Mark Zuckerberg, Tyler Cowen, Patrick Collison Transcribed 11/22/19 00:00:00

Mark: Hey, everyone, and welcome to the next in our series of discussions on the internet and technology and progress and some of the social issues that we face. We've done a number of these this year focused on topics ranging from regulation to journalism to biomedical research. And today what we're gonna focus on--a discussion on what progress is itself and how we might study it and what academic work is already going on in the space and what we might think about to look at examples from the past to determine how we can make more progress for humanity going forward. So today joining me is Tyler Cowen who studies economics at George Mason University and is also the coauthor of the popular blog Marginal Revolution, and Patrick Collison, who's the cofounder and CEO of Stripe which is a pretty amazing company that does--that basically does payments and economic infrastructure for the internet. So, you know, we've been talking about these topics for...for a while now. I mean, this is something that you guys have both studied in a lot of depth. And you recently wrote an op-ed together--I think it was in the "Atlantic"--about how we might have...a new or different approach for studying the nature of progress. And in order to kinda mine historical examples to figure out how we can make more progress in the future. I think it'd probably be interesting just to start off by, you know, hearing how you're thinking about it and the basically summary and what feedback you've gotten on the piece that you wrote.

Patrick: Sure. So I think that one of the most important facts in the world and the history of civilization to date is that the rate of progress has not been constant. Right? If you look at what happened in the world say between 0 and 1700, 1800, thereabouts... the rate of progress, by any major metric in terms of average income or average life expectancy or infant mortality, any of these measures, it was either constant or only improving at a very slow rate. And then something happened, something changed around 1700, 1750, the Industrial Revolution, the Enlightenment, the advent of something approximating modern science. Once that happened, so many things started to get better together. Incomes improved, life expectancies increased, we started to discover really fundamental knowledge about the world, we started to invent really important new technologies. And these things, over the last couple of centuries, really diffused around the world. So that's interesting and important, and the intuition, I think, and the thing that has been a focus of both of ours for the past couple years, is thinking about, well, we transitioned from this regime where we weren't making much progress to one where we have been making much more. Is this the best we can do? Or is there something that looks, compared to status quo today, so much better again that it's like a status quo ex ante before the Industrial Revolution. And as you look around the world today, on the one hand, we see the tremendous importance of the progress that we are generating, and that, for example, the number of people in extreme poverty has declined by more than a billion people since I was born. But, on the other hand, there's a lot of suggestive evidence that maybe we aren't as effective at generating progress today as we have been in the past. So, for example, if you look at the U.S., productivity growth mid-century or say between, 1920, 1970, was maybe about 1.9% a year. Now, most economists think it's much lower. Maybe around .4% a year, something like that. So we're at least by economic measures generating progress more slowly than we used to be. Whatever the rate at which we're making progress or figuring out ways to do things better today, whatever that absolute level is, it would be much better if we were doing it more effectively. If we were able to solve the most important problems that face us today in 50 years and 100 years rather than 500 years or a thousand years. So the meta question we're really interested in is: how does progress happen, how do we discover useful knowledge, how is that diffused, and how can we do it better?

Tyler: It's important to understand, I think, how much this is an invisible crisis. So if you have a growth rate that is 1 percentage point lower, over the course of a bit more than a century, you could have been three times richer with a higher growth rate. That would be something like the difference between the United States today and Mexico. So by having a lower rate of productivity growth, in no given year does it feel that bad, but two, three generations later, you're much worse off, it's harder to pay off your debits, harder to solve climate change, harder to address a whole host of problems.

Mark: Yeah, so before we kinda dive into, you know, how we can improve this, you know, what do you say to the people who question whether all this progress is positive? I mean, certainly as we make progress in one area, it creates issues in other areas, and that's been a big topic that, you know, I focused on in my work at Facebook over the last few years and a lot of these challenge discussions. But how does that fit into the overall framework of what you're studying and, uh, this discipline here?

Tyler: I don't think economic growth is always a positive, but the world and America has serious problems. I would rather address those problems with more resources rather than fewer, whether it is paying off our debts, addressing climate change, fixing global poverty. And knowledge matters too. So there's a recent paper by Ester Duflo and Abhijit Banerjee and they find if you give foreign aid combined with coaching, the rate of return to that intervention is maybe 100 to 400%. And that may or may not be true, but what I would like to see is a world where everyone is obsessing over that claim, over that debate, working very hard to figure out that it's true, that should be on the front page. People should be talking about it, you know, calling up their siblings, my goodness, I just read this, what are we gonna do? Do you agree or not?

Patrick: Yeah, and look, while again, I think it's unequivocally the case that certain kinds of progress in certain places that to a certain extent can have harms and externalities and all the rest, and a really important part of progress is figuring out how do we mitigate those, how do we solve them and so on. I think climate change is probably the foremost global example today. But I think it's really important--or it is easy for us sitting here in the Bay Area in California, I think, to undervalue the prosperity and the kind of wealth we've been able to generate over the past couple, again, hundred years. Since I was born, for example, global life expectancy has increased by about six years, and infant mortality has fallen by more than 50%. I mentioned the statistic of the number of people who have left extreme poverty. This is incredibly important, right? And so I think there's… we're not the first people to say it, but there is a moral imperative to this kind of progress, and we shouldn't lose sight of that fact.

Mark: Yeah, I agree. I just--I think it's important--You know, a lot of these things are not uniform and, I mean, you know from running a company that, you know, when you look at averages and anything that hides a lot of issues. Your example on the rates of poverty going down I think is an interesting one in this because, you know, what a lot of people don't particularly wanna talk about these days is that most of the benefit of people coming out of poverty has happened in China, and a lot of other places around the world, in some places, poverty has actually increased. So it's--You know, it's...I generally agree with the premise of--of--And I think studying this stuff will generally help us to make more progress in those places. I mean, that may be a good example because perhaps looking at some of the examples of what has done well in China could be applied to other places where there have been issues. But before we dive into the discussion on this, I just wanted to make sure that we didn't, you know, cover this is a way that comes across as if, like every step forward comes without a cost. And I'm sure as we talk through the different examples, I mean, that'll come up as well. 00:08:24

Patrick: Yeah, and we should emphasize that when we talk about the phenomenon of "progress," I think GDP or GDP per capita is a pretty good first approximation measure of it and it correlates strongly with many of the things we care about. But they're definitely not the same thing. I think an important question for anybody interested in this area to think about is, well, how should we define progress, right? And what are the better and worse kinds of it. Again, in GDP, we have a relatively effective metric we can use across countries, but, there already is interesting work on what might better measures be, and I think that's really important to study.

