the ones providing care. And, you know, we're not family relatives. And so it was really interesting to watch and learn through.

Jacob Morgan 23:01
You mentioned a blue sky process. What exactly is that? Because some people might be familiar with it. But I suspect that a lot of people watching and listening to this have no idea what a blue sky process actually is, how it works, what the purpose of it is.

Dr. James Downing 23:17
Yeah. So when we, you know, have a strategic plan for ours, it's just a six year cycle. So I mean, some people may say that's too long. But for biomedical research, even today, it's not too long. It takes a while to recruit people to establish programs to do research, et cetera. And so we have a six year plan, we have absolute defined goals for that plan, we develop goals. Every year, we meet every quarter, we see our progress against that. And we allocate resources based on the goals that we've laid out in the strategic plan. And they're very, very ambitious strategic plans across cancer, non malignant hematology infectious disease, you know, pediatric neurologic diseases across our workforce across

our global efforts. But as we develop that plan, and we then publish it, and everybody starts watching it, and we develop new goals every year, we realize there are new opportunities that are going to arise that are not in the plan. And these may be major new initiatives that just aren't in the plan. And so let's start what we call a blue sky process. Anybody can come forward with an idea that is significant, of significant impact either for our workforce, for our mission, or for our patients and families. And they can apply with a single sheet. You know, here's what my idea is. It goes and gets evaluated by a series of committees that look at it. So the first year we had 21 proposals that came in then none of them got funded. No, we evaluated them, some came forward. And in the end, none got funded. Thank you know, maybe the next year, three got funded, and then maybe six, and then three again, and then maybe 10. So it's a very open process, but very rigorous. You know, we evaluate, we want to know, you know, what's the problem? What's the opportunity, what's the cost, what's the impact going to be? But some of these have been game changers. So the Family Commons was a blue sky initiative, you know, and I think it was almost $40 million dollars in the end is what we spent, we had one that we call seeing the invisible and protein kinases. So this is a structural biology proposal to look at transient structures that exist in protein tyrosine kinases that are major targets of drug that have developed in precision medicine. And, you know, our chair of structural biology was able to show on a single kinase that he could detect new transient structures that gave us mechanistic insights into how drug resistance occurred. And so he did it on one kinases. And we said, Well, what about doing it on all? And you know, you could see him first think that's crazy, and then think, well, wait a minute, maybe we could do that. And so he put forward to blue sky, it got evaluated internal, we assembled an expert panel from outside the institution. And then we funded it at $50 million, over six years. And it's going to be a game changer. In our pediatric neurology initiative was a blue sky proposal. You know, so there's a number of big ones like that, and there are others that are much smaller, but still impactful. And so it is a process that gets everyone excited. It brings new ideas to the table that are fresh ideas that are coming up every year. But you have to be incredibly rigorous. You know, again, is it really important is it going to have lasting impact. And I'll say one of the blue sky initiatives just to show the the expanse of this, well, two, actually three. So one was our St. Jude cloud, which is a cloud with Microsoft and DNA Nexus that has all of our genomic data. And so it's the largest repository of pediatric cancer data in the world. And that came out of a blue sky, we have one we announced last December. In our global efforts, one of the obstacles is patients can't get access to get quality drugs in for cancer treatment. So can we provide those drugs free of charge, and so 200 million to this program, we announced last December in collaboration with the World Health Organization, we're negotiating with UNICEF to be part of it, and will provide drugs free of costs, ultimately, to 50, low and middle income countries. So game changing efforts that come out of this blue sky that just weren't relevant at the time, we developed that strategic plan two years ago now, but they come up now. So you know, a way of constantly searching for new ideas for new innovation, and providing a platform to do it with money set aside, so that it's not disrupting the business practices you've already set up. And in everyone can participate, you know, from faculty to staff across the institution. So it brings great, I think excitement, it brings people at all levels thinking I can contribute to the overall mission of this organization. So don't get disappointed when it gets turned down. But those with a little bit of persistence. I've seen projects get turned down three years in a row and eventually get funded. So