Tyler: Well, let's say you want to improve the lot of people in West Virginia. One growth-enhancing way of doing that is to make it easier to build, say, in Washington D.C. and the Bay Area. Right now, to move from West Virginia, say, to Menlo Park, it's extraordinarily expensive. You can't just pick up and show up here and hope to get a job washing dishes the way one might have done in America 50 years ago. So by having more building, more economic growth, also more GDP, it would increase more opportunity. So economic growth and opportunity--they do tend to be correlated, and sometimes the problem is we don't have enough growth, not that we have too much.

Mark: Mm-hmm.

Patrick: And look, not to hammer this point too strongly, but you did invite the two people who wrote the piece for our progress here, and--

Mark: Yeah, and I wanna spend most of the time actually talking about that. I just wanted to make sure that we hit that up front. So what are you, um...when you're talking about--You know, there are a lot of people who already are studying this in different ways, right? They're historians, economists. When you're thinking about what the field is, when you're talking about trying to create a new science of studying progress, what more do you think needs to get done, or what do you envision on that? I mean, I know you have a fund that you've put together, Emerging Ventures. 00:10:15

Tyler: Emergent Ventures.

Mark: And where you're basically finding academics who are studying examples of where there's--of progress in the past to start this field. But what does this kind of add up to? How do you--What form does this take over time?

Tyler: One view of mine is that not enough philanthropy is long-term oriented. In this regard, I've been influenced by your Chan Zuckerberg Initiative. And also in philanthropy, there are too many choke points that can say no, so foundations become their own bureaucracies. They become very risk-adverse. So Emergent Ventures is a new kind of philanthropy. There's one layer of yes or no. People are encouraged to apply. If the payoff is 30, 40 years down the road, the attitude is, "Great," Take a lot of chances. I'll worry about getting some [indistinct] and some risk and not expecting the median project to be something that necessarily looks good when taken to a board. So that's one way that thinking in terms of progress helps us restructure at the micro level particular decisions we're making.

Mark: Yeah.

Patrick: Yeah, and so...strongly agree that there's a lot of really important work already happening across multiple disciplines that is relevant to these questions. And part of--Like the idea of there being a new science of progress--that's not quite--that was the headline placed on the article but not exactly what we're saying. What we're arguing is that the work that's already happening should be receiving more attention and there should be much more of us. And just to give a couple of quick examples so that there's strongly suggested evidence that we can teach management practices so people can run firms more effectively, right? So there are a couple of studies on this. There's a good one from some folks at Stanford that did a randomized trial in India. And there's a really neat one that came out, I think, last year from Michela Giorcelli looking at firms in Italy and showing that like over 15 years after, again, a management training program with some natural randomization, that again, those firms were employing more people, paying more wages, being more successful. And another randomized trial in Mexico conducted over the past couple years, again, 600 firms, ensuring that just teaching better management practices actually makes those companies much better off. 00:12:31 If that's true, that's amazing low-hanging fruit, right? We should be investing much more in this area. We should be figuring out which kinds of management training work better or worse than others, is this generalized to all countries, how can we actually implement and execute this in the world more broadly? So that's one. Second is, Tyler mentioned this point about geographic mobility. When you think about ‘how do we grow GDP?’ or ‘how do we generate progress?’ maybe housing policy is not the first thing we're naturally drawn to thinking about. However, if you look at the world in, say, the USA in 1980, 40% of people, when they took a new job, they moved somewhere else. So those things went together much more. If you look last year, about one in ten people moved when they took a new job. So within the U.S., geographic mobility really declined. That is in large part because the costs of movement have enormously increased as housing costs have increased especially in our most productive regions over the past couple decades. Now if you look into that more closely, again, there are economists who've been studying these questions quite closely for the past couple of years. These two guys Hsieh and Moretti published a paper, an updated version of a previous paper, this summer claiming--Putting forward a model showing that if you...if you look at the zoning restrictions that existed in the Bay Area and New York between 1964 and 2009, and you imagine a counterfactual world where there was much more supply elasticity in these places, we built way more homes here in the Bay Area and in New York, and in that counterfactual world, average U.S. income would be, in their model, $3,700 per person hired. Again, not just for people in those places, but across the country, right? That's a huge effect size. And so again, we should be studying these questions much more closely and we should be figuring out, okay, well, if that's true, what are the policy prescriptions? How do we actually go act upon that? It's amazing low-hanging fruit. And then to give a third one, as those two examples show, funding science is incredibly important. But there's surprisingly little work about how we should be funding science and how could we do that most effectively. And actually, beneath the surface, it's been changing a tremendous amount here in the U.S. over the past couple of decades, and there are important policy questions that is that a good thing? So for example, in 1980, 12x more dollars--The NIH spent 12x more dollars on researchers under 40 than researchers over 50. So they predominantly funded younger people. Today they spend 5x more dollars on people over 50 than under 40. And so it's really inverted--it's gone from primarily funding these young investigators to this kind of gerontocracy where they’re funding older scientists. Maybe that's good, maybe that's bad, I don't know, but that seems like a very important question to answer. And so part of our point in arguing for progress studies is when you really look at the expansive version of all the different things that can influence our ability to discover new useful knowledge to generate economic growth, the set of questions is super-broad, and we should be trying to synthesize this effectively.

Mark: Yeah. So let's go deep on medical research here for a second, because this is an area that you wrote this paper about before about how the progress in the field might be slowing. And like you mentioned, The Chan Zuckerberg Initiative, the philanthropy that I run with my wife, I mean, a big focus of it is on medical research and trying to--You know, we have this aspirational goal that we wanna help build tools that can help scientists cure, prevent, or manage all diseases by the end of this century. And basically, the math of how you get there is, you know, starting about 100 years ago, call it, you know, there was really this uptick in medical research where we started doing randomized control experiments, treating it more like an experimental science. Since around that time, the average life expectancy has increased by 1/4 of a year every year, relatively [indistinct]. There's no guarantee, of course, that that continues, but if we're able to have that continue, then that would imply that by the end of the century we will generally have had to have either cure, prevent, or be able to manage most if not all of the diseases that we're aware of now. So there's some trend that suggests that this should be reasonable, and the approach that we're taking in the work at CZI is largely about building tools to help compound the rate of science. So and what we see is that, you know, like you mentioned, the government is the largest and most important funder of science and, you know, it basically funds the whole establishment of scientists across the country. But the grants tend to be very...very spread out across a lot of people. They're not typically put into kind of big infrastructure projects. And that's the niche that we felt through CZI that we can maybe help to fill is, you know, investing instead of, you know, a million dollars in a lab, put $100 million or a couple hundred million dollars over time into building up really important scientific assets for the community. Like helping to fund scientists to go put together this Human Cell Atlas. It's almost like the-- 00:17:45 Kind of think about it as it's almost like the periodic table of elements but for biology of all the different kinds of cells in the human body. And the goal is just, you know, if you look throughout the history of science, at least, you know, most major scientific breakthroughs have been preceded by the invention of new tools that help people look at things in different ways. And so the theory is kind of similar to what you're going at of how do you increase the compounding rate of progress? But there are a couple of different directions that I think we could go in here, and one is I'm curious what you've seen in your studies in the space that suggest to you that the rate of progress is actually slowing. And I'm also curious--what are the examples that you've seen overall of how the science around studying progress would potentially lead to a different approach or different portfolio of how this kind of work gets done. So I don't know where you want to start with that, but there's a lot here to do.

Tyler: Here's what worries me, and it should worry you too. So as you mentioned, U.S. life expectancy is basically going up in linear fashion. But if you look at expenditures, we used to spend a few percentage points of GDP on healthcare, and now it's about 18%. So we've gone up to 18%, and we're not even boosting the rate. I'm not saying it's the fault of any one group of people, but something has gone wrong. There's some kind of last-mile problem. You can turn to the newspapers and read all kinds of fantastic stories--new research, new ideas, new tools--but when the rubber hits the road, people living longer, we're spending more and more and more for exactly the same returns. So if that trend continues--and you see a similar trend in many areas--also crop yields, feeding the world, other areas--the question becomes, you know, where does all the progress go? So the idea that you need to look at each structure and encourage more risk-taking, better decisions with the money, less bureaucratization--maybe in some cases more centralization, whatever it takes--but that there is this invisible crisis, and people are distracted by the headlines about [indistinct] or whatever. But actually what you get for the money--performance is so-so, I think. 00:19:50

Patrick: Yeah, and so what we wrote in this article a year ago about what's going on in science... if you look at it by the most macroscopic measures, right, like the number of PhDs in the U.S., like active PhDs has grown by---Actually, if you take all the macroscopic measures, they all have grown by about a factor between 50 and 100. Number of PhDs, number of papers published every year, just actual dollars going into science funding, and so on. So in a very stylized way, if you look at the first half of the twentieth century as compared to the second half, just way more input in the second half of the century. And again, not by 50%, but by orders of magnitude. And so then the question for all of us would be, well, in which half of the century did we get more out in terms of useful scientific knowledge? And whichever we think did better, to what degree? And, again, this is a very difficult question to answer. How do you weigh scientific knowledge? And so you have to look at it, I think, in various applied context. Like life expectancy or semiconductors or, as Tyler mentioned, crop yields, or whatever. And I think what's interesting and should be concerning is that for almost every conceivable applied measure, we seem to be getting, at best, constant returns. But that's really bad because we have exponentially increasing inputs and we've constant return outputs. That is almost by definition not a process that we can sustain. Now, there's two, I think, broad possibilities there. One is it's just getting intrinsically harder to generate progress and to discover these things. And, who knows, maybe some significant part of that is true. But the other possibility is it's somehow more institutional, right? It's more contingent, it's more sociological. And, again, we do have suggestive evidence that our institutions are....well, they're certainly older than they used to be, and they're also, as in the NIH funding example, there are changes happening beneath the surface and so on that may or may not be good. So I don't think we should write off the possibility that it's not inevitable, and that there is or that there do exist alternate forms of organization where things would work better. And again, if we, dig a little bit into the evidence there you see things like...there's a science funding program that, obviously, you're familiar with called HHMI, the Howard Hughes Medical Institute. They give grants along the lines of how CZI does where they're longer term, they're more open-ended and so on. Pierre Azoulay and my team wrote a paper a couple years ago in trying to look at, well, if you take ostensibly identical scientists some of whom receive HHMI grants, some of whom don't, how much more successful are the HHMI recipients? And he concluded they're about twice as likely to produce a top 1% paper by citation count. Again, that's really suggestive.

Mark: Top 1% if they do what?

Patrick: They're about twice as likely to produce a top 1% paper by citation count.

Mark: If they…

Patrick: Oh, say, if they receive an HHMI grant.

Mark: Well, that might be correlation, not causation.

Patrick: Yeah, so he tries--

Mark: They do get a lot of the best people.

Patrick: Yeah, yeah, yeah. So he tries to control for that, and uses a reasonable methodology for it, but some of it could totally be just that selection effect. But again, I think it's very suggestive that, hmm, maybe there are things we could do that would better enable this kind of discovery. And this might seem like a bit of a red herring, but I think it is suggestive that in many other domains where we can objectively assess progress, it's very clear that our productivity has fallen off a cliff and for reasons that we can be pretty sure are not that it's getting intrinsically harder. And so, for example, when New York decided to build the subway in 1900, they decided to build it. 4.7 years later, they opened 23 subway stations, and in 2019 dollars, they spent just over a billion dollars doing so. So, 23 stations, just over a billion dollars. When New York decided to build the Second Avenue subway in the year 2000, 17 years later they opened three stations and they spent $4 1/2 billion doing so. And so our productivity in subway construction has, at least in New York, decreased by a factor of 40. Here in the Bay Area, we decide to build the Golden Gate Bridge and the Bay Bridge starting in 1933. Both projects finished within four years, and to celebrate it, we decide to build man-made island, and we built that island in 18 months. 00:24:20 And, I haven't tried, but I would wager that if one tried to build a new island in San Francisco, it would be difficult to do so today in 18 months. And so--And I mean, California, you have high speed rail where... when France decided to build the TGV, its high speed rail, it opened the first line after five years. California started pursuing high speed rail 11 years ago. They forecast--we forecast--being finished in 2033. So we project a 25-year project, but of course, that's a projection. It'll probably end up being much longer. So this is the domain where it's hard to imagine that building infrastructure had gotten intrinsically harder, right? Like, the atoms aren't physically heavier than they used to be, right? And so clearly there's something institutional, sociological going on with infrastructure. Larry Summers talks about the idea of the “promiscuous distribution of the veto power” and it’s how much harder it is to get things done. Inasmuch as that's true, then there's the question of, well, have other institutions, have other progress-generating mechanisms in our society--have they also got less efficient? And if so, what can we do about it?