Jacob Morgan 29:00

I like this. It's kind of like an internal incubator. And I think we've seen companies do that over the years. I think 18 T used to have that I think Adobe had that I think LinkedIn had that. Maybe they still do. Where are the ideas that any employee using a piece of technology can come forward pitch an idea employees can talk about it, maybe even vote on it, when employees have to pitch and present the idea to a team of leaders to get approval. And I think what Adobe used to do is they used to give somebody they they used to give all these ideas, a very small amount of funding, maybe like a couple 100 bucks just to build a prototype, like do a little bit with it. And then after that, they would decide if it's something that is worth funding and turning into something more substantial. So it seems like you have that kind of a process internally, which has been very useful for your employees. It empowers everybody to speak up and come forward in at least at your company. It sounds like it led to some very massive transformations and investments and I'm assuming probably See some small things here? And there too, right?

Dr. James Downing 30:02
Oh, yeah, yeah, we have, you know, a variety of smaller projects that people have gotten excited by, and, you know, they go forward, you know, just creating an archive of the St. Jude history was one that came forward this year, and it's gonna get funded, you know, and we'll go forward. Cool. No, we, you know, we, everyone's got a job and got a responsibility. And so it's not like they can go in the incubator space and spend time doing this, it's really, that's why we're very careful on the decisions on whether to go forward or not. And then we'll provide the resources to actually move it forward. And so you know, sometimes the evaluation takes well over a year. But that's okay. You know, it helps refine the project and allows it to move forward.

Jacob Morgan 30:51
This episode is made possible by AvePoint. No matter how big your business is, or what it does. AvePoint can help modernize your digital workplace by rapidly reducing costs, improving productivity, and protecting your data with the renowned confidence platform, go to av point.com. today for a free consultation. And to learn more, that's a V p o i n t.com avepoint.com. So getting to the the next step of how you approach a problem. So you kind of go through this process, you have this incubator, you have blue sky, I suppose the next logical step would be to test ideas or to test solutions, before maybe going all in into those ideas and putting in a lot of money. I mean, you mentioned some pretty big investments into some projects, whether it was the one with IDEO, you mentioned $200 million into making some of these drugs free. But with such a large investment, there's obviously always a downside, right, that maybe like what happens if you would have put that $200 million? And realize like, there's, there's no way to do that? Like, how do you test to see if that's something that you can actually do?

Dr. James Downing 32:04
Yeah, it's a good question. And, you know, what we typically do on on all these larger funded projects, is, we want a phased approach. Right? And so, you know, in the, you know, the global platform for access to childhood cancer medicines, it's 200 million, I think over six years, you know, but the first phase was really a deep risk analysis, and, you know, setting up of a secretariat within the incubator space and the World Health Organization, what would that look like? What would the governance of that be, and it's acceptable to the World Health Organization and to St. Jude, as the two principal

founders of this, and so, you know, that involved? It did involve, you know, experts from that set up the Global Fund a variety of other people, you know, the next phase, or part of that initial phase development phase was also what would a, how would we get drugs? And so what, what would a procurement agency look like? Who are the players in that space? And how would we select them. So again, different experts participating in that process, then, once we buy in, okay, so we're going to set up this Secretariat, we are going to set up a contract with this procurement agency, we know what the drugs are, we're going to buy now who are the initial pilot countries. So we'll start with six countries, I'll eventually go to 50. But we're just going to start with six. And so then there's a rigorous process on how to choose those six countries, and, you know, talking to their government, talking to the physicians within there, making sure that they're participating in the St. Jude Global Alliance, so we know the level of care, you know, then we'll bring them on campus. And that will occur in April and have logistics meeting, you know, how's it actually going to work in the port of entry and into the last mile into the hospitals, okay, you don't until sometime in December, we'll finally announce those pilot, then it'll grow a next phase slightly larger, and then ultimately, at the end will be those 50. And then at the end of that, we will open it up to external funding, because it will need additional funding to grow. So that all low and middle income, so each project, even the protein kinase was sort of phased in. One before all of this was the pediatric cancer genome project. And it was pilots, you know, not not pilots, but phase one, you're not going to phase two unless phase one is actually shows yield and productivity, you know, is returning what you think it's going to return. But I've been surprised the vast majority of these have actually worked. And, you know, part of it is the rigorous evaluation upfront, you know, if you they don't have preliminary data, especially on the biologic or experimental ones, we're not going to go forward, you know, we want, we want to see that there's a high likelihood that the basic concept is going to work. And so now, okay, let's apply it to this, or let's scale it to a level that's never been thought of before.