Mark: So as an aside, if you're watching this, Patrick collects these examples of, um, of historical projects that went fast and that you can't imagine how they went that fast. So if you Google his website, he has like a whole list of these that I think is pretty interesting and compelling when you go through all of them.

Patrick: Yeah, I think it's just important to understand...how effectively we as a species, how effectively we can do things when we're organized the right way. Humanity is pretty amazing. And when possibilities are unlocked, when efficacy is enabled, we can do great things.

Tyler: Sometimes it is a matter of actual will. So for the last 40 years, getting around for almost all Americans, it is slower. And before that, we had a period from 1800 say to 1970 when it got quicker and quicker and quicker. And now even flying in airplanes for most people is slower. Traffic is worse. Those are solvable problems. Manhattan should have congestion pricing and a stiffer form of it than they're likely to opt for. So the notion that people have lost the ability to imagine a future much different and much better than what they know to me is one of the most worrying aspects of where we are now.

Patrick: Yeah, and quantitatively, I mean, if you look at the percentage of Americans who think that their kids' lives will be better than theirs, that has been in monotonic decline--not strictly monotonic--but generally declining since World War II until, on an empirical basis, Americans are getting less hopeful about their futures, their kids' futures, and that's a really bad thing because it can be auto-catalyzing and a self-fulfilling prophecy.

Tyler: And we're supposed to be the most optimistic, forward-looking country. The data on France, how many people think their kids will be worse off--that's much more worrying yet. And there may be a self-fulfilling prophecy to this. If you think the future won't be so great, you'll invest less, you won't work as hard, you'll contract your risk-taking. And you end up with a kind of social and economic malaise. And, indeed, you see falling rates of economic growth in most of the Western world.

Mark: So I'm curious how you would think about going about and studying these kind of organizational changes. Going back to biomedical science, for example, just 'cause this is an area we do a lot of work in, you know, the woman who runs our--who runs CZI Science Initiative, Cori Bargmann she's a very renowned scientist, and she has this theory about that a lot of the granting process that NIH does--but also HHMI--it basically encourages very individualistic work, right? You give people grants, they work on their own, you're not incentivizing people to work together. People actually wanna work together, they wanna coordinate. And when I was talking about the Human Cell Atlas, you know, a lot of the issue there that needed to get dealt with was, you know, a lot of people were working on cell atlases for different parts of the body, the liver cell atlas, you know, whatever. But they were all in different data types and formats, so that way you couldn't compile a holistic thing. So a lot of what she did and the work of CZI was basically helping to coordinate, that way when these grants were given, everything--like the teams worked together, the data types were similar, so that way it all added up to a bigger thing. And that certainly seems like one of many theories that one could have for how you could organize this stuff better. But there's this question of how much of progress...whether that's something that one could have determined just through historical data versus this is the type of thing you need people or the government or foundations to go out and just run different experiments and see how this works. And I'm curious how you think about, in terms of studying this, how much this is like...this history and kind of history of science based on data that's already out there versus we should just try different models of things and encourage more creativity and more competition and try different things.

Tyler: It's striking to me, if you look at American universities, the list of the top places in 1920 and the list today--it's completely the same, except we've added on California. Otherwise, no change. Top 50 universities--if you look at--

Mark: It's very different companies.

Tyler: Of course, even from 1980, it's--

Mark: Decade over decade, the list of the top ten companies by market cap almost completely turns over.

Tyler: Procedures for tenure in the top 50 research universities--almost exactly the same. Whatever you think of those, there's something gone wrong in the sector. There's not enough experimentation with how you reward people. More schools should experiment with a different kind of tenure or reward people more on the basis of practical impact. And again, you might object to any particular solution, but the extent to which experimentation has died at the institutional level, to me, is striking. 00:30:19

Patrick: And to underscore that point, if you look at the top 25 universities in the world today per The Times's ranking. 7 of the top 25 are American universities that were started in a single 30-year period between 1861 and 1891. And if you look at where those universities come from and what were the people behind them thinking, they were very deliberately specifically reform minded.

Tyler: And progress-minded.

Patrick: Absolutely. They thought well, obviously, academic institutions exist. Harvard, Yale, and so on, were already around. But they saw the success of German research university model, they saw the possibilities of the U.S., and they saw at least what they thought was required for the future. And they very deliberately decided, "We will try something different." And again, that yielded now 7 of the top 25 universities today. So I think it strongly empirically underscores the value of the kind of experimentation you're talking about. And I fully agree. I think we should be historically informed but ultimately a certain amount of commitment, decision, and just willingness to experiment is going to be required. The other thing I think your point with--so the teams [indistinct] is there are these really, uh...thought-provoking examples of just like productive cultures through history, right? Look at Vienna, 1880 to 1940 or something. You have in so many different fields you have people who do this incredibly informative work. Klimt, and you had Mahler, and you had Mach in physics, and you had, of course, Austrian economics and von Mises and Hayek and all the rest and you had Freud and you had Wittgenstein. Vienna was amazing in this period. And when you dig into the specific stories you realize a lot of these people knew each other and they were inspired by each other. They give credit to each other for, again, across multiple disciplines, different parts of their thinking. Or if you look at Edinburgh during the Scottish Enlightenment. Again, a tiny place. Edinburgh, at the time, in 1780, was the size of Santa Cruz, right? And yet you get modern economics from Smith, you have Hume, you have, the birth of modern geology, amazing literature, poetry and so on. And so clearly there was something excellent in Edinburgh in 1780 that was not there in Dublin in 1780. And I think obviously, it's hard to pin down, like what was that, but at the same time, the difficulty in defining it doesn't mean it wasn't there or it's not important.

Tyler: I would say this. I'm sitting here with two university dropouts. That's notable to me. The Bay Area is our modern Vienna, you know. Bravo to the Bay Area. But we're not working nearly hard enough to build other new Viennas and other places. And I don't really think it's quite Manhattan anymore. It's a wonderful city and amazing place to go, but it is not a world leader for ideas in the way in it was, say, in the 1920s through the 1980s.

Mark: So...people study this, right? I mean, so what would have been the main things that people have learned so far from studying Vienna or Edinburgh?

Patrick: Well...I don't think there's a rich literature of lessons from those places. Obviously, lots has been written about them. There are great historical accounts. I've enjoyed reading them. But… well, it's an intrinsically very difficult thing to do to figure out, well, which things causally mattered. And these things--There's a certain degree to which they might be over-determined. And it's very hard--You don't have counterfactuals. Obviously, you can't run trials, and so I think it is a very difficult question to answer. And I think for understandable reasons, people studying these questions are reluctant to take definitive stances that "this is what mattered" in 1900 in Vienna.

Tyler: But one lesson I would say--the Scottish islands, people moved to Edinburgh, right? Vienna, you have Jews coming in from the Pella settlement. The Bay Area, people coming from all over the world, and indeed, you're from Ireland. So immigration--immigration is not a guarantee of things going well. But the bringing together of different ideas and cultures and the new clash of opposing perspectives has been correlated with a lot of these Viennas in the world history.

Patrick: Very true, although I'm pretty sure that in the 1900s, Paris had more foreigners than Vienna. I think it was like 2% in Vienna, so-- 00:35:04

Tyler: If you go from the Scottish islands to Edinburgh in 1740, that's a huge difference. It's a bigger difference than maybe, you know, Mexico to Los Angeles today.

Mark: So if you're thinking about what kind of work to fund in terms of studying historical progress, what's your framework for figuring out where to even begin studying? 'Cause I mean, what you're talking about here is basically studying the economic and scientific result of immigration, which is obviously a massively socially important debate that's at the center of a lot of political debates and has been for a long time. So, you know, from one perspective, it would be very--It's surprising that it wouldn't have been studied in more detail to understand the impact of it. But that's very different from kind of the biomedical science-type stuff that we were talking about a second ago. Do you have a framework in your head for how you...would you think about or prioritizing study in different areas, or is it mostly just about finding really sharp people who have new ideas and funding them to do different kinds or work? How do you think about that overall?

Tyler: People who are curious. People who have bold ambitions. People who have what I call stamina--they just don't even stop. People who are working in productive small groups that maybe through WhatsApp, in fact, or it could be their next door neighbors, their colleagues at a university. When those, say, four items come together, then I think you have possibly what is a very good funding decision, and I would take a lot of chances on those people, not worry too much about the micromanaging, and let talent rip and let groups form and see what happens.
Mark: Got it. So it's very much like entrepreneurship in that way. You're betting on the person more than the idea--

Tyler: But also the vision, right? There has to be a vision, and there are plenty of successful entrepreneurs who are not curious. So for intellectual progress, to really put curiosity very highly is part of my philosophy. 00:37:04

Patrick: On the one hand, not only do we acknowledge that an immense amount of very important, insightful work, has already been created, and it's that work that, to a large degree has, I think, inspired both of our viewpoints. For example, the paper Tyler mentioned about declining research productivity in semiconductors, crop yields and a couple of other fields, that was done fairly close to here and that is work squarely relevant to these questions that I think is really important and we may not be here in the same way without it. On the other hand, it is simultaneously true that major swathes of these questions really are surprisingly under investigated. And so, again, to return to biomedical funding and the NIH, as far as I can tell, there are no books assessing how well the NIH is working. And I don't have a strong view on the answer to that question, but I do have a strong view on the importance of knowing. Which parts of the NIH are working better and worse? And inasmuch as the NIH has changed over the last couple of decades, was the old NIH better or the new one? Like, this stuff is so important, and so while it's the case that there's a huge amount of good research happening today with fantastic researchers, in a sense, there aren't enough of them. And a lot of the central questions are still unanswered.

Mark: Yeah, interesting. So you were talking a minute ago about the explosion in costs in healthcare. And right now, I think one of the defining aspects of the moment that we're in is a lot of the basic costs of living for a lot of people have just increased a lot. You know, the story that we tell about our society is that, okay, you have technology and you have competition and it drives down prices. So, you know, if you bought a TV today--if you bought a TV from, you know, a ten-year-old TV today would cost, you know, 5% of what it cost ten years ago. So clearly, the value and efficiency has increased a lot there. But then in things that matter so much like healthcare, education, rent--those things have generally just increased, right? And the normal dynamics that you'd be hoping would play out aren't. And to some degree, for the quality of life for a lot of people, the increases in those costs may even be dwarfing all the other advances in everything else.

Tyler: Sure. Absolutely.

Mark: So do you think that that is--that those things are all related? Or do you think--I mean, I think you used the phrase "cost disease," right, when referring to, you know, the cost explosion of things like healthcare and education, student debt, and rent. Do you think that that's a different type of problem, or do you think that is fundamentally related to the rate of progress in biomedicine, as an example? 00:40:11

Tyler: I think there are common features to these problems, though each one is different. Restrictions on entry is one, highly bureaucratized institutions. Sometimes a lot of third party payment--which may be required in the case of catastrophic healthcare, but it nonetheless has distorting effects. Areas where people have very strong moral feelings I think we often make worse decisions about. We're not analytical enough. And you put all of those together. But I would stress, say healthcare--if you go to Singapore, healthcare there, I think it's about 4% of GDP. They have slightly higher life expectancy than we do. Their system is by no means perfect. But we can see, though comparative analysis, there are ways of doing this better. The NIMBY problem, cost of living, getting an apartment. In Japan, it is mostly solved because building in Japan tends to be regulated at higher levels than the city or the county, so more gets built. Living in Japan is cheaper, the cost of renting an apartment. So often we kind of know the answers. We shy away from really focusing on a concerted effort to get to doing them in this country.

Patrick: Yeah, and agree with all of that, and I would just underscore the entry costs aspect. And the entry costs aren't always--they take different forms, right? And empirically the entry costs of forming a new university are really high, but that's not because there's a kind of formal toll you have to pay. It's not like zoning where there are deliberate, specific legal restrictions that prohibit you from doing so. But just as a practical matter sociologically, institutionally, accreditation dynamics, who knows, it's apparently almost impossibly difficult to create a successful new university today. And so I think answering the cost disease question is one of the most important set of subcomponents in this broader question of what is it that enables our progress. And at an over-acting level, it's just surprising to me that we don't have more definitive and clear answers there. Alex Tabarrok, a colleague of Tyler--

Tyler: Wrote a long paper on this.