Jacob Morgan 35:18
It seems like the phased approach is the best, the best way to do it, it's safe. And I suppose if something happens during one of those phases, then you can kind of take a step back and decide if there's a solution. Or if you need to reevaluate or kind of kill the project or whatnot. So it's not, it's not like a all in all at once. Investment, you kind of take it in these in these steps, which I think is is obviously very, very smart.

Dr. James Downing 35:43
And another way that we foster innovation or expanded efforts at St. Jude is something we call our research collaboratives. Okay, and so this is not sort of incubators stuff on campus. This is the basic concept was, Are there gaps in our knowledge that we think need to be filled, for us to be able to make progress against pediatric catastrophic diseases? And if we can identify what those gaps are? Do we have the expertise to address that gap? Or not? And if not, can we assemble a team of experts from around the world to actually address it? And so we call these research collaboratives. And so we've funded six of these now. And the last one we haven't announced yet, but you know, it is sort of the gap is identified by faculty here at St. Jude, okay. They assemble a team of world experts together, usually one or two more investigators at St. Jude, and two or three investigators from anywhere in the world. And they put together a proposal that, again, comes to a review committee, the review committee takes a look at it early on and says yeah, we want to see a real proposal, they come with a real proposal.

Typically in grant funding, you'd have to submit this and maybe six to eight months later, you'd find out if you got in, there might be a site visit. We you know, during COVID had them present online. And, you know, within a couple hours, we say funded or not funded. Yeah. So they're relatively quick decision. But these are, you know, significant dollars, you know, more than $10 million, over the course of each of these projects, think $110 million dedicated to this research collaborative program over the next five years. You know, and just incredible work. And so the last one involved, you know, Nobel laureate from Duke, you know, two professors from Stanford, you know, from a couple from St. Jude, we've got investigators from Mass General Dana Farber. Yeah, great people from all over coming together. But again, ideas that come up, and are worth investing in.

Jacob Morgan 38:08
Yeah. Have you ever had any of these ideas fail, so something you've invested in and either I don't know, ended up not working or setting up something happening, because I'm assuming not every idea you invest in is always going to turn out perfect. So any, any failures or stories that you can share from those?

Dr. James Downing 38:30
Yeah, I mean, I think, you know, not so much a failure of judgment by any individual or even an organization, but a failure of a technology to actually produce the way one thinks it would be. It would produce so you know, new technology comes out, because now it's gonna solve all these problems, and you end up investing in it, and it doesn't, right. And so, you know, for us, our early foray into drug development, you know, there was a push for high throughput screening and large libraries, and, you know, you throw them at your target, and you're gonna find lead compounds, you'll be able to modify them and develop a lead compound and push that forward to be a drug. And most of that went nowhere. You know, almost nothing came out of that, you know, it's a bad target or a bad lead or, you know, insufficient opportunities to move it through chemistry or not, not that many people actually interested in even doing it after a while. And so there are these high throughput screening facilities all over the country and every major institution, most of them ended up shutting up, you know, they just shut down because they didn't yield what people thought they were going to yield. They yield a lot of noise. So I think those were, you know, the question is what less than can you learn from that? Right? Because it was a failure. It wasn't a judgment failure at the time. It's just the the concepts didn't really yield what you thought they were going to use. Yeah. And, you know, part of it is, you know, there's one idea in biomedical sciences, if you build a resource they'll come. That's just not true. Right? Unless they have a need and a use for it, they're not going to use it. And so you need to be careful, because you can build the big resources, and nobody uses it. Now. It'll be a waste of time.