Patrick: Exactly, last summer and there are other...papers also analyzing the question, but it's a surprisingly sparse literature. Alex's list of citations was not that long. And he had some suggestions as to what the underlying etiology might be--maybe he's right, maybe he's wrong--but again, to your point, it's one of the most pressing questions for American society, for global society in 2019. We really have to know what's happening. And to return to something Tyler said earlier, part of our hope… it's not to promote any specific solution, any specific, I don't know, aspect of it, but rather that, even though this is not what's focally central in the headlines today, it should be. As we think about what the world is gonna look like in 50 years or 100 years, it, plausibly more than anything else, is going to determine the shape of that.

Tyler: As an entrepreneur, what is it you find most striking about America's dysfunctional economic sectors? Because you intersect with them all the time, right?

Mark: Yeah, I mean, I would wanna see this get studied more, but...so there are just so many different factors, and I think part of it what is a little bit confusing is that the things that are making healthcare so expensive--they may have some fundamental link to the things that make college tuition so expensive. But on its surface it seems like there are also more proximate causes that are quite different. So I mean, with college tuition, the fact that, okay, it's really expensive, so then we do more to subsidize the cost of it, and then by doing so, we're not providing any pressure on colleges to make it more efficient, and then the cost just goes up further is a pretty different dynamic than what's going on with healthcare where basically Americans wanna know that if someone in their family gets sick, they're going to be able to get every treatment possible, which ends up-- You know, I'm sure you've seen all the stats on this, that, you know, half of the healthcare costs that someone incurs during the last six months of their life--And that's, I guess, part of what you're saying is an American moral value, which is that, you know, we believe that you should do everything you can to help someone who's sick, whereas in a lot of other countries--I don't know what Singapore's situation is--but a lot of the ones that are often cited as more efficient healthcare systems don't have that approach. They say, okay, okay, if someone in your family has this form of cancer, we'll do these two treatments, and then we're done. And, you know, part of that is because they may not be able to incur the level of debt as a country that U.S. can, so they may just have to make that tradeoff. 00:45:19 But it creates all these downstream dynamics where, okay, now if you as a society are willing to say, okay, we're gonna have two treatments for this kind of cancer and not try all seven things, then now, you know, France can go, for example, negotiate with the drug companies and say, all right, I'm only gonna support the two that are the most cost-effective, and the other ones are out to dry, whereas the American system, you know, you don't have that kind of negotiating leverage. So it seems like they're very different things. But I kind of-- Intuitively, it seems like at their root, there should be some commonalities. And I would be very interested to kind of understand that in more detail. I'm curious why, you know, from what you're saying about--that the literature is sparse--

Patrick: On cost disease in particular.

Mark: Yeah, why do you think more people aren't studying this? I mean given that this is just such a central thing in the lives of most people, right, I mean, the cost of living in the city has gone up so much. We have a whole generation of students--I think the total student debt is now almost $2 trillion, right? I think it was 1.7 the last stat that I saw. And, of course, healthcare is--is just, you know, the number of people in the country who are within, you know, one issue of being bankrupt is just kind of staggering. So-- [overlapping chatter]

Mark: What's preventing people from studying this?

Tyler: I wouldn't say anything's preventing them. The incentive is to build a brick and to build a brick that can survive scrutiny by referees. The incentive is not to build a building, in most cases. Biomedicine actually is often different. But in the social sciences, so, there's so many bricks out there and so people wanna say, oh, we're already studying this. It's correct, the bricks are there in the millions. But the bricks and the buildings are a different thing. But I have a question for you, if I may be allowed.

Mark: Go for it.

Tyler: What is it you would most like to see from academics? And I don't mean research on social media. I mean America, the world. What do you want?

Mark: Although I would like more research on social media.

Tyler: Absolutely. That's fine. 00:47:19

Mark: No, look, I think these issues on exploding costs and why the systems aren't working the way that they're supposed to for people is probably one of the most pressing questions. And when I think about, you know, our work over the next decade, and it's like what are we gonna do that's gonna fundamentally make people's lives better? There's a lot that we can do. But if these problems continue at the rate that they're going at, it's actually quite hard for me to imagine how we could do enough good to overcome the increase in costs that people are incurring at things that are so fundamental. So, you know, we're working on them in somewhat different ways. But I think healthcare is difficult because it is so inherently political because it touches on moral values. If you wanna have a difference in approach of how we treat the last six months of people's lives, that's something that's more of a democratic question than a technocratic one, I think. People need to be able to support that. So I don't personally feel like that's an area that I'm gonna have a huge impact. A lot of people are focused on that. But the area that I do think we can make a big impact is on long-term science research. So if you can just make it more efficient to cure, prevent, or manage diseases, then that over the long-term should really be the answer for bringing healthcare costs in line, not in the next ten years, but maybe in over the next 50 years. I'd like to see a solution before that, so I'd love to see more studying of the healthcare part of this. But on the science side, I'm quite optimistic about that. On housing, I don't know. You know, there's always the question of what--which forces in technology end up being stronger than--it's like which trends end up being stronger? So, you know, on the one have, you have this giant mismatch of opportunity where people feel compelled to move to cities because that's kind of where a lot of the jobs are. But then there's not enough building of supply of housing, so rent just increases. And then that means that even though people are going and doing higher-value things, their lives actually aren't benefitting as much from that because so much of their costs are just of the value that they're generating is just going to housing because rent is getting so high. 00:49:43 So historically, what have people done? I mean, we invented cars, right, and freeways. That way people could live further out. Maybe something like the hyperloop could extend suburbs like five times as far, so that could make it so someone could live quite further away. And that would be good, right, if you can increase the effective radius of a city--that's one way to alleviate constraints, political constraints or concerns about people building things, so that way you can get more supply, bring the cost down. But I happen to have a more--I happen to think a different thing is probably the right solution. You know, in 2019, it's a lot easier to move bits around than it is atoms. So rather than people moving--inventing a new hyperloop or cars, I tend to think the set of technologies around--whether it's augmented reality or virtual reality or video presence that just lets people be where they wanna be physically and feel present with other people wherever they need to be to do their job, to connect with the people they care about--that feels to me the better long-term solution. Don't make everyone move to cities. Make it so people can choose where they wanna be and can get access to all the opportunities they want. So those are kind of--It's hard for me to imagine more important problems, at least over the next--pressing problems for the next decade. I think over the longer term, you know, potentially climate change is more of an existential issue. But in terms of people's lives today, I think the exploding costs from these areas is such a profound issue, and the trend is so out of control. 00:51:19