Jacob Morgan 40:34
Yeah. Yeah. No, that's, that's interesting. And I like the idea of you. I mean, you should ideally turn everything into a learning moment, right? All the all the failures that you have, and,

Dr. James Downing 40:45
you know, I'm trained as a I'm a human animal pathologist. Right. And so I'm trained as a pathologist doing autopsies, you know, and so, it's that concept, you need to do an autopsy, you know, what went wrong here? Yeah. And walk you in, you learn, I even remember, in college not doing, you know, not

getting a perfect score to test or something. And then it was okay. What did I do wrong? How did I miss this question? Was I prepared? Did I skip over the, you know, did I go through too, you know, and what can I do differently next time? And I think, you know, in any, any decision, they're always it's never a unanimous success, right? There's always some failures billet built in there. And it's better not to just, you know, sort of brush those off, it's better to stop and think about, and what can I learn from those?

Jacob Morgan 41:38
I was wondering if the scientific method you think can be applied to the business world. So for people who are not familiar, the science with the scientific method, actually pull it up here? Because I knew I would forget it. So to find a question to investigate, make predictions, gather data, analyze the data and draw conclusions. I think those are the five steps there.

Dr. James Downing 41:58
Yeah. Do you think of six? And I'll tell you the difference.

Jacob Morgan 42:02
Oh, okay. So I guess first is, I'd love to hear your six steps. And second, is this approach only applicable to the science world? Or can business leaders who are running companies that have absolutely nothing to do with anything related to health or medicine? Can they use this approach as well to solve their business problems?

Dr. James Downing 42:23
Yes, I think it can be used for everything. You know, I think it is a way of thinking clearly. And so I think, as I said, the problem, what's the problem? And you got to be crystal clear on that you, you would not believe how many times a scientist is unclear on it. You know, we see young scientists all the time. And, you know, well, I'm working on this, it's like, I don't think you are, you know, what, stop and really think about what the problem is you should be working on, based on this loose idea you just put out to us. So, you know, making that problem as clear as possible. You then, you know, what is your hypothesis? You know, do you think it's caused by this? Or that? And then you set up experiments to acquire data, you know, is this true or that true? You draw conclusions, you analyze that data, you draw conclusions, but you're not done. You know, and this is the last step that is an in there, but it's the most important step, when you have a conclusion that you think explains, you know, what your problem was, or what what the solution is to your problem, you then stop and do everything you can to prove that conclusion wrong. And if you can't, it might be right. But most people just take it as right. And they then go to the next step, and they end up building houses of cards. And so I like to tell all young scientists, you can never be too rigorous. You need to spend your energy. Once you see you've got a result. Don't trust it, do everything you can to prove it wrong. And if you can't, okay, you can feel a little more confident. But there's so many results out there, both in business or in decision making, or in science that right or wrong and you you didn't spend that extra energy, that extra discipline to prove that it really is right, because you can't prove it wrong. Yeah. So I think that's important. And so, you know, you can't go forever, you got to eventually make decisions in business. But be careful, right? You know, when you say I should go this way, nothing wrong with sleeping on it and coming back and asking again, well, what might be the worst possible scenarios that would make this outcome? Completely unlikely? And then how likely are those scenarios, right? I mean, how can this go bad? And you know, think through

that and you may realize, Oh, my God, these two are likely to occur. So my Decision down the stream might be very bad. And now let me rethinking?

Jacob Morgan 45:03
Is there such thing as a good idea or a bad idea? Or are all ideas good until proven otherwise?

Dr. James Downing 45:12
Yeah, I'm, you know, I think it's probably better to be open that all ideas are just ideas, and do they fit into the temporal, spatial, you know, framework, at that time to be good or bad or doable, or not doable? I think it's a bit dismissive to Sasha, it's a bad idea, and we're not going to pursue it. I know, many times I've done that, you know, that's just crazy. Why are we doing this? I'm gonna fight this. And then it's like, well stop. Might you learn something from that? You know, and it's sort of like, yeah, just let it go. You know, I may actually learn something from that idea. So, you know, I think it's better to watch them play out. And they'll declare themselves, right, whether it's a good or bad idea.

Jacob Morgan 46:02
How important is the idea of or this concept of measures of success? You know, this, this gets talked a lot about in the business world, right? How do you know if an idea is going to be successful or not, or a product or service will be successful or not? Is that an important step? And what do you do? If you just don't know? Right? I mean, sometimes you're investing in a new product or service in a new area. And it's really hard to know what the potential is what you could do, what KPIs you should be looking at. Where does that fit into this whole process of solving problems? And coming up with ideas?

Dr. James Downing 46:42
Yeah, you know, I think there, there are different ways to think about it, or perhaps different ways to approach specific problems, there are certain problems or business lines and you, you have to define success, and you have to have a certain confidence, you're going to meet that success, or you shouldn't be investing or, or allocating resources to that, if the end result is you need return on that investment, whether it be in advance and cures or revenue coming back to support the institution. So those become, you know, more traditional, right, you can set KPIs, you got to be careful what you're measuring? Are you measuring what you really want to measure? Are you chasing some number, that's not going to tell you, I think there are other initiatives and other problems you're trying to address where it is much more open ended. And so in the research world, it is much more open ended. And so people often say, you know, there's a whole movement now that we need more directed research will be, you know, DARPA, like setups where you know, innovative things that have a practical value to, you know, advance, you know, science, so it has human application. No, you need a lot more undirected research, that's curiosity based, and that's generating nothing but knowledge, because you have no idea where that knowledge is going to go and so on those, you can't have clear definitions of success or impact or return on investment. You know, it's like Bell Labs, you know, from a TMT they just said, Just go work on things, right. And they weren't, they didn't care, they didn't know if it would transform, telecommunication, or whether it would just make it more efficient, but they invested. And it was one of the most productive, you know, research laboratories ever developed, you know, the ideas and Nobel prizes that came out of there transformed, you know, the technology world and the future, you know, with the developments that came out of there. And so, you know, in a research and

development, you got to have research, you got to have investigator initiated undirected research. So we have that at St. Jude that's not related to any diseases that we work on. It's not mission focus. It's not, you know, on this disease or that disease, it's understanding fundamental biology generates technology. It generates knowledge, it generates insights, that influence how we approach diseases and influences the entire culture that we have.

Jacob Morgan 49:25
Last few minutes, I want to spend some time talking about how to connect people to the work they do. I was certainly struck when I was visiting St. Jude Children's Research Hospital, just the passion and the connection that everybody had to the work that they were doing. Now, some people might look at that and say, well, it's easy for them to do it. They're obviously you know, they have a big goal and a big mission. They're helping children, they're helping save lives. My company doesn't have that, you know, we're not saving lives. We're not curing cancer or trying to trying to cure cancer. So how, how do you Read that connection with people and the work that they do and the organization if it's not kind of inherent already in the work that they do, like your company. My conversation with Dr. James downing continues, and you're not going to want to miss this one. It's available only for paid subscribers. So you can head over to great leadership.substack.com And subscribe over there. And in this bonus episode, and we're calling all of these bonus episodes now the leaders toolkit their 1520 minute bonus episodes. So in this particular one with Dr. James Downing, we go in depth on how to connect people to the work that they do. So we talked about that for around 15 minutes. We also talked about the importance of telling stories, and how and why leaders should be telling stories, subscribers are going to get access to one of these episodes every week, you're going to get access to a weekly in depth article that I personally write, where I talk about a leadership trend, a piece of insight, research, discovery, something along those lines. And you're also going to get access to a weekly five minute leadership hack where I share a leadership strategy that a CEO has shared with me, those are called The Five Minute leader, all three of those things available for subscribers. Again, that is great leadership.substack.com It's less than the cost of two lattes, it'll be the best investment you can make in your professional and leadership development. So I hope to see you over there again. That's great leadership.substack.com I'll see you next week.