Patrick: Three three quick points of that. One is, I think these questions are often a little bit--Like the cost of disease question, I think one of the reasons it's difficult to study is because you have to take this very macroscopic and potentially this very microscopic view. And so say, for example in science, if it were the case that the administrative burden on scientists had increased by say two thirds over the last 40 years. I'm not saying it has and not saying that even if it has that is, in fact, the cause of any kind of slowdown. But if it had, that might be quite difficult to observe because it can come in the form of, well, it takes twice as long on average for things to be approved and the forms are longer and you're interrupted more. And so actually specifically diagnosing that causal pathways, I think, can really be quite tricky, and I think that generalizes a lot of the fields. Secondly, to your point about technology solving the agglomeration imperative of cities, I think that could be true, although, you know, here we are in person.

Tyler: Others are watching--

Mark: Yeah, the people are watching wherever else. They're past that.

Patrick: Very fair. But even if technology solved that, I guess my worry would be that the socio-institutional dynamics that have kind of ruined cities or made them less effective or whatever, and probably also generalized and applied to other domains, and so we're gonna suffer the costs of those same phenomena elsewhere.

Mark: Oh, yeah.

Tyler: And what do you wanna ask Mark?

Patrick: Hmm. What have you learned from doing CZI? In that how--I mean, you launched it five years ago?

Mark: Four years ago.

Patrick: Okay, yeah. How will the next four years be different to the first four?

Mark: Well, so one of the things that we struggle with here is...this is such a long-term project, right? So we talked a lot about the scientific research. We're also doing a bunch of work with education to build tools for teachers to do more project-based learning, more personalized learning for kids. But basically make it so that teachers have tools to do the work that they wanna do--mentor students and not just have to lecture and have everyone learn at the same pace. So this stuff--we're making progress in all of these areas. And I think one of the meta questions in running CZI is at what point to check in and consider evolving the direction. I mean, obviously, there's minor execution things that you try to improve along the way. But...but I wanna make sure that we have an awareness that these are fundamentally problems that we're gonna be working on for 10 or 20 years and not--I think a lot of these things just kind of a consistency of approach, and building trust is kind of, you know, more important than constantly evaluating or potentially thrashing. In science, we've had the benefit of taking on a number of different projects. So the Human Cell Atlas was one of the original ones. Now one of the next areas that we're really excited to work on is imaging. There's a lot of advances in microscopy, but there are a lot of things that we still can't see. And as engineers, I think one of the things that you can probably appreciate is, you know, just...when you're trying to debug a system, you really wanna, like, get into the code and see, step through it and see where the thing is breaking down. But, you know, we don't really have a way today to see a white blood cell eat a...you know, a virus, right? Like in vivo, right? In the body. To see proteins folding live. And I think that, you know, there are certain optical levels--optical thresholds on the physics that you might not be able to get beyond, but between that and the advances in AI, I do think that it's possible to give scientists new imaging capacity that hasn't been possible before. 00:55:33 So a lot of what we're trying to do is--All right, so the Human Cell Atlas, we took an approach, it was kind of very broad and collaborative and somewhat chaotic, even, in a way. And I think we were able to learn some of the lessons from that as we're not thinking about how we organize the imaging project about, okay, maybe it would be helpful to have more clearly established leadership around it, up front. You know, maybe there are things that rather than having just one big project, there're gonna be areas where we can just build tools that go into every lab. There's one software package called [indistinct] that, you know, a lot of scientists--It's like--like right now, there's the actual technology of microscopes is kind of...ahead of scientists' ability to process the data. There's this weird mismatch because--it kinda makes sense. You know, the NIH funding supports people to basically have a lab.

Patrick: Yeah, tool building is not really subsidized or supported that well.

Mark: Yeah, but I mean, if you want to have a team of ongoing software engineers, that's like, okay, you're gonna want an effort that's going on for a while, that's more than a couple of people. So that kind of thing, I think, there's a real niche that no one is doing that stuff at the scale it needs to get done. So just pushing on both of these--

Patrick: Yeah, and there's uniform agreement on that particular point with every biomedical scientist that I speak with. Like, tool-building is under-supported.

Mark: Yeah, so I don't know. From a meta point I'm...I'm a little wary of concluding whether--that things have--like which things have worked and not worked well yet. I mean, certainly not everything we're gonna do is gonna work--

Patrick: Four years--it's too early to say.

Mark: Yeah, but, like...but it's certainly interesting. And what I try to push the teams to do is make sure that the work that we're doing are things that clearly would not have happened otherwise. But I think, especially in a lot of these fields--in philanthropy, I think there are a lot of potential issues where it's easy to...to give money to something and feel like you're doing good because you probably are doing some good but lack the discipline to say, okay, am I doing the most good that I can? And I think we kind of have a responsibility to do that. So that's the thing I push our team to do is develop really different theories. I'm quite confident that an education, the work that we're doing, is just stuff that, if we weren't trying it, it's not clear that anyone else would be doing an effort like this at scale. I feel really good about that. I think in imaging, something like that is gonna be similar. Even in social advocacy, we're doing a lot of work in criminal justice reform that's, you know, a combination of advocacy and building tools for accountability and working with reform-minded prosecutors, that they can be more data-driven about who they try to bring charges against. Because they wanna be fair, you know, or at least a lot of folks wanna be fair, and they don't have the data to either optimize how they run their office or to hold the people in there accountable, so building those kinds of tools can be super helpful. And I'm quite confident that if we weren't pushing on that, I'm not--I feel good. That's like a good theory to at least try to push on. So that's what we try to do in the work. 00:58:54

Tyler: So like the criminal justice work, education, biomedical--what's the underlying view or insider experience of yours that's the common element behind those areas? Like, how do we boil down Mark Zuckerberg philanthropy to a smaller number of dimensions?

Mark: Well, first of all, it's not just me. I do it with my wife.

Tyler: I'm sorry, my apologies.

Mark: No, well, she's an important element to this because she was a teacher. She is a teacher. She's building a school. I mean, she spends a lot of time over there. She is a doctor, so if you're looking at the education in health aspects, the domain expertise is more hers than mine. And she is quite, I think, compelling and insightful on some of the things that need to get done there. In terms of the approach, that may be more inspired by me in some ways where--where, you know, it's the very long-term focus which I think it comes from a lot of the lessons I've learned from Facebook. It's the tool building which comes from having the experience building engineering teams. And it's some of the--some of what we've learned just in kind of managing and partnering with folks through building the company is that it's a lot of what you said. It's like you wanna bet on the best individuals in different spaces and give them room to run. In managing complex projects, you need to know when something needs to be a little more directive versus when you want it to just be an open thing that can make progress in a more chaotic way. And that might be more art than science--or at least until your field gets--fully solves all these questions. But, yeah, it's...I don't know, it's an interesting set of questions, and certainly the...you know, I guess one animating theme certainly is, you know, as our kids grow up, we want to make sure that they live better lives. 01:00:50 So these aren't things that are primarily gonna benefit us, right? If we were trying to benefit us, we wouldn't be working on education. I think the health work is very long term oriented. If we were focused on kind of our own health, you know, you'd be probably be doing more disease-specific work rather than fundamental science to try to--or tool-building for fundamental science, which might even be a level more abstract that fundamental science to try to compound the rate of progress in science. And then a lot of work on equality. You know, the criminal justice work, I think, is--I mean, a lot of the way that our country handles this stuff is just such an unfortunate outlier compared to other countries and the amount of human capital that is locked away is...is its own thing that I think deserves a lot more than studying. But I mean, certainly I think just improving that would be...a big advance. But I don't know, it's interesting. This conversation is interesting because I think it highlights somewhat of a distinction in...I guess my approach to learning or studying these things is more the "try different things and experiment" and then play it forward, generate new data doesn't exist and see how that goes. And, you know, talking to you and seeing the work that you do, and I guess this is probably intrinsic to being an academic too, where more of the work is about, you know, looking at datasets that can exist and studying what is already there rather than trying to kind of create the new datasets or approaches. I mean, there are two approaches that I think complement each other but are actually quite different in terms of how you kind of approach learning about how to do the best work going forward. 01:02:46

Patrick: Well, I think there is a very important complementarity where for any of these really important questions about how should science be organized or which kinds of policies generate the most economic growth, or how one should support the diffusion of innovation or whatever--I don't think there exists definitive data on that question. I don't think by just going deep into the literature you're gonna come up with clear answers that one can feel confident in going and executing it or implementing. I think of the data, such that it exists, and the exiting findings as food for hypothesis-generation. For example, to return to the management training, that's one. But I would probably not have guessed the effect sizes would be that large, right? And so if those studies hadn't been conducted, I don't think I would have...ascribed particular, you know...sufficient expectation value to the effort of maybe, Stripe going and doing something better. But now because of those studies, I think, well, perhaps there are, on the margins, things we could do. Maybe there are things that end up being quite materially valuable over time. And so I think being able to marshal those, you know, potentially being able to encourage people to dig more in particular directions and then to combine that with a willingness to experiment and a willingness to frankly, just be wrong. And I think the synthesis of that is really powerful. And again, if you go back and you look at the foundations that I think have really had significant impact over the past 100, 200 years, I think it's that kind of combination in that if you look at, like...Warren Weaver, who was the guy at Rockefeller who funded Norman Borlaug, right? He'd worked with Vannevar Bush at OSRD during World War II. 01:04:54 I think he'd--he was familiar with a lot of the data and just empirical realities of how different kinds of scientific and technological ventures were likely to work. But he was also willing to just place a bold bet and that pursue the hypothesis that agronomy could be radically improved. But there was no particularly strong basis to truly have conviction of that. And so I think it's all in the combination.

Mark: Yeah, interesting. So I'm curious to push further on one question. I mean, you asked me what I would want, would it be studying? Why don't you think people are studying the, um...the cost questions as much as...as it seems like they should be? Or it seems like--if these are as big a questions for society, and it certainly seems like they're issues that most people have--what are the structural barriers that are preventing the top people in these fields from deciding to go study it? Is it that the fields don't line up with it? Is it that there's not funding for it? Is it too hard in certain ways? Like, what are the dynamics that are going on here?

Tyler: There are many big questions. It's hard to study them. So at the end, you have quite a speculative answer or set of hypothesis. So the world as a whole isn't sure what to make of that. Is it a real contribution? So the private return to you as a researcher may be as unclear. So you tend to get very famous people who are quite well established looking at really big ideas maybe a bit later in their career. And I'm not saying that's bad work, but it's not necessarily cutting-edge either. 01:06:37 And they spent their whole lives being famous, and they're not necessarily in a position to actually make the breakthrough. And then younger people, their incentive is to first get established and do something that is quite defensible. So I think in general, big questions are under-studied-- the tenure system, I think, increasingly is broken. A lot of academics do work pretty hard, but that so much of your audience is a narrowly defined set of peers who write you reference and tenure letters--I think we need to change. And the incentive for academics to integrate with practitioners and learn from them and actually try doing things--we need more of that. I've often suggested for graduate school, instead of taking a class, everyone should be sent to a not-so-highincome village for two weeks. They can do whatever they want. Just go for two weeks, think about things. No one wants to do this. No one wants to experiment with it. People who do development often do it on their own. But the notion that every economist should have studied the East Asian economic miracle, the Industrial Revolution, and spent two weeks or more in a poor village--it's just not how things are, and I'd like to change that.

Mark: So how does one go about changing that? So if you're trying to create a network of people who feel like they have an incentive to study this because it's gonna be good for their career, right, and it's not--they have a network of supportive people who might be reviewing the grants or the work that they're doing and also think that this is important work to be done--how do you go about establishing that?

Tyler: I can selfishly say that at George Mason, virtually all of our students have very directly studied these questions, and we funded a lot of them to go live other distant, strange, possibly poor places. Other departments may have more money than we do. It can be done, because we've done it at George Mason. So I think again it's a question of the will and just the ability and desire to imagine that things could be quite different in a sense that I think was more common in the America, say, of 1958 or JFK's decision to put a man on the moon than you see actually in 2019. 01:08:38

Mark: All right, is that a good place to wrap?

Tyler: Fine by me.

Mark: All right. Well, thank you, guys. This has been a great conversation.

Patrick: Thank you.

Tyler: Thank you, Mark, Patrick.

Mark: All right.